THURSDAY, MARCH 4, 1886

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873-76 *under the Command of Capt. G. S. Nares, R.N., F.R.S., and Capt. F. T. Thomson, R.N.* Prepared under the Superintendence of the late Sir C. Wyville Thomson, F.R.S., &c., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vols. XI., XII., and XIII. By N. Poléjaeff, M.A., P. Herbert Carpenter, D.Sc., Frank E. Beddard, M.A., Prof. William C. McIntosh, Edgar A. Smith, Dr. E. Selenka, and Prof. G. O. Sars. (Published by Order of Her Majesty's Government, 1885.)

DURING 1885 three new volumes of the Zoological Series of Reports have been published. Of these, Part 32 of Vol. XI., "On the Stalked Crinoidea," by Dr. P. Herbert Carpenter, has already been noticed in our pages (NATURE, vol. xxxi. p. 573). The others we now proceed to notice.

Part 31 is a "Report on the Keratosa," by N. Poléjaeff, M.A., of the University of Odessa. The Keratose sponges do not belong to the deep-sea fauna. It is therefore not to be wondered at that the total number of species collected during the cruise of the *Challenger* should have been only 37. It is, however, a little surprising that of this number 21 should be new. The collection embraced forms belonging to almost all the genera of the Keratine sponges hitherto distinguished, and the specimens were for the most part well preserved.

The Report opens with a chapter on the organisation and classification of the group. The subject of the classification of the group is undeniably a difficult one. In no section of the animal kingdom is there a greater danger of describing individuals instead of genera and species. The student has no palæontological data to refer to; embryological details so far as these are known do not help him much; minute anatomy gives but few distinctive characters, and so he is obliged to depend on general anatomical details. When the author acknowledges, as he freely does, that this is so, one is not surprised to find the writers of the past—Duchassaing and Michelotti, Gray, Hyatt, and Carter—depending for their divisions on the properties of the skeleton; nor does one wonder that in their attempts they so often went astray. The division of the Keratosa into two groups, differentiated by having homo- and heterogeneous skeletal fibres, is characterised as thoroughly artificial. The subject of the presence of filaments is capable of no systematic application (the extremely interesting question of what these filaments are is discussed at length, no definite conclusion being come to; it is strange that they never seem to have been examined by a botanist). The presence of true cells in the walls of the skeletal fibres cannot at present be defined as of systematic value. Dr. Vosmäer's arrangement of a division into families, characterised by the properties of both the skeleton mass and of the soft parts is selected as the best possible for the present.

The history of these families and of the various genera placed therein is written with the greatest care and fairness. In agreeing with Hyatt (1875) that Ceratella and Dehitella of Gray are thoroughly sponge-like forms, and not, as Carter (1873) would have them, "nothing but hydroids or coral-like skeletons," he overlooks the fact that in the *Quarterly Journal of Microscopical Science* for January 1870 it is stated that the Ceratellidæ were undoubtedly a "family of arborescent Keratose sponges." The descriptions of the species are accompanied by ten plates of figures. This able Report concludes with a few pages on the subject of the affinities of the group.

Mr. Herbert Carpenter's "Report on the Crinoidea" forms Part 32, and is followed by a "Report on the Isopoda" (part 1), by Mr. Frank E. Beddard, forming Part 33 of the series. This portion of Mr. Beddard's Report relates to the genus Serolis, which occupies a foremost place among the Isopods collected. Of the 16 species collected nine are new. A discussion of the systematic position of the genus within the order Isopoda is postponed until the next Part of the Report, but with regard to the alleged affinity of the genus and of the Isopods generally to the extinct Trilobites, as insisted on by A. Milne-Edwards, the author has nothing to add to what has already been said; the examination of the species found during the *Challenger* Expedition having brought to light no facts which tend to show any close resemblance between the two groups. Of the 22 known species all but four are found at a depth of from 5 to 150 fathoms. Of the four deep-sea forms one is found at a depth of 675 fathoms, a second at depths between 400 and 1600 fathoms, a third between 400 and 1975 fathoms, and a fourth at depths of 600 and 2040 fathoms. In the two species from the latter depth the genus attains to its greatest size. It has evidently had its origin in the Southern Hemisphere, probably around the shores of the south polar continent. While the great majority of the species live in shallow water, the deep-sea forms are in all cases strongly marked; they also show certain peculiarities, notably in the structures of their eyes, which are often absent, but, when present, show great evidence of functional degeneration; indeed none of the deep-sea species possess well-developed eyes. The eye-structure of some of the species is given in great detail and is well illustrated on Plates IX. and X. Ten plates accompany this memoir.

Part 34 of the Zoological Series forms Vol. XII., a portly volume of over 550 pages illustrated by 93 plates. This valuable Report is by Prof. William C. McIntosh. It is on the Annelida Polychæta, and marks quite an era in the history of this group. In a short notice it is impossible to do justice to this laborious work, and we must content ourselves with briefly marking our admiration of the care and research that have been bestowed upon it. Of the species collected no less than 220 are described as new. It is noteworthy that the formation of no new family was required; all the forms fall into groups already constituted, and which have been so satisfactorily diagnosed by Malmgren that the diagnoses have not here been repeated, but a most useful synopsis of the families, genera, and species described is appended to the Report, with references both to the pages and plates. In many cases the food of the Annelids has been examined, and in the case of abyssal forms, it throws some light on the food-resources of the great depths of the oceans. In the North Atlantic Region a large number of forms occur, and relatively few range to other areas, but this apparent distinctness in so vast a region is probably due to the comparatively unexplored condition both of it and the other oceans. Most of the genera are cosmopolitan in their range, but the remarkable new genus Buskiella is entirely confined to the abysses (2025 fathoms) of this and the South Atlantic. In the South Atlantic Region the two chief centres for specimens were the coast of Brazil and the Cape. In the South Indian Region one of the most striking features was the large proportion of species pertaining to Kerguelen. The abundance of Annelids in the deep water of the land-locked bays of this island was remarkable, and many new forms are described therefrom. In the Australian Region the types found were in many cases peculiar and novel; here the branched Syllis, one of the most remarkable discoveries of the Expedition, was found. In the Japanese Region a series of remarkable types were found, while comparatively few came from the North Pacific, and the majority of the specimens from the South Pacific came from the confined waters of the Straits of Magellan.

In regard to bathymetrical distribution, the greatest number of species occurred in the shallow water, 10 fathoms and under. The two regions ranging from 10 to 50 and from 50 to 100 fathoms have each about the same number of Annelids, and both are similar in respect to new forms. In the depth between 100 and 200 fathoms the number was less, but the proportion of new forms was much higher; while in depths between 200 and 500 fathoms almost all the forms were new, and many belonged to new and remarkable genera; between 500 and 600 fathoms the number fell to less than half that in the previous group, but the majority were new. The number found between 600 and 1000 fathoms include two known species out of a list of 14. The four species occurring between 1000 and 1200 fathoms are new. Those species found between 1200 and 1500 fathoms are more than five times as numerous as the last, and include only five known forms, most of which, however, are found in shallow water as well as at this great depth; between 1500 and 2000 fathoms all the species were new. The same is true of those between 2000 and 2500 fathoms; while in the lowest depths, between 2500 and 3000 fathoms several known forms occurred. The majority of the deep-sea forms are tube-dwellers.

Vol. XIII. opens with Part 35, Mr. Edgar A. Smith's "Report on the Lamellibranchiata." On the return of the Expedition all the Mollusca were placed in the hands of the Rev. R. Boog Watson for description, but after separating out the different species, and labelling the greater part of the known forms, Mr. Watson, seeing the immense extent of the collection, determined to limit his descriptive work to the Gasteropoda and Schaphopoda, and Mr. Edgar A. Smith then consented to prepare the "Report on the Bivalves." The author apologises for not using the name Pelecypoda for this group, urging that it has not only priority but also is more in conformity with the names in use for the other classes. When in 1824 De Blainville first used the term Lamellibranchs, though it is true the class for which it was used was not characterised, still the genera placed under it were so well-known, that the name itself may be said to carry its own explanation, and this might fairly secure the priority for a well-known and almost universally accepted name, which in part by accident it would seem is used in this important Report.

In many respects the collection of Lamellibranchs was disappointing. Only some 500 species were obtained, and of these five were represented by a long series of specimens, and in many cases the species were represented by only detached or single valves. When great depths were reached some of the forms found were of particular interest, but it is a remarkable fact that only one distinctly new generic type was discovered. The greatest depth at which Lamellibranchs were found was in the mid North Pacific Ocean at 2900 fathoms, but two species, *Semele* (*Abra*) *profundorum*, n.sp., and *Callocardia pacifica*, n.sp., were found. Some of the species are noted as found not only at widely distant localities but also at very different depths. The Lamellibranch fauna of the deepest parts of the Atlantic and Pacific Oceans is not of a very extraordinary and certainly not of a special character, and it would appear clear that the deeper the dredgings the fewer the species found. The memoir is illustrated by 25 plates, executed in a very creditable manner by the Cambridge Scientific Instrument Company.

Part 36 is a "Report on the Gephyrea," by Dr. E. Selenka, the well-known Professor of Zoology in the University of Erlangen. The number of species (28) collected was small, and they belonged to known genera; 10 were undescribed. The habitat and distribution of some are of special interest; forms previously known as littoral have been dredged from great depths; it would appear probable that the tube-inhabiting Gephyrea occur especially at the greater depths, where as yet has been found only a single example of the free-living forms. Four plates illustrate this Report, on one of which the strange male of *Bonellia viridis* is figured, with its till now overlooked curious segmental organs.

Part 37 is a "Report on the Schizopoda," by Prof. G. O. Sars, of Copenhagen. The collection made turned out extremely rich and of very special interest, containing several most remarkable new types, and greatly increasing our knowledge of the morphology and affinities of the group. In an introductory note the subject of terminology is dwelt on; in a note on the morphology of the group the author decides for the present to "assign to this group the rank of a distinct tribe or sub-order of Decapoda." This sub-order occupies as it were the most primitive position within the division of the Podophthalmia, containing apparently the least modified forms, in which the original characters distinguishing the progenitors of the whole division would seem to exhibit least change.

In an appendix to the Report some interesting details are given of some ecto- and endo-parasites found in the Schizopods; 38 plates, drawn by the author with the aid of the camera lucida, represent all the new species; the drawings are very highly finished, and have been clearly and beautifully printed. It may be noted that the new genera and species described in the Report were briefly characterised by the author in the *Transactions* of the Christiania Scientific Society for 1883.

We have pleasure in again noticing that these splendid contributions to zoological knowledge have been edited and seen through the press with wonderful expedition and accuracy by Mr. John Murray, whose labours as Editor now seem coming to a close.

THE GERMAN NAVAL OBSERVATORY

Aus dem Archiv der Deutschen Seewarte. VI. Jahrgang, 1883. Herausgegeben von der Direcktion der Seewarte. (Hamburg, 1885.)

THIS, the sixth yearly report of the German Naval Observatory at Hamburg, of which Dr. Neumayer is the Director, contains much valuable information as to the increasing and successful application of scientific methods and results to the safe navigation of the German Imperial and Mercantile Navies, in addition to the usual details as to the *personnel* and working of the whole establishment.

The volume contains four papers, but the first is that in which the most general interest is likely to be taken, the other three referring to special reports on subjects connected with one or other of the four departments into which the work of the Observatory is divided.

Commencing therefore with the first paper, there will be found a general report showing much activity in the collating and distribution of information on the important subjects of meteorology, magnetism, and geography, and describing the arrangements for making the observations in the head Observatory at Hamburg, and the affiliated stations on the coast. It may also be noticed that one of the principal additions to the Observatory in 1883 was an instrument for the systematic observation of refraction, but the description is deferred to another report.

Following the general report is an account of the work accomplished in each of the four departments before mentioned.

Department I. is devoted principally to maritime meteorology, and it will be found that the system adopted ollows closely on the lines of our own Meteorological Office in the collection of observations on the coast and at sea, and publication of results.

In Department II. the work resembles in some particulars that which is now making Kew Observatory such a valuable aid to the nautical world in the testing of sextants, barometers, and thermometers. At the Hamburg Observatory, however, they also prove compasses, compensating magnets, log-glasses, and position lanterns for ships' use.

It may be remarked in passing that the production of a novel form of compass by Sir William Thomson in 1876, and the full explanation by him of the principles involved in its construction in several lectures, accompanied by the subsequent success of that instrument in its later form, has had a world-wide influence in modifying the previously conceived ideas of the best form of compass for navigational purposes. For example, at p. 32 of the first paper there will be found the announcement of a new compass constructed in 1882 under the supervision of the Director of the Observatory in which all the principles of the Thomson compass have been carefully retained, but with certain changes securing greater strength in the compass card. These changes were introduced in view of the consideration that the Thomson card was too fragile to stand the rough handling it might be subjected to on board ship. This new compass has been patented in Germany, and after considerable trial in their mercantile marine has proved successful.

But this department has also turned its attention to the instruction of officers in the magnetism of iron ships by approved teachers of navigation, and at pp. 32 and 33 information is given as to the number of ships swung for deviation of the compass by the officials of the Observatory, and their compass arrangements scientifically treated, also of the large number of ships' compass journals sent to the Observatory for discussion. When the journals have been examined, instructions for the future guidance of the captains of the several ships are given as to the probable deviations of their compasses in the ensuing voyage.

As a useful aid to this scientific examination of ship's compasses, the terrestrial magnetic elements with the annual change are given for certain ports on the German coast.

In Department III. the important work relating to weather forecasts, coast meteorology, and storm warnings is carried on, and tables are given showing the number of days on which forecasts were given for inland and the coast, and of the degree of accuracy attained.

In 1883 the first attempt in establishing a limited night service for issuing storm warnings at night was commenced, a lantern showing a red light being hoisted as the signal.

Department IV. conducts all matters relating to the trial of chronometers, and an account is given of the trial of several descriptions of those valuable instruments, as well as of an apparatus for simulating the action of a ship in a seaway upon them, the effect of temperature being observed at the same time.

Six chronometers of the German Navy have been tried in this apparatus, but the results are not reported. It may be a matter of curiosity to hear how far this apparatus has been a success, but long and varied experience in England has shown that if the rate of a chronometer due to the elements of time and temperature are properly ascertained and furnished to the seaman, he will soon find out the effects of the ever-varying motion of his ship at sea with a precision which an apparatus on shore is not likely to attain in advance.

At p. 43 some useful information is given respecting the scientific work carried on independently by the several departments of the Observatory.

Paper No. 2.—This has been written as a guide for popular instruction in the nature of the deviations of the compass in iron ships by means of a model. A woodcut of the model is given, and the results of some twenty-seven experiments recorded. Models of a similar kind are in use in England and America, and are found very useful in imparting practical information concerning the causes and correction of the deviations of ships' compasses.

In Paper No. 3 there is a discussion of a series of observations of the magnetic declination in Barth, made during the years 1881-2-3-4, a period of time which should render them interesting to magneticians.

The fourth and last paper consists of a special report on the trials of marine chronometers sent in by different makers, and of varied construction, during the three years 1880-83. The results of these trials are here mathematically discussed, and chiefly by the use of M. Villarceau's formula for rate, in which the rate g of a chronometer is considered to be a function of the two independent variables—t, the time, and θ, the temperature. The tables of rates recorded resemble very closely those published by the Greenwich Observatory, with the exceptions that, instead of weekly sums of the rates being given, the sums are given for every ten days, and the Centigrade thermometer is used. The chronometers are also kept in a constant temperature for each ten days of the period of trial, commencing with 15° C., then with the temperature raised 5° for each decade until the maximum, 30°, is reached. The trials are continued in the same manner with decreasing temperatures until 5° is reached, and lastly with temperatures increasing to 30° as before.

From what has been said it will be seen that at the Naval Observatory in Hamburg a wide range of subjects is taken under its supervision, and it may be added that the volume now under review is a full and able exponent of its work and aims.

VARIATION IN DOMESTICATED ANIMALS

Fancy Pigeons. By J. G. Lyell.
Poultry for Prizes and Profit. By J. Long.
Book of the Goat. By H. Holmes Pegler.
British Cage-Birds. By R. L. Wallace. (London: L. Upcott Gill, 1885.)

WE have grouped the above-named works together inasmuch as they all treat of the varieties existing in domesticated animals, and are moreover serials in course of publication by the same publisher.

Previous to the issue of Darwin's great work on "Variation in Plants and Animals," the subject was treated with undisguised contempt by biologists generally, and thought to be worthy of consideration only by florists and fanciers, not even its importance in reference to the food supply of man being properly estimated. The origin of this opinion was no doubt correctly given by the late Dr. Gray, when, in reply to the question put to him by the writer of this notice, "Why naturalists ignored the existence of varieties, a variation, however abnormal or monstrous it might appear, being as real as the most normal species," he answered. "The reason, my dear sir, is that they know nothing at all about them."

Nor is this ignorance extinct at the present day. In the Museum of the College of Surgeons may be seen the skull of a crested fowl, with the peculiar bony growth supporting the crest, and the accompanying hour-glass-shaped cavity of the cranium, which are characteristic of the entire race, described as the result of disease in the catalogue compiled by Sir Richard Owen. Nor need we go further than our own unrivalled zoological vivarium to see specimens which every breeder of domestic animals believes to be mere varieties, such as woolly cheetahs and black-shouldered peacocks, exhibited as "good species."

How many naturalists even now care to ascertain what are the limits of variation in any given species, or to what extent the characteristics of allied animals or groups of animals may be reproduced by what Darwin termed "analogous variation."

By careful selection, aided by great practical experience, the skilled breeder can produce almost any pattern of plumage or any disposition of colour he pleases, limited only by the range of colours and markings natural to other animals of the family to which the species belongs on which he is experimenting Thus all the markings of the wild Felidæ can be reproduced in the domestic cat; those of the Columbidæ in the pigeon; but the feline markings cannot be produced in the dog, nor the distribution of colour seen in the Australian pigeons be implanted in the domestic fowl.

It unfortunately happens that the peculiar bent of mind which makes a man a "good fancier" does not necessarily tend to constitute a good naturalist, and it is rare for the two pursuits to be combined in the same person; the zoologist despising the fancier and his monstrosities, which are the result of artificial selection, and the fancier, on the other hand, if he has even a slight acquaintance with zoology, laughing at the naturalist who manufactures what he calls "good species" out of a slight variation of plumage, which he, the fancier, would breed to order without the slightest difficulty. For examples of this proceeding, we need only turn to recently manufactured species of the genus Phasianus.

To those ornithologists who would wish to note the almost infinite variety of pattern, colouration, and marking to which the descendants of *Columba livia* can be bred, we would recommend the "Fancy Pigeons" of Mr. Lyell; it contains a more detailed and fuller account of the numerous breeds than any book in the language, although some of the theories of the writer will not meet with the support of ornithologists. The work, moreover, has a sufficient number of engravings, both coloured and plain, to render the descriptions easy to follow; and these are not, as is too often the case in works written by a fancier for fanciers, grossly exaggerated.

"Poultry for Prizes and Profit" treats, as far as it has proceeded, of the characteristics of the various breeds of fowls, of which, as of pigeons, new varieties are being constantly produced. Of the manner in which fanciers confound species and varieties a strong example is given, the author describing the very distinct and strongly characterised species, *Gallus furcatus*, as a variety of the domestic fowl.

The "Book of the Goat" contains a very good description of the various breeds of goats found in different parts of the world, and most valuable and practical directions for the management of the animal in a domestic state as a useful milk-producer.

"British Cage-Birds," the last of the serials on our list, deals more with wild species than with varieties. It gives the mode of capture, treatment in captivity, &c., of British birds that are kept in confinement for the sake of their song or beauty of plumage. This work is also largely illustrated, but the engravings in many instances are capable of improvement.

W. B. TEGETMEIER

OUR BOOK SHELF

Differential and Integral Calculus, with Applications. By A. G. Greenhill, M.A. Pp. xi. and 272. (London: Macmillan and Co., 1886.)

WITHIN the limits of 267 pages it is not easy to make improvement in so vast a subject as that of this treatise. The chief novelty is the concurrent treatment of differential and integral calculus. A great step in perspicuity has been made by the use of the *complete* notation of hyperbolic trigonometry (sinh, cosh, &c., and $\sinh^{-1}$, $\cosh^{-1}$, &c.), which shows the perfect analogy of the circular and hyperbolic functions in both differentiation and integration. The gain is for mathematicians; its use to practical men may be doubted, as the numerical calculation of these functions is (at present) best done by the familiar logarithms. In the older treatises the applications were chiefly algebraic and geometric; the author's system is to introduce the student at once to a wide scope of applications in both geometry and physics, including some of the higher branches (*e.g.* central orbits, harmonic vibration, Fourrier's and Green's theorems, &c.). It is clear that the account of each must be very brief. In some cases (*e.g.* the article on "Curve-Tracing," Art. 127) it amounts to merely a sketch of procedure and results with scarcely any proof. In an "introductory" work this seems a defect. It is, however, a masterly introduction to the subject, and the wide scope of the applications is well fitted to interest the student.

It remains to notice some defects (in our judgment). About ten pages are devoted to *ordinary* trigonometric relations and tables of mere trigonometric formulæ. This seems too much space (being 4 per cent. of the whole) to such elements. No definition is given of a maximum or minimum, and the treatment of maxima and minima is made to depend wholly on geometry.

On p. 189 it is stated that Taylor's theorem is one "by means of which any function whatever can be expanded"—an obvious slip, corrected lower down (pp. 193, 201). The necessity for the subject-functions, and in many cases also their differential coefficients, being continuous and generally also finite within the limits of any question is not stated. This is, unfortunately, a not uncommon omission in elementary works. ALLAN CUNNINGHAM

Elementary Algebra. By Charles Smith, M.A., Fellow and Tutor of Sidney Sussex College, Cambridge. (London: Macmillan and Co., 1886.)

IT is a pleasure to come across an algebra-book which has manifestly not been written in order merely to prepare students to pass an examination. Not that we think Mr. Smith's book unsuitable for this purpose; indeed, with its carefully-worked examples, graduated sets of exercises, and regularly-recurring miscellaneous examination-papers, it compares favourably with the most approved "grinders'" books. The real want of the present day is a text-book logically arranged and logically written. Apparently no author cares to risk the chance of the financial ruin of his book by going thoroughly to the root of the evil. A policy of "safety" is the most we can expect. This is Mr. Smith's policy, and although we think he might have made fewer concessions to custom and yet have been safe, we welcome his effort very cordially, trusting that, when his book has gained the success which it well deserves, he will see his way to introduce further improvements. He shows to great advantage as a teacher, his style of exposition being most lucid: the average student ought to find the book easy and pleasant reading. The second set of exercises on the binomial theorem is worth specially noting; in many other mathematical books the sets of exercises proposed to the student might well be, as in this instance, *collections of really useful theorems.*

LETTERS TO THE EDITOR

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

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

The Pleomorphism of the Schizophyta

SOME students of natural history are content, when the explanations of phenomena which they have advanced and the arguments by which they have supported those explanations are appropriated by other observers, to remain silent, trusting to the justice of future generations for the vindication of their claims. So far as my own experience goes, an active observer who should trouble himself to obtain honest treatment from all his contemporaries in regard to the significance of his published writings, might abundantly employ the latter half of his life in struggling with new writers for that just recognition of his efforts in earlier years in advancing the knowledge of this or that subject, which is the one reward desired above all others by most men who have not attained to the heights of philosophic contempt for the regard and sympathy of fellow-labourers. I do not intend to largely employ my leisure in this pursuit, but there is one subject on which I am anxious once for all to establish the significance of my observations and reasonings published twelve years ago in relation to similar views advanced and accepted at this moment.

That subject is what is now spoken of as the pleomorphism of the Schizophyta or Bacteria.

The view that the genera then recently established by Cohn, viz. Micrococcus, Bacterium, Bacillus, Vibrio, Spirillum, and Leptothrix, are form-phases, or variations of growth of a number of "Protean" species of Bacteria, each of which may exhibit, according to undetermined conditions, all or some of these forms, was definitely and precisely formulated by me in my memoir on "A Peach-coloured Bacterium," published in the *Quart. Journ. of Microscop. Science* in 1873. I distinctly recognised the existence of true species of Bacteria or Schizophyta, but I pointed out that these must be characterised, not by the simple form-features used by Cohn, but by the *ensemble* of their morphological and physiological properties as exhibited in their complete life-histories. I illustrated my conception of the Protean or pleomorphic character of Bacterian species by a reference to the similar character of the species of Calcareous Sponges, and I had in my mind also the closely parallel facts established by Carpenter in relation to the endless variety of forms of the Protozoic Foraminifera.

My view was no merely speculative suggestion, but was based upon a careful study of a remarkable peach-coloured Bacterium, which exhibited a wide range of forms, connected by intermediate forms, growing together in the same vessel, and linked to one another most unmistakably by the fact that they all were coloured by a special pigment which I studied with the spectroscope, and to which I gave the name "Bacterio-purpurin." I observed this organism on many different occasions from various localities; I figured and described its various form-phases; I obtained some modifications of form by cultivation, but chiefly depended upon the association of the different forms, the presence of completely transitional forms, and the common bond of the pigment, for the view as to their nature which I put forward. I gave the name *Bacterium rubescens* to this pleomorphic, or, as I termed it, "Protean," species. I gave an account of further observations on this organism in the *Quart. Journ. Mic. Sci.*, 1876, pp. 27-40.

Cohn opposed my view as to the genetic connection of the various forms associated by me under this name, and, contrary to the established laws of nomenclature, substituted a manuscript name in one of Rabenhorst's collections (viz. "*roseo-persicina*"), for the duly-published name applied by me to this organism. He further described some of its form-phases, already figured by me, as *Monas okeni*, *Monas vinosa*, and *Rhabdomonas warmingii.*

On the other hand, two years later, Dr. Warming of Copenhagen (*Vidensk. Meddelelser. naturhist. For. i. Kjöbenhavn*, 1875), after studying the same organism and figuring many of its form-phases, adopted my view as to their nature, and the extension of that view to the Schizophyta generally. He says: "Les bactéries sont douées en réalité d'une plasticité illimitée, et je crois qu'il faudra renoncer au système de M. Cohn." In 1883 Dr. Neelsen, in his "Studien über die blaue Milch" (Cohn's *Beiträge*, vol. iii. p. 241) cites my views and their confirmation by Warming, and rightly contrasts them with the later views of Nägeli and Billroth, and with that of Lister, who conceived that certain Bacteria were developed from a filamentous fungus (*Dematium fuscisporum*). As the result of his investigation of the *Bacterium cyanogenum* of blue milk, Neelsen says: "Viel eher würde für unsern Fall der Ausspruch Lankesters zutreffend erscheinen, von dem Proteus-ähnlichen Organismus, dessen einzelne Erscheinungs-formen eine Serie von Adaptationen vorstellen."

In 1884 Prof. de Bary of Strasburg, in his "Vergleichende

Morphologie der Pilze," p. 511, says, in regard to the question of species among the different forms of Bacteria:—"There exist two views on this subject which are, at any rate in appearance, totally opposed to one another. The first is, as I think erroneously, ascribed to Cohn. . . . Cohn distinguishes merely what we have above spoken of as form genera and form-species. The other view in its most extreme form amounts to this, that all distinction of species among the Bacteria is denied, and all forms are regarded as modifications of a single species or whatever else it may be called, and these modifications can be transformed by cultivation into one another reciprocally. This view was (if we leave out of consideration older intimations of a similar nature) set up in opposition to Cohn's classification by Lankester in 1873, and by Lister; and in 1874 carried to such a length by Billroth, that he united all the forms of Schizomycetes known to him under one collective species, his *Coccobacteria septica*. It received later a support through the views which Nägeli (1877) expressed in the words, 'I have investigated during the past ten years many thousands of Bacterian forms, and I could not maintain (if I except Sarcina) that there was any need for a separation into even two specific forms.' Nägeli, however, adds that he by no means maintains that all forms belong to one single species: it were a bold thing in his opinion to express a definite conclusion in a matter in which morphological observation and physiological experiment leave the investigator so much in the lurch. He expresses himself again in the same sense in 1882. He nevertheless is, when carefully considered, in agreement with Cohn's fundamental conception, since Cohn erected his form-genera and his form-species (the latter based on physiological properties) primarily in order to gain a provisional survey, and irrespective of the question (as he distinctly states) as to whether as thus distinguished they correspond to natural species.

"Nägeli's words above-cited contain a pregnant criticism of the whole controversy, so far as it had then gone. Both parties failed to bring forward (as is especially the case in Billroth's book) the only certain basis for their opinions, namely, the strict observation of the continuity or the non-continuity of the forms or species in question. In the absence of this, our judgment could only remain suspended, more especially since the forms in question are minute, very like to one another, often mixed together, and consequently easily to be mistaken for one another in the absence of quite strict observation. Lankester certainly came somewhat nearer towards establishing a special case of strictly-observed continuity, since the forms of his *Bacterium rubescens* (*Beggiatoa roseo-persicina*) gave evidence of their connection with one another more clearly by their characteristic coloration. Strictly-made morphological and developmental researches are now to hand. They have demonstrated that the forms known as Cocci, Rods, Threads, &c., are phases of growth (*Wuchsformen*)."

Thus writes Prof. de Bary in 1884. To some extent I have reason to thank him for the recognition which he gives to my position in this matter. But I cannot think that he has given a correct statement of my relation to the conclusion which he finally adopts when he associates me with Lister, who derived Bacteria from Fungi, with Billroth, who massed all Bacteria under one collective species, and with Nägeli, who declared that he did not see grounds for distinguishing as many as two.

The view which I put forward in 1873 is *precisely* that which Prof. de Bary now espouses, and I think I may very rightly object to its being confounded with the extreme and exploded theories of other naturalists. As to the "strict morphological and developmental researches" which now have made my doctrine of the pleomorphism of the Schizophytes acceptable to Prof. de Bary, I beg to point out that they do not differ in *character* from my own researches on *Bacterium rubescens*. Prof. de Bary very properly cites the later researches of Cienkowski, Neelsen, Hansen, and Zopf as the chief amongst those which have tended to establish that view as to the forms and species of Schizophyta which I promulgated in 1873. They have done so, *not* by affording us any stricter evidence of actual observation of change of form taking place under the observer's eye, but by multiplying cases similar (in regard to the kind of observation made) to that published by me in 1873, viz. observations of the juxtaposition and structural continuity of different forms, and of the co-existence with extremely divergent forms of abundant intermediate forms.

In relation to the attitude taken up by one of the above-named observers, I have something further to say. Dr. Zopf has made valuable researches on various Bacteria and on the Mycetozoa, and has published the best systematic account of each of these groups which has appeared. In his quarto memoir (Leipzig, 1882) on the Schizophyta, as well as in the smaller hand-book which he has since produced, Zopf gives a reference to my memoir on "A Peach-coloured Bacterium." He has himself repeated my observations on that organism, but he has entirely abstained from pointing out in the text of his work how far his observations are simply repetitions of those published eleven years previously by me (which they are almost entirely), and he has in the most exact details adopted the view as to the pleomorphism of Bacteria which I then put forward, and on precisely the same grounds, without stating that he had been anticipated by me in this respect.

Not only this, but Zopf actually goes out of his way to ascribe to me a view differing from his own, and one which I have never suggested. Either Zopf is writing about my views without having troubled himself to ascertain what they are, or he is purposely misrepresenting them, when he says ("Morphologie der Spaltpflanzen," 1882, p. v.): "Die Annahme Billroth's und Lankester's dass alle Spaltpilzformen nur Einer einzigen naturhistorischen Art oder Gattung zugehören, lässt sich nicht aufrecht erhalten."

I think Dr. Zopf will find it difficult to bring forward a citation from any writing of mine in which I have hinted, even in the remotest way, that "all the forms of Schizophyta belong to a single natural species." Billroth's declaration on this subject was published a year after my statement of the pleomorphic nature of the numerous natural species of Schizophyta, and never appeared to me to have any foundation in a general botanical experience, but to be the result of the restricted observations of a pathologist.

To remove all possibility of further misapprehension, I may be allowed to quote my own words ("A Peach-coloured Bacterium," *Quart. Journ. Mic. Sci.*, 1873, p. 410):—

"The series of forms which I have found in the growth of *Bacterium rubescens* leads me to suppose that the natural species of these plants *are* within proper limits 'Protean.' . . . The natural species among the Calcispongiæ have been shown by Haekel not to correspond at all with the series of *forms* distinguished by his predecessors. . . . It seems exceedingly probable that the same manner of regarding the Bacteria will have to be adopted, Cohn's tribes and genera taking the position of an artificial or formal system, whilst the natural species must be based upon some of those more profound characteristics which Cohn has himself indicated to us in his divisions—saprogenous, chromogenous, pathogenous. The indications of natural species do not lie under our hands in the case of the Bacteria, but have yet to be sought out."

I have now, I think, sufficiently pointed out the position of my publication on *Bacterium rubescens* in the history of the modern doctrine of the pleomorphism of the Bacteria. It will accordingly be readily understood that I cannot contentedly see this doctrine referred to, as it was recently in your columns by my friend Dr. Klein, as "Nägeli's theory of the pleomorphism of the Schizophyta," since Nägeli's view was announced four years after my publication, and is not identical with that at present accepted by De Bary, Zopf, and others, which is, in fact, *precisely* that put forward by me in 1873.

Equally objectionable as falsifying the history of knowledge by assigning to one individual the property of another is a statement in your review of Mr. Crookshanks's "Practical Bacteriology" (NATURE, February 18, p. 361). The reviewer quotes and apparently indorses a statement by Mr. Crookshank, whose book I may observe, though useful in many ways, is wanting in accuracy and in references to original sources. The passage to which I allude is as follows:—"'Researches,' writes our author," "by competent observers have quite recently clearly demonstrated that several micro-organisms in their life-cycle exhibit successively the shapes characteristic of the orders of Cohn. This had as early as 1873 been observed by Lister in a Bacterium in milk. Lister detected forms of Cocci, Bacteria, Bacilli, and Streptothrix genetically connected.' Recent observers also have obtained similar results, so that the very foundation of Cohn's classification has been shaken, and we are left without possessing a sound basis for classification into genera or species." In the original work of Mr. Crookshank (p. 110) I find the names of Cienkowski, Neelsen, Zopf, Van Tieghem, and others of my successors in this field cited, but no reference is made to the memoir published by me in 1873. Lister's observations led him to quite different conclusions, which he has since abandoned. I am sure that those who are at present busy in this country with the study of Bacteria, and who undertake to write hand-books of the subject, can have no desire to do otherwise than give a just statement of the history of knowledge of the organisms of which they treat. Hence it is with no unfriendly feeling that I offer to Mr. Crookshank and other writers similarly engaged the statement contained in this letter.

February 20 E. RAY LANKESTER

Notes on the Volcanic Phenomena of Central Madagascar

MADAGASCAR is as yet almost a *terra incognita* to the geologist; nothing, so far as I am aware, but notices of the most vague and fragmentary kind ever having appeared in regard to its geological features. Nor indeed may we expect to have other than the most general descriptions until the island is surveyed by thoroughly competent men. In the absence of something more complete, I hope that the following notes on the volcanic phenomena of Central Madagascar may not be unacceptable to your readers, and may prove a contribution, however slight, to our knowledge of the geology of this great island. And first let me mention the volcanic cones, of which there are many scores, probably hundreds, in the part of the island of which we are speaking. These volcanic cones are situated in two localities especially: in Mandridrano, on the western side of Lake Itasy, and in the neighbourhood of Betafo in Vakin' Ankaratra; the former being from 50 to 60 miles west, and the latter from 70 to 80 miles south-west, of Antananarivo, the capital. Both localities are about 130 miles from the sea on the eastern side of the island, and 150 on the western side. It is hardly necessary to say that all these volcanoes are extinct, and that there are none in activity at the present time in any part of Madagascar.[1] On the west side of Itasy the volcanic cones exist in great numbers, and these, therefore, shall be first described.

The extinct volcanoes of this district of Mandridrano extend for a distance of about 20 miles north and south, and perhaps 3 or 4 east and west. They are, for the most part, scoria cones. The cones are thickly studded over the district, in some parts clustering together more thickly than in others. There is no single large volcano to which the others are subsidiary, or upon which they are parasitic. Occasionally there is a series of cones which have evidently been heaped up by the simultaneous ejection of scoriæ from different vents situated on the same line of fissure, but so that the cones have run one into the other, leaving a ridge, generally curvilinear, at the summit. None of these extinct volcanoes reach the height of 1000 feet. Kasige, which is probably the highest, I found by aneroid to be 863 feet above the plain (5893 feet above the sea). Andranonatoa is perhaps next in height to Kasige. Kasige is a remarkably perfect and fresh-looking volcano, whose sides slope at an angle of about 40°. The scoriæ on the sides have become sufficiently disintegrated to form a soil on which are found a by no means scanty flora; for among other plants growing here I found an aloe (*A. macroclada*), and clematis (*C. trifida*), two or three Composite herbs (*Senecio cochlearifolius*, *Helichrysum lycopodioides*, *Laggera alata*, &c.), some grasses (*Imperata arundinacea*, &c.), a species of Indigofera, and an orchid. On its top is an unbreached crater, which measures, from the highest point of its rim, 243 feet in depth. It may be mentioned in passing, that on the very summit, in a hollow "cinder," there were found a small piece of money, perhaps of the value of a halfpenny, and a small bead, as also a portion of a banana leaf, with a few pieces of a manioc, and two or three earth-nuts placed upon it these had been deposited there by some of the heathen inhabitants of the place as a votive offering either to their ancestors or to the Vazimba (the aborigines of Central Madagascar). Continuous with Kasige, and adjoining its south side, though not so high, there is another volcano, Ambohimalala, and dozens of others are to be seen near by.

One thing with regard to these volcanic piles soon strikes the observer, which is, that they are frequently lop-sided, one side of the crater being higher than the other. The higher side varies from north to north-west and west. This is undoubtedly accounted for by the direction of the wind during the eruption, causing the ejected fragments to accumulate on the leeward side of the vent. Now we know that the south-east trades blow during the greater part of the year in Madagascar, hence the unequal development of the sides of the cones. The same thing may be also observed in the volcanic piles in the neighbourhood of Betafo. This phenomenon, as is well known, occurs also in other parts of the world.

A very large number of the cones have breached craters, whence lava has flowed in numerous streams and floods, covering the plains around. These streams and floods consist in every instance, I believe, of black basaltic lava; a sheet of this lava, the mingled streams of which have flowed from Ambohimalala and some other vents, has covered the plain at the foot of Kasige to such an extent as almost to surround the mountain. Similar sheets are to be seen in other parts of the district, but they are so much alike that a description of one will suffice for all. Amboditaimamo (or Ambohidratrimo?) is a small volcano to the north of Lake Itasy, and at the northern confines of the volcanic district. It possesses a breached crater turned towards the east; from this has issued a stream of lava which, following the direction of the lowest level of the ground, has swept through a small valley round the northern end of the mountain, and spread out at its west foot. This sheet of lava, which is horribly rough on the surface, occupies but a small area of some two or three square miles. It has been arrested in its flow in front by the side of the low hills. It is cut through in one part by a stream which, in some places, has worn a channel to the depth of 80 or 90 feet. Its surface, which is slightly cellular, is covered by some hundreds of mammiform hillocks, which must have been formed during the cooling of the liquid mass. The hillocks are mostly from 20 to 30 feet high, and apparently are heaped-up masses of lava, and not hollow blisters. The lava itself is black, heavy, and compact, being porphyritic with somewhat large crystals of augite. As yet it is scarcely decomposed sufficiently to form much of a soil, though grass grows on it abundantly, and a few other plants are to be seen.

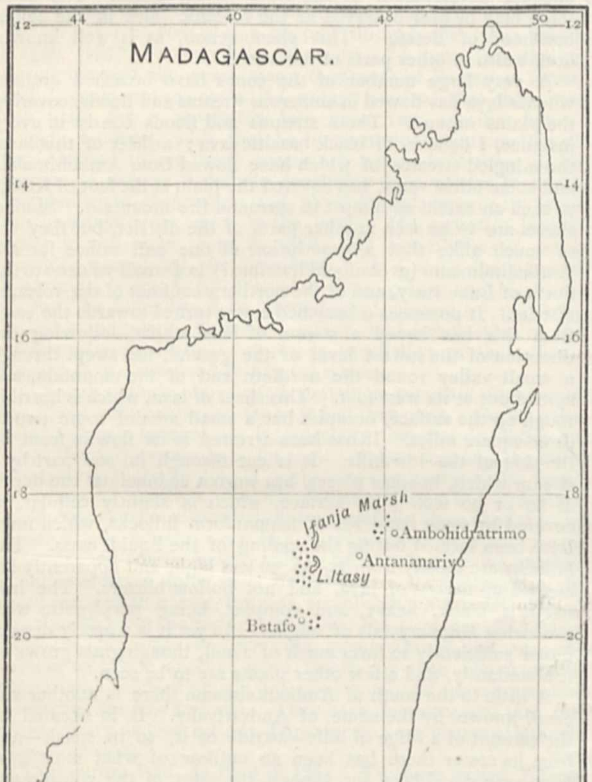
A little to the south of Amboditaimamo there is another volcano, known by the name of Andrarivahy. It is situated on the summit of a ridge of hills—astride of it, so to speak—and from its crater there has been an outflow of what must have been very viscid lava, for, though the sides of the volcano and the ridge of hills form an angle of from 30° to 40°, the ejected matter has set or "guttered" on the slope, only a small portion of it having reached the valley below. This ridge of hills, through which the volcanic orifice has been drilled, is composed entirely of gneiss; and indeed it may be here stated that the whole of these volcanoes, as is the case also with those about Betafo, rest upon a platform of gneiss, chiefly garnetiferous.

Throughout the district numerous fragments of basic lava, trachyte, trachytic tuff, and basaltic conglomerate lie scattered about in abundance. The trachyte is of various shades of yellow and gray, and frequently porphyritic with large crystals of sanidine. Pumice, obsidian, and pitchstone do not seem anywhere to be found.

In addition to the numerous scoria-cones, there may be seen here and there in the district some half-dozen or more bell-shaped hummocks of trachyte. They are for the most part composed of a light-coloured compact rock. This rock, having originally had a highly viscid or pasty consistency, has evidently accumulated, and set immediately over the orifice through which it was extruded; such hummocks are Ingolofotsy, Beteheza, Angavo, Ambasy, Isahadimy, Ambohibe, Antsahondra, &c. Ingolofotsy, situated to the north-west of Itasy, is perhaps the most striking in appearance of these trachytic hummocks. It bears some resemblance to a bell or Turkish fez, except that its sides are furrowed with water-channels and its truncated summit is notched in a remarkable manner. Its height above the plain is 665 feet (5258 feet above the sea); the inclination of its sides averages probably 50° Adjoining Ingolofotsy on the south-

[1] Scrope, in his "Volcanoes," second edition, p. 428, says of Madagascar, "There is some reason to believe in the existence of active volcanic vents in this great island;" and Dr. Daubeny, in the second edition of his "Volcanoes," p. 433, in referring to the islands on the eastern coast of Africa, says: "The principal of these are the great Island of Madagascar, the Isle of Bourbon, and the Mauritius, the first of which has been too little explored to allow of my announcing with certainty anything respecting its physical structure;" and in a note he adds: "Madagascar is stated by Daubuisson to contain volcanoes, on the authority of Ebel (*Bau der Erde*, tome ii. p. 289), who reports that in this island there is a volcano ejecting a stream of water to a sufficient height to be visible 20 leagues out at sea." What remarkable eyesight those from whom Daubuisson heard the story must have had to see an invisible phenomenon so far away! Dr. Daubeny continues: "Sir Roderick Murchison, December 1827, exhibited at the Geological Society some specimens of a volcanic nature, said to have come from this island, but the locality was not mentioned."

west is Beteheza, a large mass of trachyte which has probably welled out from an orifice on the same line of fissure from which Ingolofotsy was extruded. Angavo is another of these trachytic domes. One singular feature in this mountain is its numerous shallow water-channels, which make their way down from the summit in a surprisingly regular manner (at least on the north side), giving the appearance of an opened umbrella with numerous ribs. From one point of view I counted as many as thirty-four of these channels. It may be mentioned in passing that, in a valley at the west foot of Angavo, there is a small crater whose lips are level with the surface of the ground. This may perhaps be accounted for by supposing that the ejected materials from this and other craters near have so accumulated



as to raise the level of the valley between up to the rim of the crater, and so obliterate the cone, probably never of any great height.

It is hardly necessary to say that these extinct volcanoes of Mandridrano must have been in activity in comparatively recent times. Possibly they belong to the historic period, though no tradition lingers with regard to their being in a state of eruption.[1] That they are, at any rate, of recent date, is shown by the almost perfect state of preservation in which most of the cones are still found, and by the undecomposed (or but slightly decomposed) character of the lava-streams that have issued from them. There have been no terrestrial disturbances or modifications of any magnitude since the days of their fiery energy; the conformation of hill and dale was the same then as now, for, in every instance, the lava-streams have adapted themselves to the form of the existing valleys.

Another feature worthy of mention in this volcanic district is the lakes and marshes which occupy many of the valleys. Itasy is the largest of the lakes, and Ifanja the largest of the marshes. Now most of these lakes and marshes have been doubtless formed by the sinking in of certain portions of the district, a fact made evident by the two following circumstances:—(*a*) On the south side of Kasige the gneiss may be seen distinctly to take a sudden dip beneath the volcanic pile, showing that, as the matter has been discharged from below, there has been a settling down of the cone, a fact made further evident by the existence of a small sheet of water, known as Bobojojo, in the immediate vicinity. But (*b*) on the western side of Ifanja marsh there is a small pond known as Mandentika. In the time of King Andrianampoinimerina, about a century ago, so the people say, there was a headland projecting into this pond, upon which was situated a small village of two or three houses. On a certain unhappy day the foundations of this headland suddenly gave way, and down it sank with the village and its inhabitants, only one of the latter escaping. From that time the pond has been appropriately termed Mandentika ("sinking"), but previous to the catastrophe it was known as Amparihimboahangy. There is no doubt as to the truth of this story, as I have myself seen traces of the submerged headland and village appearing just above the surface of the water. The natives of the place say that the sinking was caused by a Fananimpitoloha, a seven-headed, mythical, serpent-like monster that is supposed to live beneath the water.

Ifanja Marsh is some four or five miles from one end to the other, and perhaps a mile or more wide in its greatest width. It runs in a northerly and southerly direction, with its southern end bending round towards the west, at the foot of which is the volcano of Amboditaimamo, mentioned above. The marsh is 3700 feet above the sea, forming a considerable depression below the surrounding country, which is about 5000 feet in altitude. At its south-eastern corner there are some hot springs which are much resorted to by sick folks.

Lake Itasy covers ground, roughly speaking, to the extent of about 25 square miles. It may not improbably occupy an area of depression due to volcanic action;[1] but be this as it may, there is a cause at its outlet sufficient to account for its formation. Here, lying in the river-bed, may be seen numerous blocks of gneiss, many of them blackened with a covering of oxide of iron; and beneath this gneiss lava may be seen. Several volcanoes cluster round the outlet; but there is one—an inconsiderable hill—situated on the southern margin of the outflowing river, just above the rapids. There distinctly enough may be seen a low and much-worn crater, with its breached side facing the outlet; and gneiss blocks may be traced from the bed of the river all up the hill-side to the crater. There has apparently been first an ejection of volcanic matter, followed probably by an explosion tearing up and flinging out the gneiss through which the vent was bored, hence the gneiss blocks are superimposed upon the lava. Thus the water has been ponded back. The river has now cut its way several feet through the barrier thus thrown across its course; and by this continual erosion at its outlet, and the accumulation of sediment, and the growth of vegetation at its head, the lake is slowly, though surely, decreasing in extent year by year.

It seems that the lava also occupies the bed of the river further down, as Mr. W. Johnson says: "Went down the Lilia as far as the waterfall at Ambohipo. A more beautiful fall I think I never saw. The river, broken into three streams, falls in foaming white masses over an edge of black lava some 50 feet deep. The whole bed of the river for a mile above is of the

[1] I was told by a native that near the village of Ambəniriana, north of Angavo, and not far from Ingolofotsy, there is an emission of gas (? "fofona"), and that the people say that formerly fire was to be seen The place is named Afotrona ("afo," fire; and "trona," grunting or hard breathing).

[1] Mr. W. Johnson says: "I am told here that Itasy was once a huge swamp, and that its becoming a clear lake is within the knowledge, or perhaps the tradi ions, of the people." If this be really true, it can only be explained on the supposition that there has been a recent subsidence of what is now the bed of the lake, as in the case of Mandentika, mentioned above.

Mr. Sibree says: "The natives say that the lake Itasy . . . was formed by a Vazimba chieftain, named Rapeto, damming up a river in the vicinity and so the rice-fields of a neighbouring chief with whom he was at variance were flooded, and have ever since remained under water."—"The Great African Island," p. 136.

same black character, the lava broken in innumerable blocks, and setting out in vivid colour the verdure of the river banks."

A good deal of what has been said respecting the volcanic district of Itasy also holds good in regard to that of the Betafo valley and neighbourhood, where, however, the volcanic cones are fewer, and where trachytic domes do not appear to exist. One of the volcanoes in the Betafo valley, Iavoko, is of greater dimensions, and has a much larger crater than any to be found about Itasy. From this volcano a large sheet of basaltic lava has issued, upon which are to be found in abundance various species of plants, notably a Euphorbia and a stonecrop (Kitchingia). Almost all the plants growing on this lava-bed, however, are of a succulent character, and can dispense with soil, requiring merely a foothold. On the sides of Iavoko may be picked up fragments of calcined gneiss, which have been torn from the sides of the vent in the passage upward of the volcanic matter. On some of the cones numerous crystals of augite as large as marbles may be found among the volcanic debris. There is one volcano, Tritriva, near Betafo, which, inasmuch as it is different in character from any others mentioned above, deserves a few words. It is one of those volcanoes off which the summit has been blown by explosive action, leaving what is known as a crater-ring, which is now the site of a small lake. The lake is not more than 100 or 200 feet in diameter, perhaps not as much as that; but there is reason to suppose that it is of very great depth. The inner sides are steep for the greater part of the circumference, but on one side the lake is easily accessible.

It is possible that, when the country is more thoroughly explored, it may be found that the volcanoes near Itasy and those in the Betafo valley are connected by intermediate ones; indeed, on Dr. Mullens's map several craters are shown somewhat west of a straight line drawn between these two volcanic districts.

About 25 or 30 miles to the north-east of Antananarivo I discovered, a couple of years ago, several small volcanic craters. These also seem to belong to the class of crater-rings or explosion craters. Although fragments of volcanic matter have been ejected from them, they are not in such quantity as to form a cone; and the craters, none of which exceed 100 yards in diameter, and 30 feet in depth, have been formed probably by a single explosion of the pent-up forces below. With the exception of scoriæ and lapilli, which are sparingly scattered about, there is no visible sign of volcanoes, and one may come to the very verge of the craters before being aware of their existence. Two of the largest craters consist of saucer-shaped depressions, but are rather elliptical than circular in form; the others consist mostly of small cavities, deep in proportion to their width. Several of the craters are occupied by sheets of water, with rushes and other aquatic plants growing around their margin.

Besides the volcanic phenomena mentioned above, thermal springs occur in various localities in the interior of Madagascar. The following is an analysis by Dr. Parker of water from springs in the district of Antsirabe:—

"On evaporation, one pint (20 oz.) of water from each spring yielded the following quantities of solid salts:—

Spring No.	1	yielded	40 grs.	of salt,	or	2 grs.	to 1	oz.	of water.
,,	2	,,	38	,,	,,	1·9	,, 1	,,	,,
,,	3	,,	42	,,	,,	2·1	,, 1	,,	,,
,,	4	,,	28	,,	,,	1·4	,, 1	,,	,,

All these springs contain the same ingredients, viz. lime, magnesia, soda, and potash, in combination with chlorine, iodine, sulphuric acid, and carbonic acid, with the addition of free carbonic acid gas."

At Antsirabe there is a deposit from one of these springs of carbonate of lime, which is occasionally used for building purposes in the capital. Bubbles of carbonic acid may be seen rising from the surface of the deposit, and at one point, where there is a small spring, a mass of calc-sinter has been formed which, speaking from memory, is probably 12 feet high by 18 feet long.

In one of the valleys in the vicinity of the crater-rings of Ambohidratrimo, spoken of above, there is a deposit of siliceous sinter. It appears in one or two places, scarcely rising above the surface of the ground, in a valley of rice-fields, and has been deposited by springs which have long since ceased to flow. The sinter is exceedingly hard and compact, and is used by the natives for fire-flints. In some portions of it numerous fossils of a species of Equisetum are embedded. The longitudinal striæ leave no doubt as to the nature of the plant. The fistular stem has been filled in, and the vegetable substance entirely replaced, by silex. The stems of some of these fossil plants are quite half an inch in diameter. Now, the only Equisetum found in Central Madagascar at the present time is *E. ramosissimum*, but this never attains to such a thickness as the Equiseta in the sinter; so that the fossil species have become extinct since the springs which deposited the geyserite were in a state of activity.

So little is known respecting earthquake phenomena in Madagascar, no scientific observations ever having been instituted, that it is scarcely worth while to refer to the subject. However, it may be stated that scarcely a year passes without one or more shocks being experienced in Central Madagascar, though they are never severe or of long duration; and the destruction caused by these earth-waves in some parts of the world is entirely unknown here. The natives, I may say in passing, strangely imagine that earthquakes are caused by a whale (Trozona) turning on its back.

Extinct volcanoes and thermal springs exist also in other parts of the island, but so little is known about them that I can do no more than merely allude to their existence. R. BARON

Antananarivo, Madagascar, December 2, 1885

Coal-Dust and Explosions

THOSE who have given the labours and conclusions of workers antecedent to, and contemporaneous with, Mr. W. Galloway, on the subject of the part played by coal-dust in mine explosions, the careful consideration which these merit in common with the results and writings of that zealous exponent of the question, will hardly feel disposed to concur in his conclusion that, except by him, "the very simple, and yet all-important, element" to which he refers in his recent letter has been treated with neglect.

On the other hand, they will consider that when Mr. Galloway "goes the length of crediting coal-dust with the *rôle* of principal agent (in coal-mine explosions), and of relegating fire-damp to a secondary position," he altogether loses sight of some very obvious facts which forbid so sweeping a conclusion.

Any one who is led, by special interest in the subject, to study the forthcoming Report of the Royal Commission on Mine Accidents, will find that the important part which may be, and no doubt frequently is, taken by dust in coal-mine disasters is recognised to its full extent, and that, in a careful consideration of the accumulated knowledge on this subject, all due weight has been given to the experimental results arrived at by Mr. Galloway and others. FREDK. A. ABEL

March 3

Deposits of the Nile Delta

PERMIT me to say that Prof. Judd is in error in supposing that I intended to withdraw my statement that desert sand underlies the Nile alluvium at a very moderate depth. The general succession of the newer deposits of Lower Egypt, according to the information I have been able to obtain (and which I have endeavoured to state as plainly as possible) is as follows, in descending order: (1) Modern alluvium, varying from zero to about 40 feet, and of course more in old eroded channels. (2) Desert sand of the Post-Glacial continental period. (3) Pleistocene or Isthmian deposits, lacustrine, estuarine, or marine. The question is not whether this succession exists—that I am prepared to argue on other grounds—but whether it appears in any or all of the recent borings. It is scarcely necessary to say that such general succession admits of alternations at the junctions of beds, and of local absence of some of its members. On finding, however, that the recent borings had been stopped by quicksand at the depth of about 35 feet, and that this quicksand consisted of the rounded grains of desert sand, and was mixed with gray clay or marl, and concretions like those of the Isthmian formation, I naturally concluded that the succession above referred to was distinctly indicated. Prof. Judd now affirms, as I understand, that, in all the Delta borings, mud of "precisely similar mineral character" to that of the surface extends to the bottom. The evidence of this, as well as the promised consideration of the other points to which I have alluded, I am content to wait for till the report appears in full. J. WM. DAWSON

Montreal, February 18

On the Intelligence of Dogs

WHEN reading in NATURE of November 12, 1885, the abstract of Sir John Lubbock's paper "On the Intelligence of Dogs," I called to mind an incident of a little Blenheim spaniel which belongs to my mother.

The readers of NATURE may perhaps be a little tired of stories relating to the intelligence of the dog, especially when these are illustrations of the effects of training. My excuse for troubling you now is that the following incident seems to indicate a singular power of *reasoning*.

"Middy" was about nine months old when he was picked off the streets of Melbourne, and he had many traits of the "larrikins," as the human waifs there are called. He had been three months in our family, and we had almost begun to despair of breaking him in to civilised life.

One Sunday my sisters set off for Sunday-school, and were surprised, on nearing the church, to find "Middy" at their heels. He was told to "go home," and he was found at the house on their return. Nothing more was said on the subject, which was forgotten by the next Sunday. But when my sisters entered the school-room on that day, great was their amusement to see the little dog seated calmly as a scholar in one of the classes! He behaved quite quietly during the lessons, and then left with the children, and trotted home alone. To prevent constant repetitions of this behaviour, he had to be caught hours before school-time and shut up. He was very clever in evading capture—crept into hiding early in the day, and bolted when we were off guard. On these occasions he was certain to be found in his place at school.

It perhaps should be especially noted that "Middy" had never been to the church before, and that a whole week had elapsed between his first and second attempts.

MARY KNOTT

7, Kaga Yashiki, Tokio, Japan, January 20

Frost in Devonshire

THE Rev. A. D. Taylor, Rector of Church Stanton, a parish in Devonshire, some 900 feet above sea-level, writes me under date of the 22nd inst. :—

"We have had for three days the most wonderful rime. The trees have been covered, every twig and bud, with ice, on the average an inch at least in depth. I have measured several pieces, and have found them 1¼ to 1½ inches from base to edge. The whole place has been like fairy-land, or a silver country. To-day it has all fallen, with a continuous rushing and rattling on the bushes for four hours. The very leaves of the laurels were so frozen that you could take off each leaf a perfect *ice-leaf*—an exact reproduction in transparent ice, of about twice the thickness of this (ordinary letter) paper, of the laurel leaf—every vein and unevenness of edge distinct and clear. The children collected scores of them, and very lovely they looked. I have never seen anything of the sort which would compare with it. The people call it *rängling* (phonetic spelling), a queer word of which I never heard before."

Keen frost in an excessively moist air no doubt sufficiently explains the beautiful phenomenon itself; but can any Devonshire man explain the country people's word?

Bregner, Bournemouth, February 24 HENRY CECIL

"Pictorial Arts of Japan"

IN my review last week of Mr. Anderson's "Pictorial Arts of Japan" I inadvertently wrote the "eight Nirvanas" of Gautama instead of the "eight incidents (more properly 'features'—*fa siang*) of the Nirvana." F. V. DICKINS

University of London, Burlington Gardens, W., March 1

DISCOVERY OF A NEW ELEMENT BY CLEMENS WINKLER[1]

IN the summer of 1885 a rich silver ore was found at Himmelsfürst, near Freiberg; it was pronounced by A. Weisbach to be a new mineral, and was named *Argyrodite*. T. Richter examined its behaviour in the blow-pipe flame, and found that it consisted chiefly of sulphur and silver together with a little mercury, which latter element has never before been found at Freiberg.

The author has analysed the new mineral, and finds that the amount of mercury only amounts to 0·21 per cent., whilst silver is present to the extent of 73–75 per cent., and sulphur to the extent of 17–18 per cent. He also finds a very small quantity of iron, and traces of arsenic. However often and however carefully the analysis was conducted, a loss of 6–7 per cent. always remained unaccounted for. After a long and laborious search for the source of this error, Clemens Winkler has at length succeeded in establishing the presence of a new element in argyrodite. *Germanium* (symbol Ge), as the new element is called, closely resembles antimony in its properties, but can, however, be sharply distinguished from the latter. The presence of arsenic and antimony in the minerals accompanying argyrodite, and the absence of a method of sharply separating these elements from germanium, made the discovery of the new element extremely difficult.

The author, having a more detailed communication in view, confines himself to the following particulars:—

When argyrodite is heated out of contact with the air, which is best effected in a current of hydrogen, a black crystalline and moderately volatile sublimate forms, which melts to brownish-red drops, and which consists principally of germanium sulphide, together with a little mercury sulphide. Germanium sulphide dissolves readily in ammonium sulphide, and, on the addition of hydrochloric acid, is thrown down again in a pure state as a snow-white precipitate, which is immediately dissolved when treated with ammonia; the presence of arsenic or antimony colours the precipitate more or less yellow.

On heating germanium sulphide in a current of air, or on warming it with nitric acid, a white oxide is produced which is not volatile at a red heat and which is soluble in potash solution; when the alkaline solution is acidulated and submitted to the action of sulphuretted hydrogen, the characteristic white precipitate is produced.

The oxide is readily reduced by hydrogen, whilst the sulphide on account of its volatility is more difficult to reduce. The element, like arsenic, has a gray colour and moderate lustre, but is volatile only at a full red heat, and is decidedly less volatile than antimony. Its vapour condenses to small crystals recalling those of sublimed iodine; these show no tendency to melt and could not be confounded with antimony.

When germanium or its sulphide is heated in a current of chlorine it yields a white chloride which is more readily volatile than antimony chloride; its acidulated aqueous solution yields a white precipitate with sulphuretted hydrogen.

The author intends to undertake the determination of the atomic weight of germanium, even if it can be decided only approximately, as this will show whether the new element is to occupy the vacant position in the periodic system between antimony and bismuth.

[1] From the *Berichte* of the Berlin Chemical Society, No. 3.

THE STORY OF BIELA'S COMET[1]

II.

BRANDES, one of the two German students spoken of, was riding in an open post-waggon on the night of Dec. 6, 1798, and saw and counted hundreds of these shooting-stars or meteors. At times they came as fast as six or seven a minute. These meteors which Brandes saw that night we know now were bits from Biela's comet. In November 1833 occurred the famous star-shower, which some of you saw. The facts of that shower gave to two New Haven men, Profs. Twining and Olmsted, the clue to the true theory of the shooting-stars. From that date

[1] A Lecture delivered by Prof. H. A. Newton, on March 9, 1874, at the Sheffield Scientific School of Yale College, U.S. From the *American Journal of Science*. Continued from p. 395.

shooting-stars have belonged to astronomy. The November meteors were admitted a new constituent of the solar system. Three years later, M. Quetelet, of Brussels, found that shooting-stars are to be seen in unusual numbers about August 10 of each year. A few months afterwards Mr. Herrick made independently the same discovery; but he also told us of star-showers in April and January. What Brandes had seen in December 1798 led Mr. Herrick, moreover, to expect a like shower in other Decembers, and he asked that shooting-stars be looked for on December 6 and 7, 1838. This shrewd guess was justified, for on the evenings of those days hundreds of these meteors were seen in America, in Europe, and in Asia by persons thus induced to look for them. These shooting-stars also had once been parts of Biela's comet, though this fact was not dreamed of at that time.

In the course of time we came to know more about the meteoroids: that in general they moved in long orbits like comets, rather than round ones like planets; that some of them were grouped in long, thin streams, many hundreds of millions of miles long, and that it was by the earth's plunging through these that we have star-showers; that the space travelled over by the earth has in it everywhere some of these small bodies, probably the outlying members of hundreds of meteoroid streams.

Also the periodic time and the path of the stream of November meteoroids were found out. Then came the interesting discovery that in this stream, and in that of the August meteoroids, lay the paths of two comets. Then Dr. Weiss of Vienna showed that the meteors seen

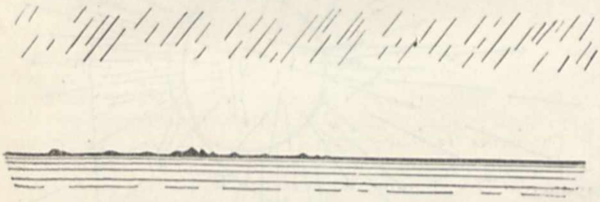


FIG. 9.

by Brandes in 1798, and by Herrick in 1838, as well as many meteors seen near December 1 of other years, and the Biela comets, all belonged to each other.

It is then properly a part of my story to show you the behaviour of one of the streams of meteoroids. Standing several hundreds of miles away, see them enter the upper atmosphere. They are entirely unseen until they strike the air. They then come down like drops of fiery rain a few miles, in parallel lines, burning up long before they reach the ground (see Fig. 9). The air is in fact a shield, protecting the men below from a furious bombardment. The region of the luminous tracks is many miles above that of the highest mountains.

Go farther away. Parallel lines may show the paths of the meteoroids (Fig. 10), though the bodies themselves are too small to be seen. They strike a little way into the air, to some persons coming from the zenith, to some coming obliquely, to some skimming through the upper air—and unseen by all upon one whole hemisphere. I need hardly remind you that sunlight, and twilight, and clouds often come in to prevent the seeing of the star-flights by persons below.

Go still farther away. From outside look in toward the sun upon the earth and meteoroid stream. The meteoroids in fact are not to be seen. The stream is of unknown depth, perhaps millions of miles deep. Its density increases in general toward the centre. We cross the densest part of the November stream in 2 or 3 hours, and the whole of it in 10 or 15 hours, while the passage of the August stream requires 3 or 4 days. The Biela stream is crossed obliquely, the meteoroids overtaking the earth. The August stream is nearly perpendicular, and the November stream meets the earth.

Again go still farther away, out to the point from which we first looked down upon the earth and comet. We then see (by the mind's eye) the meteoroids strewn along the elliptic orbit of the comet for hundreds of millions of miles, forming a stream of unknown breadth, but in the

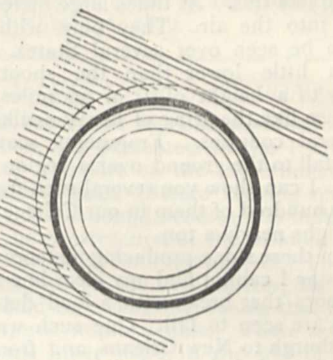


FIG. 10.

scale of the first figure shown you about $\frac{1}{80}$ of an inch in thickness.

Come back now and stand inside the stream, at its densest part. You in fact see nothing; but the meteoroids are all about you scattered quite evenly, and distant each from its nearest neighbours 20 or 30 miles. They all travel the same way and with a common motion.

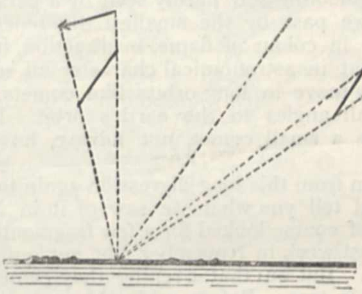


FIG. 11.

Once more change your place and look up from the earth's surface. The meteoroids can now be seen, for when they strike the air they burn with intense light, becoming shooting-stars. As it is from this position only that we ever see them, note their behaviour with more care. A shooting star coming toward you appears only as a bright stationary point in the sky. That point is a

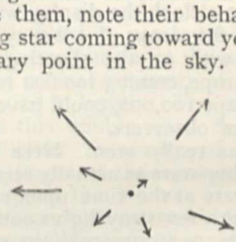


FIG. 12.

marked one in every star-shower, and is called the radiant. The meteors to the right and left of the stationary one are, in fact, moving in the common direction, but they seem to move in the sky away from the radiant (Fig. 11). In other words, the tracks produced backward will all meet in one point in the sky (Fig. 12). This radiant-point may be in the horizon, or in the zenith, or at any place between. It will in general rise in the east and set in the west, like the sun or a star, keeping always its fixed place among the stars.

Need I tell you how much we would like to have some of these bits from the meteoroid streams to handle, to try with the blowpipe and under the microscope, perhaps thus to learn something of their history? We do have something like this. At times large meteor masses come crashing into the air. They burn with a light bright enough to be seen over several States. Coming down usually a little lower than the shooting-stars, most frequently to a height of 25 or 30 miles, they break up with a noise like the firing of heavy artillery, to be heard over several counties. Fragments scattered in every direction fall to the ground over a region 10 or 20 miles in extent. I can show you several such fragments. There are over a hundred of them in our College Cabinet, one of which weighs nearly a ton.

Between these stone-producing meteors and the faintest shooting-star I cannot find any clear line of division. We have meteors that break with a loud detonation, but no fragments are seen to fall. One such was seen in 1860 from Pittsburgh to New Orleans, and from Charleston to St. Louis. It exploded over the boundary line of Tennessee and Kentucky. We have others which are only seen to break into pieces, no noise being heard. Then we have those which quietly burn out. Like the larger ones, these may leave smoky trains that last for minutes. One such I have seen for 45 minutes as it slowly floated away in the currents of the upper air.

Thus through the whole range, from the meteors that give us these stones and irons for our museums, down to the faintest shooting-star hardly seen by a person watching for it, we pass by the smallest differences. They differ in size, in colour of flame, in direction, in train, in velocity. But in astronomical character all seem to be alike. They move in long orbits like comets, and like comets at all angles to the earth's orbit. In fact, a meteoroid is a small comet, not having, however, the comet's tail.

Let us turn from this long digression again to the story of Biela, and tell you what we saw of it in November 1872. We of course looked for a few fragments from the comet the last week in November, but not quite as early as the 24th. But on that evening they came, in small numbers it is true. Before midnight we saw in New Haven about 250 shooting-stars, three-fourths of them from Biela. Very few of them were to be seen the next morning and evening. Then for a day or two it was cloudy. But in the early part of the evening of the 27th they came upon us in crowds. Over 1000 were counted in an hour. By 9 o'clock the display was over. But we saw only the last few drops of a heavy shower. Before the sun had set with us the shooting-stars were seen throughout all Europe, coming too fast to be counted. At least 50,000, perhaps 100,000, could have been seen then by a single party of observers.

Notice what was really seen. Here is a chart of the paths of the shooting-stars as actually seen on that evening, and drawn with care at the time upon maps of the stars (Fig. 13). You see a few stray flights cutting wildly across the others. These are strangers to the system.

You see also that the paths do not, as we had reason to expect, all meet in one point. This is not due to errors of observing, for we see it in every star-shower. It is probably because the small bodies glance as they strike the air, just as a stone skips on the water. In fact, we often see the meteors glance in the air—the paths being crooked.

The meteors came from the northern sky. A German astronomer, Prof. Klinkerfues, at once thought that if this was the main body of the comet it ought to be visible as it went off from us. For this, however, we must see the southern sky. He telegraphed to Mr. Pogson at Madras in India: "Biela touched earth Nov. 27. Search near Theta Centauri." Mr. Pogson looked for the comet and found it. On two mornings he saw a round comet with decided nucleus, and having on the second morning a tail 8′ long. But clouds and rain returned the next day. This is the last that has been seen of Biela's comet.

Was this Pogson comet one of the two parts of Biela seen in 1845 and 1852? This is yet an open question among astronomers. It may have been, but I think it was not. The Biela comets should have been nearly 200,000,000 miles away. Their orbits had been computed with care. The comets, as single or double, had been observed for 80 years, that is 12 revolutions, and we knew well their orbits. All known disturbing forces had been allowed for. It could hardly be that they should have gone so large a distance out of the way. It is much more probable that this was a third large fragment, thrown off centuries ago. The two observations made by Mr. Pogson were not enough to compute an orbit from, but they do

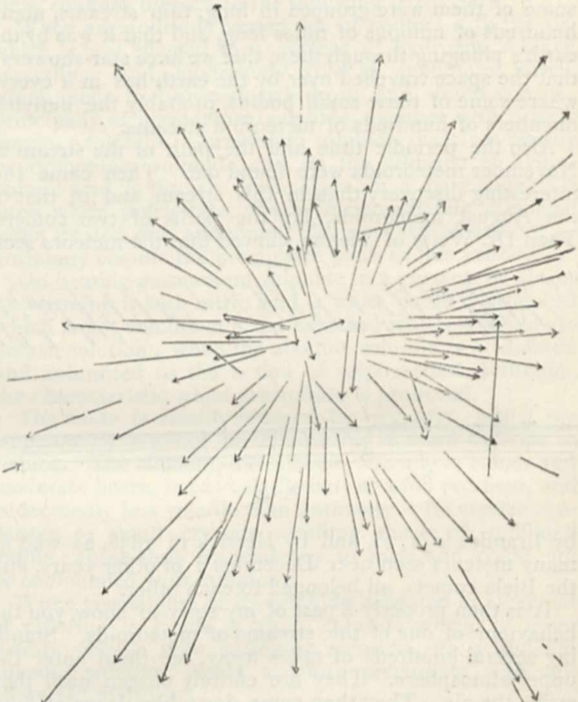


FIG. 13.

show that his comet was very near us, and were such as one travelling in the Biela stream might give. But they also show that the earth did not pass through the Pogson comet centrally.

Orbit of the Biela Meteors.—In 1798, when the earth was at N, and Brandes saw the fragments from Biela, the comet was at C (Fig. 14). In 1838 Mr. Herrick and others saw such fragments of the comet at N, 300,000,000 miles ahead of the main body at A, and in 1872 we met like fragments at N, 200,000,000 miles behind the main body, which should have been at B. Thus the fragments are strewn along the comet's orbit, probably in clusters, for at least 500,000,000 miles.

My story of Biela's comet and of its fragments has covered 100 years. Do we get any glimpses of its earlier life, and can we guess how it grew into its present shape? Yes, we may make our hypothesis. But we must not forget that to tell others how God must have made the world is bewitching to many minds, and that of the thousands of trials at world-building almost all have been grievous failures. With this caution let me give you a plausible form of this early story of Biela.

Once upon a time, hundreds of thousands of years ago, this comet was travelling in outer space, among the fixed stars, too far away to be attracted by the sun. What I mean by this outer starry space may be told by the help of the pictures I have shown you. In them the earth's distance from the sun is 10 inches, and the comet's longest range about 5 feet. Upon the scale of these figures only a few of the nearest fixed stars, perhaps two or three only, would be in the State of Connecticut. In this starry space the comet was travelling. What had happened before I do not try to guess. How, when, by what changes, its matter came together, and had become solid, I do not know, nor whether, in fact, it had not always been solid.

In the course of time its path and the sun's path through space lay alongside of each other, and the sun drew the comet down toward itself. If the comet had met no resistance as it ran around the sun, whether from the ether that fills space, or from the sun's atmosphere, and if it had not come near any of the planets, it would have gone off again into outer space whence it came. Some such cause robbed it of a little of its momentum, and it could not quite rise out of the sun's controlling force, but it came around again in an elliptic orbit to remain thenceforth a member of the solar system. It may or it may not then have been a great comet, like Donati's (in 1858).

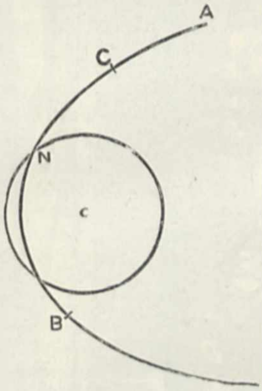


Fig. 14.

It was probably a small one. It may have made its circuit of the sun in tens of years or in tens of thousands.

At some time, probably in the early historic ages, it came near the huge planet Jupiter. When it had gone out of his reach it had just momentum enough left to go around the sun in its present orbit of $6\frac{2}{3}$ years. It went away from Jupiter an entire and single comet. As it came near the sun, his burning heat acting upon the cold rocky body of the comet cracked off and scattered in every direction small angular bits. At the same time a very thin vapour, shining by its own light, was set free. To this vapour both comet and sun had an unaccountable repulsion. It was driven off first by the comet every way. But soon that which was sent toward the sun was driven back again, and it went streaming off into space to form the comet's tail, a process ably set forth by Prof. Norton.

This matter which made the tail of the comet never got back. It had, moreover, nothing whatever to do with the meteoroid stream. The meteoroids are solid fragments. To them the sun, at least, had little repulsion. The comet was so small that perhaps the force with which a boy can throw a stone would have sent the bits of stone entirely off the comet, never to come back. Those which were shot forward from the comet near P (Fig. 1) went up along the orbit with greater velocity and rose higher from the sun than the comet did near D. Having a longer road to travel, they took a longer time to come around to P in each circuit. On the other hand, those bits which were shot backward followed the comet with less velocity and could not quite rise to D, and so having a shorter road to go over came sooner back to P, gaining on the comet at each circuit. Thus the stream grew longer slowly, and new fragments being thrown off at each circuit, the meteoroid stream grew in length to its hundreds of millions of miles. At times, the main comet has broken into two or more parts, giving us the double comets of 1845 and 1852, the Pogson comet of 1872, and the double meteor stream of November 1872.

THE NAVIGABLE BALLOON[1]

M. RENARD, captain of the Chalais-Meudon navigable balloon, has presented to the French Academy of Sciences a report of the experiments made with that balloon last year. Before starting on a fresh campaign in 1885, it was found necessary to make certain modifications in the construction of the balloon, affecting the ventilator, voltaic piles, commutators, &c. To measure the velocity of the balloon, an anemometer, the registrations of which would be too strong, seeing that the spiral is placed in front, was impracticable. There was no inconvenience, on the other hand, in the use of an aërial log. A balloon of gold-beater's skin, 120 litres in capacity, was accordingly filled in part with common gas, so as to keep exactly in equilibrium in the air. This balloon was attached to the central extremity of a bobbin of silk thread just 100 metres in length. The slightest effort is sufficient to unroll this bobbin when the central thread is drawn. The other extremity of the thread is wound round the finger of the operator. To obtain a measurement of speed the balloon is let go, when it quickly flies to the rear, and, on reaching the end of its line, conveys a perceptible indication of the fact in the finger holding the thread. The instant of its departure and that of the twitching sensation in the finger at its terminus are marked on a chronometer counting tenths of a second. Although the force transmitted to the small balloon during the unwinding of the thread is very slight, it is yet necessary to take account of it. Repeated trials in a closed place showed that the little balloon swerved 7 metres per minute, or 0·117 metre per second, under the influence of this light effort. If, then, t be taken as the time in seconds elapsing in the process of unwinding, the way traversed by the navigable balloon during the opera-

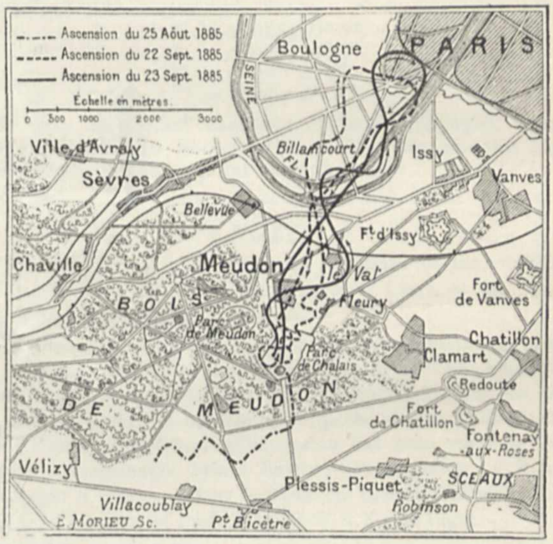


Fig. 1.—Map of the journeys of the *La France* balloon.

[1] From *La Nature*.

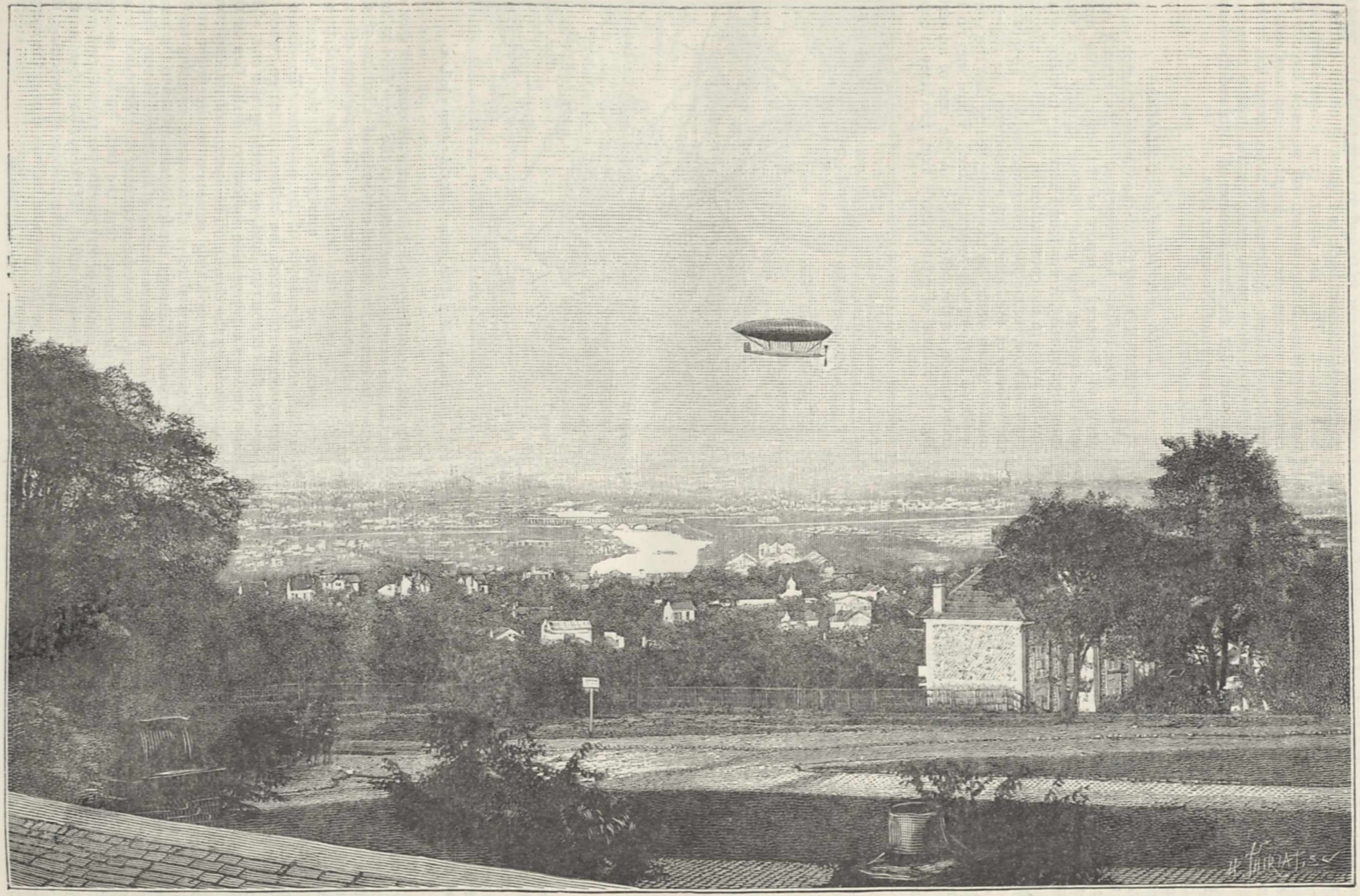


FIG. 2.—The *La France* above the Pont-du-Jour, Paris. Facsimile of an instantaneous photograph executed at the Observatory of Physical Astronomy, Meudon.

tion of unwinding will be $100 + 0{\cdot}117\,t$, and the speed of the balloon will be given by the formula—

$$S = \frac{100}{t} + 0{\cdot}117.$$

The preparations above referred to having been all completed, on the first fine day thereafter, August 25, the new mechanism was put on its trial, and behaved in a manner leaving nothing to be desired.

The balloon, which had been already filled for a certain time, having lost a considerable portion of its ascending force, M. Renard was under the necessity, on this occasion, of dispensing with the services of a third aëronaut, and mounted in the company solely of his brother, Capt. Paul Renard. The wind blew from the east. The speed measured at a low height by means of small balloons, appeared to be no more than 5 metres a second. Taking as a basis the approximate values of the preceding year, they calculated on obtaining a proper speed of nearly 7 metres per second, and were greatly surprised at being unable to gain the aërial current which prevailed at 250 metres above the valley of Chalais. The spiral, launched at fifty-five rounds per minute, proceeded with perfect regularity, yet they fell back—slowly, indeed, but continually. Desiring, nevertheless, to continue the experiment, and fearing to be carried away above the woods of the Chaville quarter, M. Renard turned the head of the balloon a little to the right, and soon, under the combined action of the wind and its own speed, it took a southern direction, and, the backward movement continuing, alighted after a voyage of 50 minutes close by the farm of Villa Coublay, whither he had directed it.

By reason of the bad weather the second definitive experiment did not come off till September 22, when the wind was blowing from the north-north-east—that is, from Paris, and its velocity in the lower strata varied from 3 to 3·50 metres per second. This time the aëronauts had their full complement of three: Capt. Renard at the helm and the motory machine, Capt. Paul Renard taking measurements and various observations, and, in addition, M. Duté-Poitevin. They started at 4.25 p.m. in a moist and foggy atmosphere. The spiral was set in motion and the head directed towards Paris. Though at first inclined to yaw, the course of the balloon soon righted itself, and, crossing the railway line above the station at 4.55, the balloon reached the Seine towards the western extremity of the island of Billancourt at 5 o'clock. Here, a measurement being taken, the progress of the balloon was found to be precisely 6 metres per second (time of unwinding = 17″, whence

$$S = \frac{100}{17} + 0{\cdot}117 = 5{\cdot}882 + 0{\cdot}117 = 5{\cdot}999\text{ m}).$$

At 5.12 p.m., after an excursion of 47 minutes, the balloon entered the enceinte by the bastion 65. It was only the increasing damp and fog which induced the aëronauts to cut short their voyage and make for home. The turning of the balloon was easily effected, and, aided this time by the aërial current, it approached its point of departure, which was entirely concealed by the fog, with surprising rapidity, retracing in 11 minutes the road it had taken 47 minutes to cover in going. The aërostat tacked about at first to keep its head to the wind, and in 10 minutes the little skiff touched the sward, whence it had ascended. During this voyage the balloon mounted to only 400 metres above the ground.

The next day before Gen. Campenon, Minister of War, and Gen. Bressonnet, President of the Committee of Fortifications, the balloon, *La France*, performed a fresh ascent with a success equal to that of the previous day. The itinerary of this voyage was much the same as that of the 22nd. The wind was weaker and bore the balloon to Paris. The time of the passage was 17 minutes going, 20 returning. The landing was very easy, and the balloon returned to the precise spot of its departure. The voyage could not be further prolonged for lack of ballast, the previous ascent having cost the balloon a partial loss of its ascending force.

On the valid basis of the experiments above described, M. Renard lays down some fundamental formulæ for calculating the resistance of balloons of construction analogous to that of *La France* with network and car.

Let R be the resistance in kilogrammes of the balloon *La France*, moving by the point; S, its speed per second in metres; θ, the work of direct traction (motory work in kilogrammetres); T, the work of the propelling screw shaft (in kilogrammetres); T', the work at the limits of the motive power in kilogrammetres); then

$$(1)\quad \left\{\begin{aligned} R &= 1{\cdot}189\,S^2. \\ \theta &= 1{\cdot}189\,S^3. \\ T &= 2{\cdot}300\,S^3. \\ T' &= 2{\cdot}800\,S^3. \end{aligned}\right.$$

At the rate of 10 metres, which would suffice for having the direction in most cases, we get

$$\begin{aligned} R &= 118{\cdot}9 \text{ kilogrammes.} \\ \theta &= 1189 \text{ kilogrammetres.} \\ T &= 2300 \text{ kilogrammetres, or 31 horse-power.} \\ T' &= 2800 \text{ kilogrammetres.} \end{aligned}$$

In general for a balloon of D diameter (in metres) we would get

$$\begin{aligned} R &= 0{\cdot}01685\,D^2 S^2. \\ \theta &= 0{\cdot}01685\,D^2 S^3. \\ T &= 0{\cdot}0326\,D^2 S^3. \\ T' &= 0{\cdot}0397\,D^2 S^3. \end{aligned}$$

It may be added that out of seven voyages, from August 9, 1884, to September 23, 1885, the aërostat has in five returned to its point of departure.

NOTES

WITH much regret we announce the death of the eminent Belgian botanist, Prof. C. J. E. Morren, of Liége, at the early age of fifty-three years.

AT a meeting of the Managers of the Royal Institution, held on Monday, March 1, the Actonian Prize of one hundred guineas was awarded to Prof. G. G. Stokes, P.R.S., for his lectures on Light, in conformity with the Acton Endowment Trust Deed. The following alteration has been made in the lecture arrangements before Easter:—Prof. Dewar, F.R.S., will begin a course of four lectures on Electro-Chemistry, on March 25, in place of Prof. Tyndall, F.R.S., on Light.

DR. JULIUS VON HAAST, C.M.G., F.R.S., the eminent geologist and Director of the Canterbury Museum, New Zealand, who is charged as Commissioner with the exhibits from that colony for the Colonial and Indian Exhibition, has arrived in London, and is busily at work in carrying out all the preliminary arrangements of the extensive court allotted to New Zealand. Dr. von Haast has been exceedingly successful in his journeys through the colony in obtaining large and valuable collections illustrating the fauna, flora, and geology, as well as collections of the art and industry of the Maori tribes. The food and other fishes, the birds, the timbers, as well as other native products and local industries will be well represented.

M. PASTEUR, at the last sitting of the Paris Academy of Sciences, stated that out of 325 cases of inoculation for hydrophobia, only one had failed—namely, that of the youth Pelletier, who came too long after being bitten, and under very unfavourable conditions. He advocated the establishment of an international hospital, to which patients would come from all parts of the world; and he suggested a discussion as to the locality and the fund for its support. At the close of the meeting Prof. Pasteur announced that he should next investigate whether diphtheria could not be treated by a similar process to that which he had found so successful against hydrophobia.

M. CHEVREUL caught a cold a few days ago, and some anxiety was entertained for his health, owing to his great age rather than to the gravity of the indisposition. We are glad to learn that he has since improved. His 101st birthday will take place on August 31 next.

IT is probable that the Observatory of Montsouris will be discontinued as an independent establishment. M. Marie-Davy will be placed on the retired list, and the credits paid in support of Montsouris suppressed entirely. It will become the headquarters of the Central Bureau of French Meteorology.

THE *Colonies and India* states that it is the intention of Mr. Morris, on leaving Jamaica to take up his appointment at Kew, to make a tour of the West India Islands for the purpose of becoming personally acquainted with their circumstances and resources, and with the view of being able afterwards to give them advice and assistance in the development of new industries. Several of the islands have already been visited by him, but it is said to be his intention to make now a careful study of their circumstances, to be afterwards embodied in a special report, or utilised in directing the resources of Kew to the amelioration and improvement of West Indian industries.

AT the Scottish Geographical Society on Tuesday afternoon, March 9, at 4 o'clock, a paper will be read by Prof. James Geikie, F.R.S., Vice-President of the Society, entitled "The Evolution of Europe."

THE Department of Public Works in Japan having recently been abolished as a separate office of State, much interest is felt, especially by scientific Europeans in the Japanese service, as to the future fate of the Imperial College of Engineering, which, since its establishment, has been under the control of the Minister of Public Works. It has been attached now to the Education Department, but it is uncertain whether it will remain a separate College or will be incoporated with the University of Tokio. In the latter case a considerable readjustment of the staff would take place, as the University has already professors of most of the subjects taught at the Engineering College, and a number of holders of Chairs of scientific subjects would be redundant. Commenting on this subject, the *Japan Mail* says that graduates of the College are found doing useful work in every part of the empire, and so high is the esteem in which they are held that to have been educated there is a certain passport to employment. It possesses the handsomest buildings and the most perfectly-equipped laboratories and museums of any educational institution in Japan, the University not excepted, and hence it would be a pity to destroy the individuality of an institution which has been so markedly successful. Accordingly it is suggested that the wisest plan would be to affiliate it to the University, and to transfer the engineering classes of the latter to its care. If any Japanese institution may be said to be British, the Engineering College may be said to be so from its foundation until the present moment. Its Chairs have all been held by English men of science, and are still held by them.

THE administration of the Ethnological Section of the Royal Museum at Berlin has commenced the publication of a periodical having for its object the description of the contents and additions to the collection. It is published by Spemann of Berlin. The first number contains an account of Dr. Nachtigal's ethnological collections, of those from Easter Island, a description of the burials of the Pelew Islands by Herr Kubary, also one of the ourney of the collector, Herr Rohde, in Matto Grosso and the Indian tribes of this region. Dr. Grube describes a collection of Taoist pictures; Dr. Grünwedel contributes notices of the iconography of the Lamas of Tibet and Mongolia, and Bishop Thiel supplies a vocabulary from Costa Rica. Some of the papers are illustrated by plates.

THE Christiania University has received a valuable collection of ethnographical objects from East Greenland from the Danish Government, as a mark of appreciation of the services rendered by Dr. Knutsen, a Norwegian naturalist, to the expedition under Lieut. Holm, of which he was a member.

THE well-known Norwegian *savant*, Dr. Rausch, makes in the Norwegian press an appeal respecting meteors and earthquakes which is not without interest. Respecting the former, he points out the scientific value of these objects, and describes their outward appearance when reaching the earth, with a request that a better look-out may be kept by people than has hitherto been the case. Only two meteorites have hitherto been found in Norway, viz. one in 1848 and one in 1884. Dr. Rausch is, however, of opinion that a great many more have fallen. Respecting the belief prevalent among the peasants that a stroke of lightning is preceded by a stone, said to be dark and burnt in appearance, the so-called "Thorelo," *i.e.* "Thor's wadding," and which are kept on farms as a kind of "household medicine," he ventures to suggest that they are meteorites, and begs those who may possess such to send him a small sample of the same. With regard to earthquakes he begs that a closer attention be given to their duration, extent, and the exact number of shocks, &c., than has hitherto been the case, remarking that only the most minute details will enable us to discover the origin and laws of these important phenomena.

A LETTER from Bagamoyo, published in *Cosmos*, describes a shower of stars seen there on November 27 last year. There were at the time neither moon nor clouds; the firmament was of a sombre blue. The phenomenon commenced at 7 o'clock, and the writer watched it until 9. Again he observed it at 11, and he was informed that it lasted all night. Bagamoyo is situated at 6° 23' south, and 36° 30' east of the meridian of Paris. Putting the average fall at eight per second, which he believes to be below the actual number, the number for the twelve hours of the night would be about 350,000 asteroids. They came from all points of the heavens, but they appeared to increase sensibly as one ascended from the horizon towards the zenith. Usually they fell singly, but sometimes a single constellation of five, eight, and ten at a time fell. Their luminous course was uniformly directed towards the south, south-east, and south-west. They did not appear to form curves, but rather to advance rapidly in straight lines. Many were like ordinary shooting-stars, but others left behind them a luminous trail of extraordinary vividness and beauty. White was the commonest colour in the train, but many had a red, yellow, and orange tint. Each lasted about a second, but some persisted for five and even ten seconds. No sound or smoke was perceived, and the phenomenon was followed by no notable change in the atmosphere. Two days after some thunder was heard and a few drops of rain fell. It is noted as curious that an old wise man on the coast had predicted shortly before that fire would fall from heaven shortly; possibly he had recognised a certain periodicity in the shower.

WE have received the first part of vol. i. of the *Annalen* of the Imperial Natural History Museum of Vienna, edited by the Director, Baron von Hauer. This part contains an account of the year's work of the Institution, the organisation of which and the arrangement of the new buildings were described in a recent number of NATURE. As the Museum includes all departments of natural history in its widest sense—geology, zoology, botany, anthropology—it will be evident that the year's work will be full of interest. This part is the first of what promises to be a series of papers issued at frequent intervals. In the next number will be papers on new species of fish from the Orinoco and Amazon, by Dr. Steindachner; on new and rare antelopes in the Museum, by Dr. Kohl; and on the flora of South Bosnia and neighbouring regions, by Dr. G. Beck.

We referred in a previous number to ova having been artificially spawned from sea-trout, *S. trutta*, at the South Kensington Aquarium, which had been retained in captivity for three years, and had therefore not visited the sea. The ova have since become incubated, and the fry, which are hybrids, appear to be in a healthy state. The mortality amongst them is heavier than with those produced from ordinary fish, especially at the period when emerging from their shell. There is an abnormal number of monstrosities amongst them, which never live beyond a few days. The ova produced from fish in captivity occupy a longer time in hatching out than those spawned from wild fish. Owing to the severity of the weather this season, hatching operations have been greatly retarded, but the fry seem to be more vigorous and healthy in consequence.

To show the value attached to the *S. fontinalis*, or brook-trout of America, when first introduced into this country some few years since, it may be mentioned that, according to the price-lists issued by pisciculturists at the time, 100*l*. per thousand was charged for them. The same amount was mentioned for whitefish, which, until last year, were not successfully acclimatised to our waters. The price charged at the present day for the same fish is about 2*l*. per thousand.

Dr. J. P. Licherdopol writes under date February 23 from Bucharest (Roumania):—"Yesterday at 3.40 p.m. a slight shock of earthquake was noticed here, followed by two others strong enough to make objects hung on the walls move. Atmosphere calm, but covered with fog. No damage."

The telephone system of Stockholm is developing rapidly: on January 15 it had 3164 subscribers, against 2335 in the beginning of 1885, and 865 in the previous year. Last year there were over 4½ million communications, of which 85,000 were by night. The subscription is about 7*l*. a year. Stockholm is connected by telephone with several neighbouring towns, the furthest being Trosa, 50 miles distant.

At a recent meeting of the Paris Medico-Psychological Society, M. Rey (who was intrusted with documents, &c., left by Broca) gave the results of 347 observations by that eminent man on the weight of the three cerebral regions (*a*) the frontal lobes, (*b*) the occipital lobes, and (*c*) the parietal regions. In men the ratio of these parts to the brain is (*a*) 1 : 2·33, (*b*) 1 : 10·66, (*c*) 1 : 2·12. In females (*a*) 1 : 2·32, (*b*) 1 : 9·88, (*c*) 1 : 2·13. In men the left frontal lobe has more weight than the right; but the difference between the two diminishes with the weight of the whole organ. In the case of the occipital lobes and the temporal regions the right side preponderates over the left. In old men the loss of weight in the parieto-frontal regions is more sensible than that in the frontal and occipital lobes. It is still more pronounced in women; so that, while in adult age men have proportionally more in the frontal lobes, this proportion is reached by women in old age. In men the frontal lobes only attain their greatest weight at 35 years of age; but at 25 the parieto-temporal regions have their maximum weight. In women, with regard to the anterior lobes, there is little difference between 25 and 35 years of age.

Mr. Van Voorst has in the press and will very soon publish a new and enlarged edition of Prof. Mills' "Manualette" of the destructive distillation of petroleum, rosin oil, coal-tar, and kindred industries, with illustrations of shale retorts.

The February number of *Petermann's Mittheilungen* contains a paper by Dr. Andries on the causes of the increasing number of accidents by lightning. As to the fact of the increase itself, he shows from statistics that this has, in the last fifty years, been three- to five-fold. In Bavaria the proportion is 1 to 5, and between 1854 and 1877 it has risen from 1 to 2·75 for all Germany. The question now is: Whence arises this striking increase? Various hypotheses have been advanced to account for it. Bezold refers it to varying maximum and minimum periods, the present being one of the former; Karsten attributes it to the decrease of forests, which made houses more and more the prominent points of a neighbourhood; others again allege the increase of lofty buildings, factories, and such like as the cause. Dr. Andries observes that although these may account for some of the increase, they cannot do so for all. Nor do they adequately explain the enormous and sudden increase in such a short period. He states the problem thus: "How can the electrical tension during thunderstorms be so increased that a greater number of bolts strike the earth than formerly? For it is not so much the increasing number of storms as their increased violence that causes the accidents by lightning." The main cause is said by him to be the enormous increase in the last half century in manufactories, locomotives, &c., filling the air with smoke, steam, and particles of dust of all kinds, the increase of population adding likewise to the impurity of the atmosphere. Having arrived at this point, viz. the enormous increase of foreign particles in the atmosphere, and their wide distribution by various currents of air, Dr. Andries describes at some length experiments made by himself and others on the subject, which showed that all the electrical phenomena of the air increased in intensity with the increase of dust in it, and to the same cause he attributes the increased appearances of the aurora borealis. Accidents by lightning in the southern half of the globe should, if this be correct, be much less frequent than in the other half; and this, he says, really appears to be the case. At any rate, he thinks, the southern lights are not visible nearly so frequently as those of the north.

The additions to the Zoological Society's Gardens during the past week include eight Viscachas (*Lagostomus trichodactylus*) from Buenos Ayres, presented by Mr. E. Vere Dashwood; four American Hares (*Lepus americanus*) from North America, presented by Mr. F. J. Thompson; six Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, presented by the Hon. Sir Julius Vogel, K.C.M.G.; a Macaque Monkey (*Macacus cynomolgus*) from India, fifteen Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, deposited; two Yucatan Blue Jays (*Cyanocitta yucatanica*) from Yucatan, two Great Barbets (*Megalæma virens*) from the Himalayas, purchased; a Red Kangaroo (*Macropus rufus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Binary Star γ Coronæ Australis.—Mr. J. E. Gore has recently computed elements of the orbit of this binary, fixing the periastron passage at 1886·53 and the period at 81·78 years. These elements differ widely from those deduced three or four years ago by Mr. Downing, who, by determining corrections to Prof. Schiaparelli's orbit, from a considerably larger number of observations than have been utilised by Mr. Gore, found the epoch of periastron passage to be 1883·203 with a period of 54·985 years. The position-angles computed from these two sets of elements now differ enormously, as is seen from the following tabular statement:—

Epoch		Angle		Distance	Computer
		°		″	
1886·0	...	51·3	...	1·28	Downing
1886·53	...	48·5	...	1·44	
1886·0	...	200·7	...	1·20	Gore
1886·53	...	196·7	...	1.13	

We venture to express the hope that those astronomers who can observe this object will not fail to do so in the present interesting stage of its physical history.

The Nebula round Maia.—Prof. E. C. Pickering states, in the *Astron. Nach.*, No. 2712, that the announcement of the discovery of the nebula near Maia by means of photography recalled to him the circumstance that certain peculiarities had been noticed in a photograph of the Pleiades taken at Harvard College Observatory on November 3, 1885. These were supposed at the time to be merely photographic defects, but it now appears that one of the markings corresponds to the Maia nebula. The other irregularities seem to afford indications of the Merope nebula. There is also a faint narrow streak of light projecting from Electra on the following side.

PROF. LANGLEY ON THE EMISSION-SPECTRA OF BODIES AT LOW TEMPERATURES.—Prof. Langley having traced the solar spectrum in the infra-red so far as $\lambda = 0·0027$mm., where it suddenly ceased, has since examined the emission-spectra of various terrestrial substances at temperatures from that of fusing platinum to that of melting ice, and more particularly of temperatures corresponding to the ordinary conditions of the soil. The result of his observations has been to show that the maximum of heat from cold and black bodies has in every case a wave-length greater than 0·0027mm.,—greater, that is to say, than that of the lowest solar heat which reaches us; and that further, that part of these spectra which has a greater wave-length than that of the point of maximum, represents a larger total amount of heat than the part with shorter wave-length. Prof. Langley believes that he has been able, by means of his bolometer, to trace out the emission-spectra of cold bodies so far as $\lambda = 0·0150$mm., a wave-length more than twenty times as great as that which Newton found for the lower limit of the spectrum, viz. $\lambda = 0·0007$mm.

FABRY'S COMET.—Dr. H. Oppenheim has computed the following fresh elements and ephemeris for this comet:—

T = 1886 April 5·5398 Berlin Mean Time

$$\left.\begin{array}{ll} \omega = & 126^\circ\ 50'\ 27''·6 \\ \Omega = & 36\ 19\ 54·0 \\ i = & 82\ 11\ 15·0 \end{array}\right\} 1886·0.$$

$$\log q = 9·804021$$

Ephemeris for Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log. r	Log. Δ	Brightness
March 7	23 19 34	31 19·6 N.	9·9441	0·1621	8
11	23 18 54	32 29·8	9·9171	0·1424	10
15	23 18 11	33 42·0	9·8904	0·1191	12
19	23 17 29	34 54·6 N.	9 8650	0·0916	16

The brightness on December 2 is taken as unity.

BARNARD'S COMET.—The following ephemeris by Dr. A. Krueger is in continuation of that given in NATURE for February 18, p. 376:—

For Berlin Midnight

1886	R.A. h. m. s.	Decl. ° ′	Log. r	Log. Δ	Brightness
March 6	1 54 54	22 35·8 N.	0·1229	0·2415	4·10
10	1 53 55	23 45·9	0·1001	0·2390	4·61
14	1 53 6	24 58·9	0·0757	0·2352	5·25
18	1 52 26	26 14·9 N.	0·0497	0·2299	6·07

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 MARCH 7-13

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

At Greenwich on March 7

Sun rises, 6h. 34m.; souths, 12h. 11m. 10·0s.; sets, 17h. 48m.; decl. on meridian, 5° 11′ S.: Sidereal Time at Sunset, 4h. 49m.

Moon (two days after New) rises, 7h. 16m.; souths, 13h. 21m.; sets, 19h. 36m.; decl. on meridian, 0° 28′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	6 54	12 49	18 44	1 47 S.
Venus	5 1	10 30	15 59	6 48 S.
Mars	17 23*	0 15	7 7	9 24 N.
Jupiter	19 10*	1 15	7 20	0 16 N.
Saturn	10 54	19 5	3 16*	22 46 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich)

March	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
8	B.A.C. 408	6½	18 11	near approach	54 —
9	64 Ceti	6	17 31	18 38	118 342
9	ξ′ Ceti	4½	18 35	19 39	156 310
13	130 Tauri	6	16 49	18 1	91 253

Saturn, March 7.—Outer major axis of outer ring 43″·0; outer minor axis of outer ring 19″·3; southern surface visible.

March	h.	
10	2	Venus stationary.

Variable-Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
T Cassiopeiæ	0 17·1	55 10 N.	Mar. 10,	0 0	m
U Cephei	0 52·2	81 16 N.	,, 8,	20 36	m
			,,	20 16	m
V Tauri	4 45·4	9 42 N.	,, 12,		M
ζ Geminorum	6 57·4	20 44 N.	,, 10,	0 0	m
δ Libræ	14 54·9	8 4 S.	,, 11,	22 10	m
R Coronæ	15 43·9	28 30 N.	,, 12,		M
U Ophiuchi	17 10·8	1 20 N.	,, 9,	2 22	m
			,, 9,	22 30	m
X Sagittarii	17 40·4	27 47 S.	,, 10,	0 0	m
			,, 12,	21 30	M
U Sagittarii	18 25·2	19 12 S.	,, 9,	22 30	m
			,, 12,	22 30	M
S Vulpeculæ	19 43·7	27 0 N.	,, 11,		m
η Aquilæ	19 46·7	0 7 N.	,, 8,	4 50	m
S Aquilæ	20 6·4	15 17 N.	,, 13,		m
δ Cephei	22 24·9	57 50 N.	,, 12,	2 30	m

M signifies maximum; m minimum.

Meteor Showers

Two showers may be looked for on March 7, viz near γ Libræ, R.A. 233°, Decl. 18° S.; and near γ Herculis, R.A. 244°, Decl. 15° N. Other showers of the week:—Near ε Cassiopeiæ, R.A. 36°, Decl. 67° N.; from Virgo, R.A. 190°, Decl. 1° N.; from Cepheus, R.A. 300°, Decl. 80° N.

Stars with Remarkable Spectra

Name of Star	R.A. 1886·0 h. m. s.	Decl. 1886·0 ° ′	Type of spectrum
124 Schjellerup	9 45 48	22 29·0 S.	IV.
132 Schjellerup	10 31 54	12 47·6 S.	IV.
D.M. +68°·617	10 37 9	68 0·6 N.	IV.
136 Schjellerup	10 46 5	20 38·8 S.	IV.
56 Leonis	10 50 5	6 47·7 N.	III.
R Crateris	10 54 58	17 42·8 S.	III.
ω Virginis	11 32 35	8 45·9 N.	III.
145 Schjellerup	12 19 24	1 24·1 N.	IV.
152 Schjellerup	12 39 46	46 3·8 N.	IV.
155*b* Schjellerup	12 51 57	66 36·6 N.	IV.
40 Comæ Ber.	13 0 49	23 13·8 N.	III.

THE SUN AND STARS[1]

II.

First Conclusions

THE view of the solar constitution, which was based upon the early work to which I have referred—work which dates from about the year 1860, and is therefore about a quarter of a century old—the view which grouped together, and endeavoured to make a complete story of all the facts which were known then, was this: the chemical substances which had been found to exist in the sun's atmosphere existed quite close—relatively quite close at all events—to the photosphere. When subsequent work demonstrated the existence of hydrogen to a considerable height above this photospheric envelope, as I shall show presently, the idea was suggested that these chemical substances existed in the atmosphere, not pell-mell, not without order, because Nature is always full of the most exquisite order, but in the sequence of their vapour-densities, so that a very heavy vapour would be found low down in the atmosphere, and a very light one like hydrogen would be high up.

It was at first suggested that gaseous diffusion would prevent such a sorting out, until it was pointed out by an American mathematician, Prof. Pierce, that it was a good deal to ask that diffusion should act along a radius something like a million of miles long, and indeed he showed that it would not.

Before we go farther, I give tables of the different substances which so far have been traced in the sun's atmosphere by means of their spectral lines. The first gives the substances according to the results obtained by Kirchhoff,

[1] A Course of Lectures to Working Men delivered by J. Norman Lockyer, F.R.S., at the Museum of Practical Geology. Revised from shorthand notes. Continued from p. 403.

Ångström, and Thalèn, who were the first workers in this field of inquiry.

TABLE A.—*Elements present in the Sun according to Kirchhoff, Ångström, and Thalèn*

Kirchhoff	Ångström and Thalèn
Sodium	Sodium
Iron	Iron
Calcium	Calcium
Magnesium	Magnesium
Nickel	Nickel
Barium	—
Copper	—
Zinc	—
	Chromium
	Cobalt
	Hydrogen
	Manganese
	Titanium

Another gives the substances which were added to the preceding list by taking a special consideration into account. Some time after the first work on the chemical composition of the solar atmosphere was accomplished, a method was introduced by which it was easy to determine the existence of a small quantity of any particular vapour in a mixture of vapours, so that the substances indicated in the second table are those substances which possibly exist in the sun's atmosphere in a small quantity only.

TABLE B.—*Elements the Longest Lines of which coincide with Fraunhofer Lines*

Certainly coincident	Probably coincident
Aluminium	Indium
Strontium	Lithium
Lead	Rubidium
Cadmium	Cæsium
Cerium	Bismuth
Uranium	Tin
Potassium	Silver
Vanadium	Glucinum
Palladium	Lanthanum
Molybdenum	Yttrium or Erbium

It is important to call special attention to the fact that Ångström and Thalèn, who followed Kirchhoff, did not agree with regard to barium, or copper, or zinc, and they added chromium, cobalt, hydrogen (a very notable addition), manganese, and titanium, the existence of which Kirchhoff had not discovered in the solar atmosphere.

A detailed study of the facts recorded by Ångström gives a good idea of the immense difficulty of the research, and also of the doubts and of the difficulties which were suggested in the very first part of the inquiry. For instance, in the case of sodium, what Ångström did, of course, was to get the vapour of sodium incandescent in the laboratory, and he got the eight familiar lines. He then observed whether there were dark lines corresponding with all of them. He found that there were. With regard to cobalt he got nineteen lines, and he found nineteen lines in the sun coinciding with them. But when he studied the spectrum of barium in his laboratory he got twenty-six lines, but of these in the solar spectrum he found only eleven. When he came to aluminium, of the fourteen lines seen in the spectrum of the metal only two existed among the Fraunhofer lines. In zinc it is not yet quite decided whether we even have two out of twenty-seven; so that we see it was not all perfectly plain sailing.

A New View

So much, then, for the chemistry of the solar atmosphere, taken as a whole. Two observations suggest themselves: the first is, that it is perfectly clear that if we have in the sun's atmosphere incandescent iron vapour, and calcium vapour, and magnesium vapour, and the incandescent vapours of many other substances which we generally know here as solid bodies, there must be tremendously strong convection-currents somewhere; for were these vapours at rest they must cool on the outside, and if they get up high enough they will condense, first into liquid particles, and then into solid particles, and then they are bound to go down. So that we see there is a new world of motion in full front of us the moment we are driven to the conclusion that we are really dealing with a mixed mass of gases so intensely hot that its constituents exist in it, except in the coldest parts of it, in a state of vapour. To enable us to think this out a little, let us consider a small part of the sun where we will imagine that the statical condition is as nearly secured as possible, and that then we suddenly upset the temperature equilibrium. When we get any solid particles, say of iron, falling into a region where they will be gradually melted, and then driven into iron vapour, the vapour is bound to reascend—it will not continue in its downward flight; whereas if vapours, by ascending, gradually get cooler and cooler, they must afterwards redescend, falling first, as I have said before, as mist and then in big liquid drops, and finally as solids—as meteorites, if you like: they are bound to go down. To put this in the most general form, we may say that in the sun's atmosphere complex molecules are bound to go down, and simple molecules are bound to go up; so that we shall have convection-currents, as I have already hinted, produced in this kind of way, and these convection-currents must exist wherever the temperature equilibrium is broken. Of course we must assume that these more or less vertical convection-currents may be modified to a certain extent by the rotation of the sun, in the same way as the up and down currents of our own air, and even the currents pole-wise and equator-wise are modified by the rotation of the earth.

The other observation which suggests itself is as follows:—We need not limit ourselves to the general chemical ideas we have acquired; the chemistry of each part of the sun (always above the photosphere) can be examined bit by bit. The photosphere has spots in it; it has the chromosphere above it with the included prominences, the inner corona above that, the outer corona above all. As a matter of fact, all these have now been examined, bit by bit, by the spectroscope—that is from the chemical point of view.

The next point of importance to urge is that a view of the solar constitution has been arrived at in consequence of this new wealth of facts, on which something must be said before we go further.

The old view put the absorbing atmosphere above the photosphere, the various chemical substances being arranged in the order of their vapour-densities, so that hydrogen would be highest, then sodium, then magnesium, till finally we arrive at iron and platinum, and so on.

Now, if that view were correct, it would be perfectly easy to prove it at once by the new method of local examination. We have only during an eclipse, or even without an eclipse, to put the slit of a spectroscope on the edge of an image of the sun thrown by an object-glass, and observe the spectrum from each part of the sun; from the photosphere to as far above the photosphere as we can get. If that view were true, we should get, as short bright lines close to the photosphere, the lines of substances having a high atomic weight. Then higher and higher we should get longer lines indicating the existence at greater heights in the solar atmosphere of those substances which have a smaller atomic weight. Further, as we have evidence to show that the spots exist in a low part of the sun's atmosphere (how low we shall see by and by, when we come to consider them in detail), we should expect in those spots to find all the familiar lines of the substances having a high atomic weight to be affected. When we examine the chromosphere and the base of the prominences which arise out of it, we should find that at the same height or about the same height where the spots give us lines of the substances of high atomic weight, the chromosphere itself should be full of the same substances of high atomic weight.

Now the fact that not one of these expectations is realised—that none of these things are so—has necessitated the putting forward of the new view to which I have referred.

This can be stated in a very few words. It is that the temperature of the sun is not only sufficient to drive all our most refractory metals into vapour, as we can do in our laboratories on the earth, but that it goes very much further; it continues the work of our laboratories, and drives them into something else altogether finer than anything that we can separate with our terrestrial conditions. According to that view, what would happen would be this:—If we could lay hold of a solar meteorite, say a hundred thousand miles from the photosphere, and watch it in its downward flight, the solid would first become liquid, it would then be vaporised, and we should have the spectrum with which we are familiar in our laboratories; but after that the vapour would still go through a series of simplifications of which we can take no count in our laboratories, because we have not the same temperature. What would happen in that view is that obviously we should know nothing whatever of the spectrum of the lower part of the atmosphere open to our inquiries. Now that is practically the fact. The spectrum of the region just above the photosphere is one of the strangest things in solar physics. Almost everything there is strange. The lines which we see are lines either altogether unknown to us, or are seen without their usual terrestrial companions. Many are found in none of the maps prepared in any of our laboratories, and whether we read this story from the facts presented by spots, or those observed in prominences, we get the same apparently inexplicable riddle.

All this, then, by way of introduction. There will be a good deal to be said as to details in the sequel. What we have next to do is to commence our detailed examination of each portion of the sun.

Description of the General Surface

To do this it is proper that we should begin with that part with which we are most familiar: I mean the photosphere—the bright shining surface which represents to most of us the actual veritable sun.

When we look at the sun by means of an ordinary telescope, taking proper precautions,—it will never do to look straight at the sun with an ordinary telescope unless we wish to be instantly blinded,—what one sees is first a bright disk, which is slightly dimmed at the edge; here and there, it may be, will be seen dark objects, the *spots*, although it may happen that no spots will be visible; on examining the disk carefully, what we further see is a strange mottling of the whole surface. The mottling is very often very delicate; but everywhere, in all parts of the sun, near the poles, near the solar equator, and universally, we get this strange mottling. These fine mottlings sometimes take certain directions, in consequence of the existence of powerful currents. Here and there we get cyclonic swirls, and here and there there is an appearance of smudginess, apparently produced by tremendous overhead currents, so to speak, that is, currents between us and that part of the sun on which they appear.

Some photographs of the sun taken of late years by Dr. Janssen at the Physical Observatory at Meudon, near Paris, have thrown great light upon the general arrangement of this mottling.

An attentive examination of his photographs shows that the surface of the photosphere has not a constitution uniform in all its parts, but that it is divided into a series of figures more or less distant from each other, and presenting a peculiar constitution. These figures have contours more or less rounded, often very rectilinear, and generally resembling polygons. The dimensions of these figures are very variable; they sometimes attain a minute and more in diameter.

While in the interior of the figures of which we speak the grains are clear, distinctly terminated, although of very variable size, in the boundary the grains are as if half effaced, stretched, strained; for the most, indeed, they have disappeared to make way for trains of matter which have replaced the granulation. Everything indicates that in these spaces, as in the penumbræ of spots, as we shall see, the photospheric matter is submitted to violent movements which have confused the granular elements.

In these investigations the sun's appearance can be better studied by these photographs than by means of the eye and telescope. This is what Dr. Janssen says on this point:—

"The photospheric network cannot be discovered by optical means applied directly to the sun. In fact, to ascertain it from the plate, it is necessary to employ glasses which enable us to embrace a certain extent of the photographic image. Then, if the magnifying power is quite suitable, if the proof is quite pure, and especially if it has received rigorously the proper exposure, it will be seen that the granulation has not everywhere the same distinctness; that the parts consisting of well-formed grains appear as currents which circulate so as to circumscribe spaces where the phenomena present the aspect we have described. But to establish this fact it is necessary to embrace a considerable portion of the solar disk, and it is this which it is impossible to realise when we look at the sun in a very powerful instrument, the field of which is, by the very fact of its power, very small. In these conditions we may very easily conclude that there exist portions where the granulation ceases to be distinct or even visible; but it is impossible to suppose that this fact is connected with a général system."

Independently, then, of the phenomena of spots (about which presently), the verdict of minute examination is that the whole photosphere is riddled by convection-currents; because I shall have to show that each of those dark markings which we will call *pores*, is the seat of a downrush, and each of those *domes*, as we will call the intervening brighter portions, is, in all probability, a dome produced by the very same cause that gives us the grand domes of our cumulus clouds on a summer's day.

The Cause of the Photosphere

In discussing any subject, especially such a subject as the sun, it does not do to avoid difficulties, and therefore I may very frankly say that one of the greatest difficulties which students of solar physics have met with up to the present time has been the absence of an easy and satisfactory way of explaining the existence, and the sharp boundary, and the intense brilliancy of the photosphere.

The photosphere, as already stated, is about 400,000 miles—in round numbers—from the sun's centre. If we take the average density of the sun at a pretty low figure, as we found reason to do in the last lecture, we note that the photosphere, assuming it to be a shell, exists in a region of low pressure, and we see in a moment that, unless we suppose the photosphere, or something immediately inside the photosphere, to be solid, there is no reason for supposing any very great increase of pressure at the photosphere itself. In fact, there are a great many reasons for regarding this as improbable, not to say impossible.

Now, if that is so, we are driven to another line of inquiry, and it is this. If there can be no sudden increase of pressure at the photospheric level to account for the sudden luminosity, to what other cause must we look? Driven to our supports, it is fair to ask whether any sudden increase of temperature will help us?

In an ordinary gas-jet we have coal-gas burning. When we examine the coal-gas flame in an ordinary fish-tail burner, with the spectroscope, what we find is this: Up to the part where the luminosity—the white light—suddenly begins, about halfway up, we get the flutings of acetylene and marsh-gas, and above that we get nothing whatever except a continuous spectrum; therefore, according to the books, we have now either a solid, or a liquid, or a densely gaseous substance to deal with. That is an obvious suggestion, and one apparently in harmony with all the facts. I think that is the general opinion now. Hence in a flame, in the non-luminous portion, we have got hydrocarbons ascending. So long as they are not dissociated they are feebly luminous. The light which they give is chiefly a fluted light, by which I mean that if we observe it with the spectroscope we do not get much continuous spectrum. When the hydrocarbons reach a certain height in the flame, their dissociation becomes possible, the solid particles of carbon are set free; these solid particles of carbon when free give a continuous spectrum totally different from that which they gave when they were associated with the hydrogen in its various proportions in the lower part of the flame.

Now, it is obvious that, generally, everything above the photosphere must be cooler than the photosphere itself. Have we then a relatively non-luminous gas going down, which at a distance of 430,000 miles from the sun's centre finds a region where chemical combination is destroyed, the effect being exactly the same—different in degree, but not different in kind—from that which we watch in a candle or gas-flame, imagining the gas-flame to be inverted for the sake of simplicity? That is the question. Is it along such a line as this one is to look for the solution of the mystery of the sudden brightening of the photosphere, rather than along that other one which attributes the increase of brilliancy to the sudden increase of pressure, for which really one sees no physical basis at all?

The Faculæ

It was stated that the pores were supposed to be the seats of downrushes, and that the domes between the pores were the equivalents of our cumulus clouds.

The brighter portions of the photosphere, called *faculæ*, consist of domes heaped up together, or arranged in certain directions. We shall find by and by that they are associated with a certain stage in the history of every spot. But they are by no means limited to the vicinity of spots. We may have some development of these faculæ in parts of the sun where there are no spots at all.

Those who are familiar with this class of observations will remember that it is much easier to see the faculæ near the sun's limb than in the centre of the sun. Also it is easier to get a photograph of the faculæ using a collodion or a dry plate which works very far up in the blue, than it is with a collodion or a dry plate which works in the green or the blue-green; this latter fact proves to us quite conclusively, as it was pointed out a good many years ago now,[1] that the difference between the light at the top of a dome, so to speak, or the bottom, or between the top of the cumulus and the base of the pore, is a difference chiefly of that kind of light which writes its record by means of the absorption of the blue end of the spectrum.

The reason that we see the sun red at sunrise and sunset frequently is not that there is anything different in our air at that moment, but because we are looking at the sun through a greater thickness of the air; and the redness of the sun is the balance left after our atmosphere has done all it can in the way of absorbing the blue. We do not expect to get the sun red at midday. Of course a London fog will do anything; but I am talking of our ordinary atmosphere; and the fact that we do not get the sun red in the middle of the day is one of the same kind as the other one that we do not so easily see the faculæ on the centre of the sun as we do at the edge of it. There is absorption going on between the top of a facula and the bottom of a pore; and, as you know, to get that out in its greatest vigour and quantity we must take the greatest possible thickness of atmosphere. We see in a moment that the only way to have a considerable thickness of solar atmosphere to work this for is to make observations near the sun's limb.

These faculæ exist on an enormous scale. It is quite common to see reaches of them tens of thousands of miles long, lasting for days, and perhaps weeks; we get in that fact an indication of the enormous amount of energy which may still be changing places in the solar atmosphere, even though we do not get other phenomena which appear to us to be more important. By "other phenomena" of course I mean the spots.

J. NORMAN LOCKYER

(*To be continued.*)

[1] In 1872; see "Solar Physics," p. 404.

BARK BREAD

MOST travellers in Norway have probably had more than sufficient opportunities of becoming acquainted with the so-called "Fladbröd," flat bread, of the country. Few, however, among them who have partaken of this dry and insipid food may possibly be aware that in many districts, more especially in Hardanger, the chief ingredient in its composition is the bark of trees. This substitution of an indigestible product for *bonâ fide* flour is not necessarily a proof of the scarcity of cereals, but is to be ascribed rather to an opinion prevalent among the peasant women that the bark of young pine branches, or twigs of the elm, are capable of being made into a thinner paste than unadulterated barley or rye-meal, of which the Norse housewife, who prides herself on the lightness of her "Fladbröd," puts in only enough to make the compound hold together.

The absence of any nutritive property in bark bread, whether made with elm or pine bark, and the positive injury it may do the digestive organs, has of late attracted much notice among Norwegian physiologists, and the editor of *Naturen*, with a view of calling the attention of the public to the subject, has, with the author's permission, reprinted some remarks by Dr. Schübeler on the history and character of the bark bread of Scandinavia. From this source we learn that the oldest reference to the use of bark bread in Norway occurs in a poem, ascribed to the Skald Sighvat, who lived in the first half of the eleventh century. In the year 1300 the annals of Gothland record a season of dearth, in which men were forced to eat the bark and leaf-buds of trees, while then, and during the later periods of the Middle Ages, the frequent failure of the crops in all parts of Scandinavia led to the systematic use of the bones and roe of fishes, as well as the bark of trees as a substitute for genuine flour; and so extensively was the latter substance used that Pastor Herman Ruge, who in 1762 wrote a treatise on the preservation of woods, has drawn attention to the almost complete disappearance of the elm in the Bohus district, which he ascribes to the universal practice in bygone times of stripping the bark for the preparation of bread.

In Nordland and Finmark the root of *Struthiopteris germanica* and other ferns, as well as the leaves of various species of Rumex, have been largely used with barley-meal in making ordinary bread as well as "Fladbröd." In Finland the national "pettuleipa" (bark bread), which was in former times almost the only breadstuff of the country, still ranks as an ordinary article of food in Kajana, and in the forest-regions of Oesterbotten, and Tavastland. Here it is usually made of the inner layers of the pine-bark, ground to a meal, which is mixed with a small quantity of rye-flour to give the requisite tenacity to the dough. The Finlanders of an older generation showed marvellous ingenuity in composing breadstuffs, in which scarcely a trace of any cereal could be detected in the mixture of bark, berries, seeds, bulbs, and roots of wild plants, which they seem to have accepted as a perfectly legitimate substitute for corn-bread. In the interior of Sweden, according to Prof. Säve, the *best* bread of the peasants consisted till the middle of this century of pease, oats, and barley-meal in equal proportions, while in the ordinary daily bread the husks, chaff, and spikes of the oats were all ground down together. In bad seasons even this was unattainable by the Dalekarlian labourer, who had to content himself with pine-bark bread.

DILATANCY[1]

THE principal object of this lecture was to show experimental evidence of a hitherto unrecognised fact of fundamental importance in mechanical philosophy. This newly-recognised property peculiar to granular masses (named by the author "Dilatancy") would be rendered clear by the experiments. But it was not from these experiments that it had been discovered. This discovery was the result of an endeavour to conceive the mechanical properties a medium must possess in order to act the part of the all-pervading ether—transmitting waves such as light, but not such as sound, allowing free motion of bodies, causing distant bodies to gravitate, and causing forces like cohesion, elasticity, and friction between adjacent molecules, together with electricity and magnetism.

As the result of this endeavour, it appeared that the simplest conceivable medium, a mass of rigid granules in contact with each other, would answer not only one but all of these requirements, provided such shape or fit could be given to the grains that, while these rigidly preserved their shape, the medium should possess the apparently paradoxical or anti-sponge-like property of swelling in bulk when its shape was altered.

This required that the grains should so interlock that, when any change in the shape of the mass occurred, the interstices between the grains should increase. Having recognised this property as a necessity of the ether, the next question became, What must be the shape and fit of the grains so that the mass might possess this unique property? At first it seemed that there must be something special and intricate in this structure. It would obviously be possessed by grains shaped to fit into each other's interstices: this was illustrated by a model of bricks arranged to bond as in a wall; when the pile was distorted, interstices appeared. Subsequent consideration revealed this striking fact—that any shape of grains resulted in a medium possessing this property of dilatancy so long as the medium was continuous, or so long as precautions were taken to prevent rearrangement of the grains, commencing at the outside. All that was wanted was a mass of smooth hard grains, e[illegible] grain being held by the adjacent grains, and the grains on the outside being so controlled as to prevent rearrangement. This was illustrated by a model of a pile of shot, which, when in closest order, could not have its shape changed without opening the order and increasing the interstices. The pile being brought from closest to most open order by simply distorting its shape, the outside balls being forced, those in the interior were constrained to follow, showing that in no case could a rearrangement start in the interior.

Considering the generality of this conclusion, it was necessary to explain how it was that dilatancy was not a property of ordinary atomic or molecular matter. This was owing to the elasticity, cohesion, and friction which rendered molecules in-

[1] Abstract of a Lecture delivered at the Royal Institution of Great Britain, on Friday evening, February 12, 1886. By Prof. Osborne Reynolds, LL.D., F.R.S.

capable of acting the part of independent grains whose only property was to keep their shape. This was not inconsistent with dilatancy in ether, for these physical properties were possessed by the molecules of matter in consequence of the presence of the ether, and hence it was not logical that the atoms of ether should possess these properties.

If evidence of dilatancy were to be obtained from tangible matter, it was to be sought on the most commonplace, and what had hitherto been the least interesting, form, that of hard, separate grains—corn, sand, shot, &c.

That an important geometrical and mechanical property of a material system should have lain hid for thousands of years, even in sand and corn, was such a striking thought that it required no small faith in mechanical principles to undertake the search for it; and, though finding nothing but what was in accordance with previous conclusions, the evidence obtained of this long-hidden property was as much a matter of surprise to the lecturer as it could be to any of the audience.

To render the dilatancy of a mass of grains evident, it was necessary to accomplish two things: (1) the outside grains must be controlled so that they could not rearrange, and this without preventing change of shape or change of bulk; (2) it was necessary to adopt means of measuring the change of bulk or volume of the mass or of the interstices between the grains as its shape was changed. A very simple means—a thin india-rubber bag—was found to answer both these purposes to perfection. The outside grains indented themselves into the india-rubber, which prevented their changing their places, while the impervious character of the bag allowed of a continuous measure of the volume of its contents by measuring the quantity of air or water necessary to fill the interstices.

In these experiments neither the bag nor the fluid had anything to do with the dilatancy of the contents considered as forming part of a continuous medium, the bag merely controlling the outside members as they would be controlled by the surrounding grains, and the fluid merely measuring or limiting the volume.

India-rubber football cases were then shown full of dry sand, shot, corn, and glass marbles, shaken down into their densest form. The bags could not be distorted, as by squeezing between two plates, without enlarging the interstices between the grains, and hence the volume of the bag. Such increase of bulk was not, owing to the change of shape, evident to the eye; but by connecting the mouth of the bag to a pressure-gauge, it appeared as the squeezing began, the pressure of the air within the interstices began to diminish, and as the squeezing went on diminished as much as 6 inches of mercury, which showed that the interstices had increased a third. These experiments were introduced mainly to prevent the impression that the character of the fluid within the interstices had anything to do with dilatancy. Water affords a more definite measure of volume than air. This was shown. A bag holding six pints of sand full of water without air, connected by a tube with the bottom of a vessel of water, drew, on being squeezed, about a pint of water frnm the vessel into the bag. This was the maximum dilation; for further squeezing the water ran back into the vessel, and then again, for still further squeezing, was drawn back again, showing that, as the change of form proceeded, the medium passed through maximum and minimum dilations.

The most striking evidence of dilatancy is obtained from the fact that, since dilatant material cannot change its shape without increasing in volume, by preventing change of volume all change of shape is prevented. By closing the communication between the bag and the vessel of water, and thus preventing further increase of volume, further change of shape was instantly prevented. Starting with the sand at its densest, and the communication closed, a pinch of 200 lbs. was put on the planes without producing the smallest apparent change in the spherical shape of the bag.

Communication with the pressure-gauge was then opened, which showed that, so far from the water in the bag being at a greater pressure than the atmosphere, it was less by 20 inches of mercury, so that a little more pressure on the planes and a vacuum would have been formed. On opening communication with the water the bag instantly responded by change of shape, and again instantly stopped when the supply was cut off.

That the thickness of the envelope was of no importance so long as it was impervious to air, was shown by using india-rubber balloons, so thin that the sand could be seen through them; one of these, which was soft and yielding when the water was in excess, became hard like a cannon-ball when the excess of water was drawn off, maintaining any shape it had when the bag was closed, supporting 200 lbs.

In this way a cast was taken from a mould, into which the bag was shaken with water in excess till it took the form of the mould; the excess of water was then drawn off, and the mould removed, leaving an image which preserved its shape loaded with 200 lbs.

The firmness and softness of the sand by the sea was shown to be due to these causes; as the tide falls it leaves the sand apparently dry, but in reality full of water, the surface of which is kept up to the surface of the fine sand by capillary attraction. This saturated sand cannot yield to the tread without dilating, and cannot dilate until it has had time to draw more water, the first effect of the foot being to draw down the capillary surface, leaving the sand apparently dry round the foot. This was shown by experiment.

The lecturer then indicated how the property of dilatancy in a continuous medium would render it capable of causing an attraction between bodies at a distance, like gravitation, and cohesion, and elastic forces between bodies close together; how the ability of the grains to rearrange at a free surface would allow bodies to move freely in the medium which, if in a state of agitation by transverse waves in all directions, would transmit waves like those of light, but not like sound, and which if consisting of grains of two different sizes or shape, would give rise to phenomena resembling those of electricity.

In conclusion, it was remarked that, promising as this dilatant hypothesis of ether was, it could not be taken as proved until it had been worked out in detail. This would take long, and in the meantime it was put forward to add interest to the property of dilatancy, to the discovery of which it had led. The property of dilatancy once recognised was, however, independent of any hypothesis, and seemed to have opened up a new field for philosophical and mathematical research quite independent of the ether.

SOCIETIES AND ACADEMIES

London

Royal Microscopical Society, February 10.—The President, Rev. Dr. Dallinger, F.R.S., in the chair.—The President referred to the loss sustained by the death of Mr. P. H. Lealand, to whom microscopists were so largely indebted for the optica productions which were so well known and appreciated.—The Report of the Council was read and adopted.—Dr. Dallinger then gave his annual address, in which he detailed the results of his later researches into the life-history of minute septic organisms as carried on by means of the improved lenses constructed for him by Messrs. Powell and Lealand. Four forms weie selected for study. Each of these septic organisms terminate a long series of fissions with what is practically a generative act of fusion. The two last of a long chain of self-divided forms fuse into one, become quite still, and at length the investing sac bursts, and a countless host of germs is poured forth. The growth of these germs into forms like the parent was continuously watched, showing gradual enlargement, and ultimate, but as to time somewhat uncertain, appearance of the nucleus, and the somewhat sudden appearance of the flagella or thread-like motor organs, the latter being found in each instance to arise in the nucleus. Very soon after the adult stage is reached the act of self-division commences, and is kept up for hours in succession. The delicate plexus-like structure becomes aggregated at one end of the nucleus, leaving the rest perfectly clear, except that a faint beading is seen in the middle line, with two or three finer threads from it to the plexus. Then occurs the commencement of partition of the nucleus, followed by a slight indication of division of the body-substance. Quickly afterwards the nucleus becomes completely cleft, and the body-substance follows suit. Then the plexus-like condition is again diffused equally over the whole nucleus. When the generative condition is approached by the last generation of a long series of dividing forms, it is remarkable that the organism becomes amœboid, showing how far-reaching is the amœboid state. In this condition, when two such forms touch one another they coalesce and fuse into each other almost as though two globules of mercury had touched, until nucleus reaches nucleus and the two melt into one, and the blended bodies become a globular sac, which ultimately emits an enormous number of germs. Previous to the blending it is now made out that all traces of plexus-like structure are lost in the nucleus, which becomes greatly enlarged and assumes a milky aspect, and shows no trace of structure throughout the process of fusion. Afterwards it begins to diffuse itself radially through the body-sarcode, until every trace of the nucleus is gone, and the still globule of living matter becomes tight and glossy, but no trace of structure can be anywhere found in it. In this condition it remains for six hours, when it emits the multitude of germs. After giving similar details about several other organisms, Dr. Dallinger summed up thus:—"One thing appears clear; the nucleus is the centre of all the higher activities in these organisms. The germ itself appears to be but an undeveloped nucleus, and when that nucleus has attained its full dimensions there is a pause in growth, in order that its internal development may be accomplished. It becomes practically indisputable that the body-sarcode is, so to speak, a secretion, a vital product of the nucleus. From it the flagella originally arise; by it the act of fission is initiated and in all probability carried to the end; the same is the case with fertilisation and the production of germs. We are thus brought into close relation with the behaviour of the nucleus in the simplest condition. No doubt far profounder and subtler changes are concurrently proceeding. We of course are no nearer to the solution of what life is. But to come any distance nearer to a knowledge of how the most living part of the minutest organisms acts in detail has for me, and for most biologists, an increasing fascination." The address was illustrated by the aid of the oxy-hydrogen lantern.—The new Council was elected, Dr. Dallinger being elected President for a third term.

BIRMINGHAM

Philosophical Society, February 11.—On Resistance at surfaces of electrodes in electrolytic cells, by G. Gore, LL.D., F.R.S. This paper is a purely experimental one, and contains new proofs of some of the chief results of an extensive research on "transfer-resistance," communicated to the Royal Society, March 2, 1885 (*Proc.* Roy. Soc., 1885, No. 236, p. 209). In it the author shows conclusively that the phenomena discovered by him, and to which he applied that term, are not due to polarisation, some kind of electromotive force, or any other form of opposing difference of electric potential, because they still remain when those causes are entirely absent. He selected various cases of voltaic inversion, in which a pair of different metals in an exciting electrolyte produced no difference of electric potential and no voltaic current, and examined them for "resistance" and differences of "resistance" at the immersed surfaces of the two metals. He first tested them by a "bridge" method, and then by a "condenser" one, also described, and gives the results; and in every case he found that the "resistance" still existed, and was different in amount at the two plates. In each case the plates were of equal sizes. He also took several cases in which a pair of plates of the same metal, but of different sizes, were immersed in an exciting electrolyte, a combination which it is well known produces no difference of electric potential and no voltaic current, and tested them similarly, and found abundant evidence of "resistance," different in amount at the two plates in each instance. By the condenser method he also measured the amounts of such "resistance" at the surfaces of the two different metals, of several voltaic elements at their inversion-points, during absence of difference of electric potential, and gives the quantities. He asks: "Is the phenomenon I have discovered really of the nature of ordinary electric conduction-resistance? If it is, its characters will agree with the most essential ones of that influence. It agrees in several important points with that resistance: first, it is not able to produce a current; second, it is usually small with those liquids in which ordinary resistance is small; and third, it is considerably reduced in liquids by rise of temperature, it also, when overcome by current, evolves heat" (*Proc.* Roy. Soc., 1885, No. 236, p. 209; *Phil. Mag.*, vol. xxi. 1886, pp. 130–148). It differs, however, from such resistance in the less important circumstance that it varies in amount with the strength and density of the current; it is also usually much larger in amount than the ordinary conduction-resistance of a short section of the same liquid. "From these various fundamental truths respecting it, 'transfer-resistance' is a retarding influence essentially similar to ordinary conduction-resistance, but modified, increased in amount, and rendered more complex by taking place at the surfaces of mutual contact of two heterogeneous bodies instead of in the mass of a homogeneous substance." He concludes by remarking that "it performs an important part in the action of all voltaic batteries and electrolytic cells," and calls attention to the circumstance that "one important practical application" of it "has been made in the electro-metallurgical purification of copper on the large scale, where a great saving has been effected by arranging the depositing vats in multiple series, and thus diminish the 'transfer-resistance.'" It was in the year 1831 that the first attempt to discover this kind of resistance was made by Fechner.

PARIS

Academy of Sciences, February 22.—M. Jurien de la Gravière, President, in the chair.—Observations of the small planets made with the great meridian instrument of the Paris Observatory during the fourth quarter of the year 1885, communicated by M. Mouchez.—Determination of the elements of refraction: examination of the general geometrical conditions required to be fulfilled in the practical solution of the problem, by M. Lœwy. The question is treated under the three following heads:—(1) Given the positions of two stars, at what time of the day must the conjugated operations be effected in order to attain the greatest variation of refraction? (2) What angle of the double-mirror is most suitable for obtaining this maximum value? (3) What are the co-ordinates of the two stars enabling the observer to arrive at the maximum effect of the refraction in the minimum of time?—Experimental verification of Verdet's optical law in the directions near those that are normal to the magnetic lines of force, by MM. A. Cornu and A. Potier.—Specific determination of the imprint of fossil plants in the Carboniferous formations of the Gard, with a view to determining the sequence of the vegetable species and of the stratified rocks in this basin, by M. Grand'Eury.—On the equivalent of the terbines, by M. Lecoq de Boisbaudran.—Remarks on M. Jean Luvini's note on the subject of the conflicting theories advanced by M. Faye and his opponents to explain the action of waterspouts, whirlwinds, and analogous atmospheric phenomena, by M. Léon Lalanne. The author mentions two authentic cases which occurred on the west coast of France many years ago, and which seem inexplicable except on the supposition of *a transverse ascending movement.*—Note on the employment of dynamometric machines for the transmission of force at the marine cannon foundry of Ruelle, communicated by M. Jurien de la Gravière.—Observations of Barnard's comet made at the Imperial Observatory of Rio de Janeiro, by M. L. Cruls. These observations, made with the 0·25m. equatorial, extend over the period from July 15 to August 8, after which date the comet became invisible.—Observation of the nebula in Andromeda made at the same Observatory during the period from September 10 to December 18, 1885, by M. L. Cruls. The angle of position between the *nova* and the central nucleus of the nebula, as well as the angular distance, was measured on two separate occasions, with the following results:—

		Angle of position		Estimated distance
September 17	...	80°·9	...	15″·0
October 28 ...	...	79·1	...	12·1

—Observation of the meteoric display of November 27, 1885, made at the same Observatory, by M. Cruls. The total number of meteors observed between November 26-29 was 1792, the maximum being on November 27, when 73 were seen in five minutes and 1145 during the whole night.—Results furnished by the observation of the solar protuberances at the Roman Observatory during the year 1885, by M. P. Tacchini. The great protuberances were never seen in the neighbourhood of the Poles, but nearly always between the equator and $\pm 40°$, corresponding almost invariably with solar regions free from spots and faculæ. As regards the protuberances, solar energy may be considered as having been more active in 1885 than during the previous year.—Phosphorographic studies for the photographic reproduction of the stars, by M. Ch. V. Zenger. The author describes what he hopes may prove to be an improvement even on MM. Henry's process, which has already yielded such surprising results. He uses the phosphorescence of the sulphurets of the alkaline earths instead of the fluorescence in the preparation of his photographic plates, thereby securing greater sensitiveness and power to reproduce the invisible as well as the visible stars.—Determination of the remainder in Gauss's quadrature formula, by M. P. Mansion.—Note on a geometrical interpretation of the differential equation—

$$L\left(x\frac{dy}{dx} - y\right) - M\frac{dy}{dx} + N = 0,$$

in which *L*, *M*, and *N* denote functions of *x* and *y* at once homogeneous, algebraic, entire, and of the same degree, by M. G. Fouret.—On the coefficient of contraction of elastic solid bodies, by M. Gros.—Analysis of some specimens of the air taken at Cape Horn by the Mission sent to observe the transit of Venus, by MM. A. Muntz and E. Aubin. The mean result of the analysis gave 20·864 as compared with 20·960, Regnault's mean for the atmosphere of Paris. The proportion of oxygen appears to be also very nearly equal to that of the air in various other parts of the globe, so that the variations in the quantities of nitrogen and oxygen in the whole terrestrial atmosphere seem to oscillate within very narrow limits, as was already shown by Regnault in the course of his memorable researches.—Action of gaseous hydrochloric acid on iron, by M. F. Isambert.—Fresh researches on the earthy alkaline manganites, by M. G. Rousseau.—On the reduction of compounds optically inactive by the process of compensation, by M. E. Bichat.—Observation in reference to M. Joly's note on the titration of the phosphoric acids by means of various indicators, by M. R. Engel.—Note on the formation of monatomic alcohols derived from the essence of turpentine, by MM. G. Bouchardat and J. Lafont.—On the action of the alcoholic chlorides on ammonia at a low temperature, and on the methylic amines, by MM. Camille Vincent and Chappuis.—Note on lesions of the alcoholic nevrite, by M. Gombault.—On the *Balanoglossus sarniensis*, by M. R. Kœhler.—On the morphology of the ovary in insects, by M. Ad. Sabatier.—Note on the nervous system of *Echinus acutus*, by M. Henri Prouho.—Note on *Diplosoma Kœhleri*, a new species of Diplosomian recently found in Guernsey, by M. F. Lahille.—On the quantities of heat liberated and absorbed by plants, by M. Gaston Bonnier.—Note on a nephelinic tephrite from the valley of the Jamma, kingdom of Shoa, by M. A. Michel Lévy.—Note on the basaltic rocks of the county of Antrim, Ireland, by M. A. Lacroix.—On the Egyptian decans, by M. Omont.

BERLIN

Physical Society, December 18, 1885.—Dr. Schulze-Berge spoke on the conduction of electricity in dielectric media, a subject which had hitherto been examined in most cases only from a technical standpoint, in order to determine the insulating power of gutta-percha sheathings for telegraph wires and cables. If it were assumed that the resistance of the dielectrics differed with the thickness of the layer according to the same law as prevailed in metals, then—seeing that the resistance of a cubic centimetre of gutta-percha was, in accordance with Jenkin's determinations, equal to 25×10^{12} ohm—the thickness of a layer, the resistance of which amounted to about 100 ohm, and ought to be measurable, must be so small as to be incapable of being produced. It might possibly be the case, however, that in dielectrics the resistance varied in another relation to the thickness, and in point of fact the speaker had found that a gutta-percha layer of 1/13 mm. thickness, and a superficies of 175 square c.m. inserted between two metal plates into the circuit of a Daniell's element connected to earth, produced a very rapid discharge. Measurements executed by the speaker by means of a quadrant-electrometer on thin layers of gutta-percha, sulphur, paraffin, and sealing-wax between two metal plates, yielded resistances very well capable of being measured, and which in the case of gutta-percha amounted on an average to about 200 ohm. In the case of sulphur the values varied between 20 and 2000 ohm, and just as varied and irregular were the resistances in the case of the two other substances. The layer offering resistance was produced by placing rubber tissue or purest flowers of sulphur on a heated plate of zinc, and thereupon pressing the second heated metal plate, after which the whole was allowed to cool. In the course of time the resistance changed. In the case of sulphur it increased, in the case of paraffin and sealing-wax, however, the resistance abated; in the case of gutta-percha the resistance continued pretty equal. If the cells supplying the current were disconnected, and the dielectric brought into conjunction alone with the electrometer, then the latter showed no deviation, whence it was inferred that the dielectric did not conduct electrolytically, and that it was no electrolytic polarisation which caused the change of resistance. The measurements of resistance were also taken with the aid of the Wheatstone bridge, and, after the former results had been confirmed by this method likewise, the influence of the electromotive force on the resistance of the dielectrics was determined. If the bridge system was in equilibrium, then must it remain unaltered when the intensity of the current was varied by the insertion of a changeable resistance into the circuit of the current. The experiment, however, showed that with the change in the strength of the current the needle of the galvanometer indicated a deflection. This change of resistance with the change in the strength of the current was, as the speaker had become convinced, by means also of the bridge combination, through disconnecting the chain, not caused by an electrolytic polarisation, but probably by a dielectric polarisation, which would have to be further investigated after other dielectrics, besides the four mentioned, had been tested. The most important results of his experiments were formulated by the speaker as follows:—(1) The resistance of the dielectrics varied in relation to the thickness of the layer in a different way from metals. (2) The conduction of the dielectrics was not electrolytical. (3) The resistance depended on the electromotive force.—In the discussion following this address, Dr. Schulze-Berge further stated that the sensitiveness of his electrometer was so great that a Daniell gave a deflection of 120°.—Prof. Schwalbe gave a full report of the two volumes on "Geophysik," by Herr S. Günther, published last and this year.

BOOKS AND PAMPHLETS RECEIVED

"Gurina im Obergailthal (Kärnthen)": A. B. Meyer (Hoffmann, Dresden).—"Das Gräberfeld von Hallstatt": A. B. Meyer (Hoffmann, Dresden).—"The Chemistry of the Coal-Tar Colours": R. Benedikt, translated by E. Knight (Bell and Sons).—"Der Schall": Dr. Elsas (Freytag).—"Photo-Relief Map of Scotland": H. F. Brion and Rev. E. McClure (S.P.C.K.).—"Flowers, Fruits, and Leaves": Sir J. Lubbock (Macmillan and Co.).—"Les Aérostats Dirigeables; leur passé, leur présent, leur avenir": B. de Grilleau (Dentu, Paris).—"Führer f. Forschungsreisende": F. F. von Richthofen (Oppenheim, Berlin).—"Tourist's Guide to the Flora of the Alps": Prof. K. W. v Dallatorre (Sonnenschein and Co.).—"Hand-book of Mosses": J. E. Bagnall (Sonnenschein and Co.).—"The Laws of Nature": S. Cockburn (Sonnenschein and Co.).—"The Elements of Economics," vol. ii. part 1: H. D. McLeod (Longmans).—"Manual of Music": R. Dunstan (Hughes).—"Meteorologische Beobachtungen in Deutschland, 1883," Jahrgang vi. (Hamburg).—"Bulletin of the U.S. Geological Survey," Nos. 7 to 14 (Washington).—"Annual Report of the Comptroller of the Currency U.S., December 1, 1885" (Washington).—"Report of the International Polar Expedition to Point Barrow, Alaska": Lieut. P. H. Ray (Washington).—"On the Structure of the Brain of Sessile-Eyed Crustacea": A. L. Packard (Washington).

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