THURSDAY, NOVEMBER 18, 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. XV. and XVI. (Published by Order of Her Majesty's Government, 1886.)

VOLUME XV. contains three Reports on the univalve Mollusca collected. The first is a short Report, by Dr. Rudolph Berg, on the Marseniadæ. This family has slowly gathered round the *Helix perspicua* of Linnæus and the *Bulla latens* of O. F. Müller. These species are found in all seas. The shell is either altogether enveloped in the mantle or is very partially exposed, always either calcareous or horny. Six genera are recognised, with 33 species, 11 of which are described and figured as new.

The great bulk of the volume is taken up with the Report, by the Rev. R. Boog Watson, on the Scaphopoda and Gasteropoda. This laborious memoir occupies over 750 pages, and is illustrated by an atlas of fifty plates. Some 1300 species, new and old, were recognised among the mass collected, and there were some 400 indistinguishable forms. In a short appendix, the Marquis de Folin describes and figures the Cæcidæ. The classification adopted, "for want of a better," is that of the Messrs. Adams. The more important of the general conclusions based on the examination of the facts attending the habitat of all the distinguished forms are as follows:—(1) Depth is an important condition in connection with Molluscan life; (2) but temperature is even a more important condition than depth; (3) great differences in either depth or temperature prove barriers to distribution; (4) where these do not exist, there would seem to be no limit to universality of distribution; (5) there are without doubt such universally distributed forms. The author sees no evidence in the oldest or most widely distributed species of any essential, lasting, and progressive change.

The last Report is on the Polyplacophora, by Prof. A. C. Haddon. The number of Chitons collected was, considering the frequency and wide distribution of the group, surprisingly small. Almost the only shore-collecting done during the cruise was on or near coral-reefs, and Chitons would seem to be rare in such places. The really deep-sea forms belong to Leptochiton, of which 4 species were found, and 2 are new. These species were taken at depths of from 60 to 2300 fathoms. Three plates accompany this Report: two are from drawings by the author, and the third, a coloured plate, gives the portraits of the new species by T. H. Thomas.

Volume XVI. contains the four following Reports:—(1) On the Cephalopoda, by W. Evans Hoyle, M.A. Oxon., Naturalist on the Editorial Staff. In a preface to this valuable Report, the author acknowledges the kindly and generous assistance which he received in its execution from Prof. Steenstrup, of Copenhagen, whose knowledge of the Cephalopods is immense, and the collection under his charge is unrivalled. This Report is almost exclusively systematic in its scope, but we are promised a supplement with anatomical details. It commences with a provisional synopsis of recent Cephalopods, which will certainly be of immense value to all workers. While it is true that no systematic treatment of the class can for some time to come be other than provisional, yet the author seems to have taken the greatest pains that his shall be as natural as possible, and until we have a nearly complete knowledge of the life-history of all the forms, more will not be attainable. This list contains 388 species referred to 68 genera. It would have been, we think, an improvement if all those collected during the Expedition had been in this list distinguished by some special mark, as well as having the recorded habitats for each given. Of the 72 species found, some 32 are described as new, and for these 4 new genera and 1 family had to be established. Out of the 388 species, some 60 or 70 had been so badly described as not to be recognisable, but of some of these no doubt the types still remain. Of the new species, none pertain to those great monster cuttles—source of many a battle. The exceedingly interesting genus Cirroteuthis is enriched with three species—one, *C. magna*, being the giant of the group, and measuring 1155 millimetres in length. The type species of this genus (*C. mülleri*, Esch.) was the only one known until 1883, when a second species was described by Fischer, and now (1885) five new species have been described by Hoyle and Verrill. The balance of the evidence seems to be in favour of all the species being deep-sea forms, though at present there are great difficulties in the way of settling the question. A new genus, Amphitretus, is established for a form in which the mantle is fused with the siphon in the median line, so that there are two openings into the branchial sac. This is a quite unique feature among Cephalopods. The species *A. pelagicus* was taken near the Kermadic Islands. It seems strange that but one specimen of *Argonauta argo* was found, for this and other species are not very rare. Of the genus Octopus, 20 species are enumerated, of which 11 are described as new. The one specimen of *Spirula peronii*, found living off Banda, is referred to, but Prof. Huxley is preparing a report on its anatomy. To the already large genus, Sepia, large additions were made, and it is interesting to note that all of the 10 new species were found between Port Jackson (Australia) and Japan. The shell, or sepiostaire, was found to present differential characters in the species, and a series of new terms has been adopted to describe its various parts. The suckers also seem to offer characters of specific importance, and so possibly will the hectocotylised arms when sufficiently known. Two species of Steenstrup's genus Taonius are recorded, one, *T. hyperboreus*, the common North Atlantic form, and the second the *T.* (*Procalistes*) *suhmi* of Lankester. Willemoes-Suhm had taken it for a Clionid Pteropod. Lankester described it as a new genus; but the author regards it as but a species of Taonius, and with this Prof. Steenstrup agrees, though he thinks that the specimens found may appertain to two species. As in the case of the Argonaut so in that of the Paper Nautilus but a single specimen was found, and that off Mataku, Fiji Islands. A most important means of obtaining specimens seems to have been by the examination of the stomachs of birds, fish, and Cetacea. These creatures seem to be much more satisfactory collectors than the tow-net, in which, though it was so constantly in use, few Cephalopods were taken. Possibly the immense activity of the cuttlefish will account for this. An atlas of thirty-three plates accompanies this Report.

(2) On the Stomatopoda, by W. K. Brooks, Johns Hopkins University. These Crustacea are in their adult forms inhabitants of shallow water. The collection brought home was but small, and contained no startling novelties, so that at first the author was somewhat disappointed; but this feeling turned to delight when he discovered that the material furnished some excellent opportunities for tracing out, and that with great completeness, the phylogeny and ontogeny of this little order. This order includes about 60 adult species and a vast number of tropical larvæ. The *Challenger* collection of adult forms consisted of only 15 species, 8 of these new, while 2 others had been, to this, very inadequately described; but the collection of pelagic larval Stomatopods was peculiarly rich, and in Mr. Brooks's hands it has yielded the material for tracing the history of several of the larval types, and also, more remarkable still, for establishing in every genus except one the connection between the adults and their larval types. The larval history of these Stomatopods has been one of the most puzzling problems in morphology, and the very admirable researches of Claus had been the only guide. Though often in error, Mr. Brooks confesses that without Claus's memoir to guide him his own labours must have failed. Unlike most Malacostraca, the Stomatopods, instead of carrying their developing eggs about with them, deposit them in their deep and out-of-the-way burrows under the water. They are thus most difficult to procure, and so difficult to rear that probably not a single instance of a young Stomatopod being reared from the egg is known. The growth of the larvæ is slow, and the larval life long, and, as they are as independent and as much exposed to changes in their environment and to the struggle for existence as the adults, it has come to pass that they as larvæ have undergone countless modifications which have no reference to the life of the adult, and are therefore unrepresented in the adult organism, in which the larvæ differ *inter se* more than the adults do, thus reversing the general rule. The history of each larval type has thus to be traced by the selection and comparison of those larvæ which belong to the series, and in doing this the author was partly guided by general resemblances and partly by a series of comparative measurements. The differences between the genera are slight, and all can be grouped into a single family, Squillidæ. In the description of *Lysiosquilla excavatrix* we have a very interesting account of its habits. The Report concludes with an elaborate account of the various larval forms and their adult connections. Sixteen plates accompany the Report.

(3) On the Reef Corals, by John J. Quelch, B.Sc. Lond. The author apologises for this Report of just 200 pages being so short, as he was limited both as to time and space. A careful perusal of the memoir inclines us to the opinion that no such apology is needed. Without being a monograph, the Report forms a most important contribution to our knowledge, and this not only of the distribution of the reef corals, but also, in many instances, of their structure. The term reef coral is undoubtedly vague, but the forms described in this report belong almost entirely to the Reef Madrepores, descriptions of some few species of Millepores being added. The collection made contained representatives of 293 species referable to 69 genera, and many of the species were represented by series of specimens often presenting a considerable degree of variation. As a proof of how little known are the corals of the Pacific and Indo-Pacific Islands, it may be mentioned that 71 of the new species were found in these regions, while but 2 were from the Atlantic. No attempt has been made to describe the soft parts of the specimens. Special attention is directed to the fact that the descriptions of species apply to specimens in which the calycles are perfect; in most museum specimens these are generally to be found greatly injured, and then it is often impossible to distinguish between closely-related forms. In the treatment of the distribution of these corals, lists are given of the species obtained at each locality, together with lists of the new species, and of old species recorded from the stations for the first time. While the classification adopted is on the main based on that of Milne-Edwards and Haime, a rather startling novelty in arrangement is the merging of the Madreporia Rugosa with the section of Madreporia Aporosa. The detailed reasons for this are given on pp. 40-43; and as a result the author considers that there is not a single characteristic of the old group Rugosa which will essentially separate forms usually included under it from the more typical Astræids. Thus in many Astræids the septa present are not multiples of six, while in some typical Cyathophyllidæ the septa are simply radially arranged, without any indication whatever of a tetrameral type. Again, the presence of a fossula is scarcely even of generic value; and as to the presence in the adult rugose coral of but two sizes of septa, this phenomenon is not always present in the species, and is to be met with in some typical Astræids; while as to the tabulæ, which are no doubt very characteristic of the Rugosa as a group, still even these are present in some Astræids, and absent in some Rugosa. In a striking new Madrepore, *Moseleya latistellata*, the characters are to a marked degree intermediate.

There is yet a great deal of work to be done ere the distributional areas of the reef corals is known. Probably the coral fauna of no district, unless that of the Red Sea, has been fairly worked out. It was in the nature of things that the cruise of the *Challenger* could not, from the shortness of its sojourn at any one coral district, do much in this direction. Still some few facts of great interest have been brought to light, one of the most remarkable being the occurrence of an undoubted reef-building species, *Manicina areolata*, in Simon's Bay, between lat. 34° and 35° S., at a depth of from 10 to 20 fathoms, and at a temperature of 65° F., and this is all the more peculiar, as this coral is a well-known West Indian reef-building form. Another coral, *Cladocera arbuscula*, was also found at Simon's Bay, though a West Indian species.

Notes and descriptions of eight species of Millepora are given in an appendix. One new species is called after Mr. Murray, being the one on which he saw the living zooids of this remarkable group of Hydroids. It was *M. nodosa*, occurring at Tahiti, that afforded Prof. Moseley the material for his brilliant confirmation of the observations of L. Agassiz. Twelve plates, the figures on which are beautifully executed by Mr. H. Gawan, accompany this Report.

The concluding Report in this volume is by Prof. Sir William Turner, being on the Human Crania, &c., collected during the cruise. This forms Part 2, being on the bones of the skeleton, and is an Essay on the Comparative Osteology of those Races of Men whose bones are described in the Report, for it incorporates the study not only of the material collected during the cruise of the *Challenger*, but that brought together by the authors' eminent predecessors in the Chair of Anatomy in the University of Edinburgh.

Just a century ago Camper pointed out some of the differences existing between the pelvis of a Negro and a European, and since then a vast amount of information on the subject has been accumulated, and so far as the races described in this Report are concerned, it has been exhaustively treated by Sir W. Turner. He classifies the pelvis into three groups: dolichopellic, with a brim index above 95; mesatipellic, with a brim index from 90 to 95; and platypellic, with index below 90. As to the race and age characters of the pelvis, the details, however interesting, are too technical to be abstracted. In reference to the question of how far the mode of life may act as modifying the transverse diameter of the pelvic brim, we may add that the expression "to sit on one's hunkers," would be readily understood in the North of Ireland, where it is an attitude strictly forbidden to young people. In the section treating of the spinal column, the subjects of peculiarities of individual vertebræ and the lumbar curve are investigated; and in another section the scapula, inferior and superior extremities, are examined. In a concluding section we have a general summary, and an appendix to the first part of the memoir on the "Human Crania," in which some additional details are given of some crania from Australia, the Sandwich Islands, New Guinea, and Fuegia. An index to both parts also accompanies this Report, which is illustrated by three plates of the pelves of different races.

The greater portion of the manuscript of these two large volumes was handed to the editor between July 1885 and July 1886, and the editor is to be congratulated on the successful manner in which this immense amount of scientific matter has been seen through the press.

ELEMENTARY DYNAMICS

Lessons in Elementary Dynamics. Arranged by H. G. Madan, M.A., Assistant Master in Eton College. Pp. 180. (Edinburgh: W. and R. Chambers, 1886.)

IN this little book the author has provided teachers of elementary mechanics with a rich storehouse of materials for experimental demonstrations, although the work is not quite satisfactory in some other respects. His endeavour has been to explain some of the properties of matter, Newton's laws of motion, and the modern conceptions of energy and work, in such a manner as involves only the most elementary knowledge of mathematics. Thus symbolical reasoning and formulæ are dispensed with, and nothing assumed beyond a knowledge of arithmetic and a little easy geometry. There is a successful attempt made to arouse a real interest in the subject by continual reference to phenomena of every-day life, and especially by illustrations drawn from the sports and games of the pupils. In some cases detailed instructions are given for performing the experiments. These are valuable, and similar aid might with advantage be provided in many other instances.

The author is of opinion that mechanics ought to have the first place in a boy's scientific education. This position would be strengthened, if some series of simple experiments, to be performed by the pupils themselves, were provided, and regarded as essential.

Some expressions, such as "above," "below," "on the same level," which are usually left undefined, have their exact scientific meaning pointed out. On the other hand, there is occasionally looseness and confusion in the use of technical terms. For example, in Section 103 we read: "Momentum is the term used to express the *force* with which anything is moving." In Section 159 we have the accurate statement that, by finding the momentum of a body, we learn what *impulse* has been applied to it: here the accepted expression for the time-integral of a force is used, but we do not notice any definition of the word "impulse"; and the exposition of the second law of motion appears vague in consequence. Similarly, the *force* exerted in throwing a cricket-ball is spoken of in Section 156, where the time-integral of the force is in question.

Section 302 is devoted to the "exact valuation of the energy in a moving body," and the usual expression—energy = $\frac{1}{2}$(mass × velocity^2)—is obtained, but by a process which is at least startling. Witness these statements:—"If the work could be done *in an instant*, the energy would be exactly expressed by the product of the mass × velocity^2;" and again, "The whole amount of work which a moving body can do in the time during which its motion is being stopped will correspond to the *average* or *mean* amount of energy between that which it has at the beginning of the time and that which it has at the end of the time." *Unde, quo veni?*

After the preceding, it is a small matter to refer to Section 311, where this statement occurs: "The motion of the pendulum is an accelerated motion, and, as in al other uniformly accelerated motions, the spaces described are as the squares of the times." Here, of course the reasoning is fallacious; and, although the proof intended is sound, it involves the doctrine of limits, and wants development. It is surely better at this stage of the pupil's progress to rely on the experiments in Section 312.

There is an appendix on the metric system, and, in conclusion, a dozen pages of questions and exercises on the several chapters of the book. A. R. W.

OUR BOOK SHELF

Food-Grains of India. By A. H. Church, M.A. (London: Chapman and Hall, 1886.)

A WELL-WRITTEN, well-illustrated, and well-turned-out volume. Its thinness only enhances its elegance. Its illustrations, by Mr. G. W. Ruffles, are charming, clear, without hardness, and life-like. The text is interesting, and the number of food-grains described in excess of what most of us were aware existed. Prof. Church commences his work by what must have been to him a familiar task—describing the chief constituents of food, splitting up the sugars into their groups, and pointing out the differences between true nutrients and food-adjuncts. Part 2 is devoted to dietaries and rations. With Part 3 commences the peculiar merit and *raison d'être* of the work. After some remarks upon cereals generally, the reader is introduced *seriatim* to no fewer than twenty-three cereals, the only member of the group conspicuous by its absence being rye—a grain which occupies a very important place in Europe. The presumption is that it does not occur in India, but such a presumption surely presumes too much. Wheat is described as an annual grass of unknown origin, but we scarcely see why this *nescience* as to the origin of wheat should be especially set forth. Are we to infer that barley, oats, maize, rice, the millets, &c., are annual grasses of known origin? If so, would that the Professor had devoted a few lines in each case to this particular point! The origin of our food-grains is a deeply interesting subject, veiled, we are afraid, for the most part in mist, and only conjecturally outlined.

The author disclaims any special originality, and duly credits the works of Dr. Forbes Watson, and Messrs. Duthie and Fuller, as well as other authors, as sources from which he has industriously gathered information. Messrs. Duthie and Fuller's work, however, dealt but little with the chemistry or physiology of the plants they described, and they treated more exclusively of the cultivation of the various crops.

The interest of Prof. Church's book lies in the illustrations, which are super-excellent; in the analyses, many of which were made in the author's own laboratory; in fixing the nutrient-ratio and nutrient-value of so many foods; and, lastly, in the comprehensive view given of Indian cereal and other crops. The Indian local names and Sanskrit equivalents are also interesting. These are taken by our author on trust, but all or many of them also occur in Mr. Duthie's book, which would be a guarantee of their correctness.

JOHN WRIGHTSON

Tobacco a Farmer's Crop. By Philip Meadows Taylor. (London: Edward Stanford, 1886.)

THIS is a small book of seventy pages. The first half is occupied by pleasant matter relating to the history of tobacco in Europe not strictly or seriously relevant to the title. The latter half redeems the whole from the stigma of being unpractical. An interesting account is given of the despotic regulations of the "Régie des Tabacs," a Government Department which grants licenses for growing, manufacturing, and selling tobacco throughout France, and whose powers extend to the nomination of the cultivators, the variety of tobacco to be grown, the number of plants per hectare, and even the number of leaves permitted per plant, so that the unfortunate cultivator may and must give a perfectly accurate account of his yield down to a single leaf. The methods of cultivation followed in France are described plainly and apparently practically. The important question as to whether tobacco can be grown profitably in England is answered unhesitatingly in the affirmative, and a sensible scheme is propounded for bringing its culture into harmony with the Excise. The coldness exhibited by our Royal Agricultural Society towards the tobacco movement last April is strongly animadverted upon. As to our climate, Mr. Taylor writes as follows:—"It is stated to be too cold, too damp, too uncertain in England to allow of the introduction of the proposed culture. I cannot conceive or allow that there can exist any sensible difference between the climate of the southern counties of England and that of Picardy and Flanders. I do not take notice of Prussia and even Russia, where tobacco is grown. I believe that the general climate in Southern England is more genial than in the countries across the Channel, and I feel confident that in the said southern counties of England and in Ireland tobacco could be advantageously grown. I recall my former statement that the plant is only on the ground from June to September: cold winters, early frosts, and November fogs have naught to do with the question." The author does not appear to take into account the comparative coolness of the summer months in England, which has always prevented the successful growth of maize, vines, and probably tobacco also. This very readable little book, with its unstudied side-lights upon French rural life, and its pleasant style, may be recommended without any hesitation to the reading public.

JOHN WRIGHTSON

Marion's Practical Guide to Photography. (London: Marion and Co., 1886.)

HERE we have a very good book, which contains all necessary information and useful hints for those who are practising the art of photography. The whole process is gone through in a very clear and easy way. Extra chapters are given on different parts of the subject, such as photographic optics, re-touching, portraiture, &c. On p. 95 a table of exposures is added, preceded by explanations, taking into consideration all the variations of scenes and subjects which the amateur is likely to come across. The manufacturers deserve great credit for publishing a book in which the best way of using their apparatus is described; a book published under such conditions ought to be truly practical, and one would think that the manufacturer of bad apparatus would not be too anxious to teach his customers how to find it out.

Lecture Notes and Problems on Sound, Light, and Heat. By Charles Bird, B.A., F.G.S. (London: Relfe Bros., 1886.)

FOR students who are attending lectures on these subjects this book will be very useful, as it contains the chief fundamental formulæ, set out in a very clear manner, and it is very compact, capable of being put into one's pocket without inconvenience.

Bicycles and Tricycles for the Year 1886. By H. H. Griffin, London Athletic Club. (London: L. Upcott Gill, 1886.)

NOW that cycling has become so general, and consequently the cycle industry increased so largely, a book on the subject will doubtless be most welcome. We have here one which gives a *good* description, and in many cases a woodcut, of every known make, with the exception, perhaps, of one or two very new patterns which have been introduced very recently. We need not enter into the details from the scientific point of view, as they have been previously described (NATURE, vol. xxxiii. p. 132). A description of different varieties of bells, lamps, &c., is also given. Great pains seem to have been taken by the author to bring the book up to date, and to give an accurate account; each machine, as he tells us, having been examined by himself.

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.*]

Extension of the Corona

IN reference to the failure of observers at the late eclipse to note any such extension of the corona as was seen in 1878, I desire to add my testimony to Mr. Common's opinion (NATURE, vol. xxxiv. p. 470), that the conditions of the sky must have been wholly different; and where the visibility of the corona is in question, the atmospheric diffusion is all-important.

We have a most trustworthy criterion of the amount of diffused sky light in the visibility of the moon's limb outside the sun on the coronal background. This appears not to have been observed at all last August, and it may be useful to recall what it looked like under certain almost ideal conditions, which are not likely to recur.

On July 29, 1878, I observed it in the remarkably clear air of Colorado, and at an altitude of over 14,000 feet, on Pike's Peak, and have a vivid recollection of its appearance then. After totality, and while writing my notes, I heard a call from some bystander of "Look at the moon!" and glancing up from the paper (with an eye which could not have been in a sensitive condition), saw the moon's limb outside the sun, most conspicuously defined by a band of pearly light, which faded outward, but whose visible width can be estimated from the fact that though I went on intermittently with my notes, and took no other precaution to shield the eye than keeping it in the shadow cast by my telescope stand, the limb continued in my view under these unfavourable circumstances for *four minutes and twelve seconds* after totality was over. A similar duration was recorded by Gen. Myer, the Chief Signal Officer of the United States, who observed near me; and others at a lower altitude certified to having observed it over three minutes. Something is due to the increased sensitiveness of the eye after the darkness, but there is no doubt that, with even the slight rest of the retina which totality afforded, the phenomenon was such a salient one as to force itself on the attention of those not regarding it.

This is for a very exceptionally pure sky, of course; but if, as is stated, observers specially seeking it could not even see the limb a little outside the sun (where the corona is brightest) last August, it seems clear that no conclusions as to its non-visibility under any ordinary means are to be drawn from negative evidence of such a kind. S. P. LANGLEY

Allegheny Observatory, Allegheny, Pennyslvania

The Astronomical Theory of the Great Ice Age

IN your issue of November 4 (p. 7), my friend Mr. W. H. S. Monck asks one or two questions relative to the paper on "The Astronomical Theory of the Great Ice Age" which you did me the honour to reprint.

I take as a convenient unit the mean daily sun heat on one hemisphere. The amount of this unit is indicated by the fact that it continuously maintains the earth's temperature some 300° more or less above what it would be were the sun's heat withdrawn.

The calculations I gave showed that in the glacial winter the mean daily receipt of heat sunk to ·68 of a unit, while in the brief glacial summer the mean daily receipt was 1·38 unit.

Considering the magnitude of the unit, it is obvious that fluctuations like this must correspond to vast climatic changes of the kind postulated in the Ice age. Here it seems to me lies the great originating cause of the Ice age, and to dwell on the minor phenomena merely obscures the real point.

If it be said that no great climatic change takes place because the total sun heat in the year remains the same, then I remark, as I did at the Royal Institution, that on this principle it would be the same thing to give a horse 15 lbs. of oats a day for six months and 5 lbs. a day for the other six months as to give him 10 lbs. of oats a day all the year round. ROBERT S. BALL

The Observatory, co. Dublin, November 11

P.S.—I take this opportunity of correcting a misprint in my paper as given in NATURE (vol. xxxiv. p. 608). The maximum number of days' difference between summer and winter is 465 × eccentricity.

Abnormality in Cats' Paws

AMONG the many interesting features suggested by the genealogical table in last week's NATURE (p. 40), showing the persistence of abnormality in the number of toes on a cat's paw, there is one the significance of which seems not to have occurred to, or to have been passed over by, Mr. Edward Poulton. The peculiarity I refer to is the larger percentage of abnormality among the female offspring than among the male. Taking "Tabby V." as a starting-point, and leaving out one abnormal kitten of which the sex was unnoted, as well as the families of which no particulars are given, the total number of descendants in the table is 36, of which 12 are males and 24 females. Of the 12 males, 5 are normal and 7 abnormal, or 41⅔ and 58⅓ per cent. respectively; and among the 24 females 7 are normal and 17 abnormal, or 29⅙ and 70⅚ per cent. respectively. Or, to put it in another way, among the 12 normal kittens 5 are males, 7 are females, or 41⅔ and 58⅓ per cent. respectively, instead of 33⅓ and 66⅔ per cent. as it should be; and among the abnormals 7 are males and 17 females, or 29⅙ and 70⅚ per cent. instead of 33⅓ and 66⅔.

This would seem to indicate either (1) that there is a greater tendency among the male offspring than among the female to revert to the normal condition, or (2) that there is a tendency among the offspring to inherit rather the peculiarities of the parent of their own sex—the male parent in all cases in the table being assumed to be normal. If rather, probably the former, though the latter could easily be tested by a similar set of observations with cats, the male parent of which was abnormal, the mothers being in each case normal.

J. HERBERT HODD

Hatton Garden, London, E.C., November 15

Abnormalities in the Vertebral Column of the Common Frog

IN preparing skeletons of the frog, my students came across the following abnormalities in the vertebral column, a record of which may be not without interest:—

(1) In a large *Rana temporaria*, the centrum of the eighth vertebra, instead of being biconcave (amphicœlous), is concavo-convex (procœlous), like that of the preceding vertebra. This abnormality I have observed before.

(2) In a medium-sized *Rana temporaria*, the eighth and ninth vertebræ are both abnormal. The ninth vertebra has well-developed only one transverse process (the right) for articulation with the ilium. The other (the left) is quite small and ill-shapen; there is is no anterior zygapophysis on this side. The centrum is anteriorly convex on the right side and concave on the left side. Posteriorly, there is on the right side a convex articular surface for the urostyle; but on the left side the articular surface is ill-developed and irregular. In the eighth vertebra, the left transverse process is abnormally large and strong, has a marked backward direction, and has taken on itself the sacral function on this side, articulating with the ilium. The right transverse process is nearly, but not quite normal. There is a right, but no left, posterior zygapophysis. The anterior end of the centrum is normally concave; but the posterior end is convex on the left side and concave on the right side. The urostyle and the ilia are slightly modified in accordance with the abnormalities of the vertebræ. C. LLOYD MORGAN

University College, Bristol

Influence of Wind on Barometric Readings

ALLOW me a few words of supplement to Prof. Abbe's useful letter in NATURE of November 11, p. 29.

Sir H. James's paper is perhaps better known on this side of the Atlantic than Prof. Abbe thinks; but there undoubtedly is too great a tendency to rush into print without previously reading up what has been done. The great bibliographical work which the Signal Office has in hand will do more to check this evil than anything which could be suggested, and hence its enormous importance.

As regards the application of suction to anemometers, no reference is made to that of Bourdon,[1] of which my friend Dr. Fines was recently kind enough to show me a very fine specimen at work at his observatory at Perpignan.

The Cowl Committee of the Sanitary Institute, far from being (as has been imagined) asleep or dead, has been very hard at work, and will in a few months report the result.

I sincerely hope that Lord Rayleigh will accede to Prof. Abbe's suggestion, but in the interim I append the report of Lord Rayleigh's Southampton paper which appeared in the *Meteorological Magazine* for October 1882, p. 130:—

"'On the Effect of Wind on the Draught of Chimneys,' by Prof. Lord Rayleigh, F.R.S.

[1] See also Laughton, "Historical Sketch of Anemometry," *Quart. Journ. Roy. Met. Soc.* vol. viii. (1882), p. 177.

"The author said that the draught diminished as the direction of the wind was more and more downwards, but did not go backwards until the inclination amounted to about 30°. The maximum up-draught would occur, not, as was often supposed, with a direction of wind vertically upwards, but with one making an angle of about 30° with the vertical. A chimney with a T-piece at the top never produced an unfavourable effect on the up draught, and only in one case failed to produce a favourable one. With a T-piece to which was affixed vertical ends, every wind met with would have a favourable effect, and no wind known would have an unfavourable effect.

"Prof. De Chaumont thought that vertical ends increase the resistance of the up-draught, and described a chimney with a lamp-shade-like top and conical cap, with which it was impossible to get a down-draught." G. J. SYMONS,

62, Camden Square, N.W. Registrar Sanitary Institute

Barnard's Comet

I WONDER that more has not been written about Barnard's comet (*f* 1886). On the 9th, at 17h. 50m., in spite of the strong twilight, it was plain with the naked eye as a star. I did not notice its exact brightness, but it was perhaps equal to ρ Virginis. With the telescope its head was about 8′ diameter, and it had two faint tails at about position-angles 250° and 300°. The former, which was the brighter at its origin, was ½° long, and was straight; the latter I believe was curved, and was 1½° long. T. W. BACKHOUSE

Sunderland, November 11

Aurora

LAST evening (November 2), between the hours of seven and eight o'clock, a bright anrora was visible in this vicinity. At intervals later in the evening, patches of cirrus clouds in the northern sky became luminous. The disturbance of the suspended magnet was at its height early in the evening, when the aurora was brightest. It is interesting to note the fact that this aurora was twenty-six days removed from that of October 7 and 8, corresponding to the time of the revolution of the sun on his axis. It is noteworthy, also, that very near to the time of the appearance of each aurora there was a slight renewal of earthquake activity in South Carolina and other localities.

Lyons, New York, November 3 M. A. VEEDER

"Lung Sick"

DR. EMIL HOLUB, in writing to me a few months ago from Panda-ma-Tenka, Albert Country, Zambesi, mentions having treated his cattle in a similar manner to that referred to in NATURE of the 11th inst. (p. 29). He says:—

"Shortly after I started northward from the Vaal, a contagious disease broke out among my cattle; there was any amount of sickness among the numerous trains (forty teams a day) going to the Diamond Fields, but I could get no clue to the lameness of the front legs of my bullocks for a long time. Having shot one, the disease proved to be a contagious pleuro-pneumonia, similar to the 'lung sick' so prevalent in this neighbourhood, affecting hips and shoulder-blades, causing lameness. The lungs were partly destroyed, but the animal had but little cough. I disinfected the whole herd, and vaccinated the healthy as well as the sick. The end of the tail was pierced with a narrow-bladed dagger, and a piece of lung full of virus inserted and then bandaged. The second vaccination effectually prevented the spreading of the disease for the whole journey, even in native locations similar to the Bechuanas, in which we were surrounded with 'lung-sick' cattle dying near our encampment." PHILIP J. BUTLER

55, De Beauvoir Road, London, N., November 13

PAUL BERT

PAUL BERT, who has died at his post as Governor of Tonquin, was born at Auxerre in 1833, graduated Doctor of Medicine in 1863, and Doctor of Science in 1866. Obtaining a professorship in the Faculty of Science at Bordeaux, M. Bert devoted himself especially to physiology, and in 1869 he obtained the Chair of General Physiology in the Faculty of Science at Paris. He continued here his experiments on the influence of changes of barometric pressure on life, and presented a series of papers on the subject to the Academy of Sciences, which awarded him, in 1875, its great biennial prize of 20,000 francs. He entered political life in 1870, and has all along been known as an advanced Radical. He, however, never lost his interest in science; he did much to promote education in France, and took an active part in the legislative movement which obtained for M. Pasteur an annual pension of 12,000 francs as a national recompense. M. Bert was elected President of the Biological Society in 1878, in succession to Claude Bernard, whose most brilliant pupil he was, and more recently was admitted to the Academy of Sciences. In Gambetta's Cabinet of 1881 he was Minister of Public Instruction, and a few months ago accepted the post of Governor of Tonquin, where one of his most notable acts was the founding of a Tonquinese Academy. M. Bert's papers on "Barometric Pressure" were published as a separate volume in 1877, and his lectures at the Museum of Natural History were in 1869 published under the title of "Leçons sur la Physiologie Comparée de la Respiration." He also issued, in 1869-70, "Notes d'Anatomie et de Physiologie Comparées." For many years he had charge of the scientific department of the *République Française*.

At the sitting of the Academy of Sciences on Monday, the President, M. Jurien de la Gravière, expressed regret that politics had diverted M. Paul Bert from physiology; and M. Vulpian remarked that his death, though glorious for the country, was a calamity for science, his numerous memoirs having placed him among the first physiologists of the age. The Academy adjourned in sign of mourning.

THE RECENT WEATHER

AT the close of a short period of somewhat unusual weather conditions, it may be worth while to call attention to the more prominent features of those conditions.

Cyclonic systems, some of wide, some of small dimensions, have been primarily developed over Western Europe in unusually large numbers. Opportunities for studying those atmospheric conditions from which barometric depressions originate within the area of our European stations are by no means very rare, but they are nevertheless sufficiently scarce to merit careful scrutiny at the hands of every student of weather knowledge. So much is this the case that a meteorologist of eminence made, some years since, the statement that no one had ever been present at the birth of a storm.

Considering the disastrous nature of the floods, the sloppiness of earth and sky, and the general misery in the aspect of things, which characterise the event, few of us can wish to be very frequently spectators of it. But when it occurs, the conditions accompanying it should be carefully attended to. These may perhaps be briefly summarised thus:—

(1) Barometric depressions are primarily developed over a region where atmospheric gradients are slight, the exceptions to this rule being those systems (secondary or subsidiary, as they are termed) which first appear as loops or bulges in the isobars of a large pre-existing cyclone.

(2) They originate either in the rear of a depression which has already passed away or in the inter-space between two large anticyclones, and more especially when the anticyclones are so large that this inter-space constitutes what is called a "trough" of relatively low pressure.

(3) They are preceded and accompanied by an enormous condensation of vapour into cloud.

(4) They do not, at the moment of their birth, appear

to affect the upper currents of the atmosphere, but, if growing large, soon afterwards do so, in such a manner that the hypothetical isobars at the level of the cirrus appear to be bent into a V-shaped hollow from the great polar depression to a point nearly above the centre of the circular depression at the earth's surface.

Of the depressions lately developed near the British Isles and over the south-west of France, the greater number originated in "troughs" of relatively low pressure, such as have been above alluded to: and their movements may be said to have been unusually erratic. Yet they obeyed the ordinary rule of progression, in so travelling, as to have the highest general pressures on the right of their course. Thus, those depressions which originated near the east side of a "trough" lying north-north-west to south-south-east, tended to move to north-north-west, while those which originated on the other side of the "trough" travelled in the contrary direction. On the 10th inst., Great Britain lay between two depressions travelling in parallel but opposite directions, these directions being transverse to the mean direction of movement prevalent at this season of the year. The same phenomenon was repeated on the 12th inst. It is now more than twenty years since I began constructing daily charts of the directions taken by European depressions, and during the whole of that period there has been no instance entirely comparable with these.

We must wait for reports from a very extensive portion of the earth's surface before a comprehensive study of these phenomena can be undertaken. Two questions, so correlated that they may require but a single answer, are of prime importance:—What causes the abnormal, but temporarily persistent, determination of aqueous vapour to certain portions of the globe? What causes the abnormal, but temporarily persistent, occupation of certain portions of the globe by anticyclonic systems?

W. CLEMENT LEY

THE WORK OF THE UNITED STATES FISH COMMISSION[1]

A BRIEF memorandum of what the U.S. Fish Commission hopes to accomplish in time, in connection with its mission, is as follows:—

(1) In the department of investigation and research there is yet to be carried out an exhaustive inquiry into the character, abundance, geographical distribution, and economical qualities of the inhabitants of the waters, both fresh and salt. The subject is practically unlimited in extent, and, so far as the ocean is concerned, has scarcely been touched. With the powerful apparatus, however, at the command of the Commission it is expected that much progress will be made year by year, and that the publication of the results and the distribution of duplicate specimens to colleges and academies in the United States will be carried out on a large scale, so as to meet a large and increasing demand from teachers and students.

(2) A second object, in connection with the sea fisheries, is the improvement of the old methods and apparatus of fishing and the introduction of new ones.

The work of the Commission in bringing to the notice of American fishermen the importance of gill-nets with glass-ball floats for the capture of codfish has already revolutionised the winter cod-fishery industry in New England. Looked upon almost with ridicule by the Gloucester fishermen, when first brought to their notice by the Commission, these nets have come rapidly into use, until at the present time they represent the most important element in the winter fisheries, the number of fish taken being not only much greater than heretofore but the fish themselves of finer quality.

[1] From the "Report of the U.S. Commission of Fish and Fisheries" (Washington, 1885).

The ability to maintain a successful fishery without the use of bait is of the utmost importance, in view of the fact that when the cod are most abundant bait is almost unprocurable. Other forms of apparatus of less importance have also been introduced, and a constant look-out is maintained, by correspondence and otherwise, in connection with the improvement of fishing machinery.

(3) Another important point for consideration is that of improvement in the pattern of fishing vessels. There is annually a terrible mortality in the fishing crews of New England, especially those belonging to the port of Gloucester, to say nothing of the total loss and wreck of the fishing vessels and their contents. There has gradually developed in connection with the mackerel and cod fisheries of New England a pattern of vessel which, while admirable for speed and beauty of lines and of rig, is less safe under certain emergencies than the more substantial and deeper vessel used abroad, especially in England and Scotland.

The subject of the best form of fishing vessel has been intrusted to Captain Collins, of the Commission, himself a most experienced fisherman, and, after a careful study of the boats of all nations, he has prepared a model which is believed to combine the excellences of both English and American vessels.

An appropriation will be asked from Congress for means to construct an experimental vessel and test its qualities; but until a successful experiment has been made it will be difficult to induce the fishermen to change their present form of construction.

(4) The fourth object of the Commission is to determine the extent and general character of the old fishing localities and to discover new ones. There is no doubt whatever that there still remain many important areas, even in the best-known seas, where the codfish and the halibut will be found in their former abundance. There has never been any formal investigation on this subject, and the banks that are known have been brought to light purely by accident. It is believed that by a systematic research and a careful survey the area of known grounds can be greatly extended.

There is very great reason to hope for successful results from this inquiry in the waters off the South Atlantic coast and in the Gulf of Mexico. These regions, the latter especially, may be considered as practically unknown, the few established localities for good fishing being in very small proportion to what must exist. It is here that the service of the fishing schooner referred to above, if means can be obtained to build it, will be brought into play, and it is not too much to hope that an industry will be developed that will represent to the Southern and South-Western States the same source of income and occupation that the mackerel, cod, and halibut furnish to the fishermen of New England.

(5) There is also much to be learned in the way of curing and packing fish for general and special markets. The American methods have grown up as a matter of routine, and are adapted to only one class of demand. There are, however, many modes of preparation which can be made use of to meet the wants of new markets; and thus we can enter more efficiently into competition with European nations for European trade, as well as for that of the West Indies and South America.

A great advance has already been made towards this desired improvement since the Centennial Exhibition of 1876, where many methods of curing and putting up fish were shown in the foreign sections that were almost entirely unknown in America. Notable among these were the preparations of sardines and other species of herring in oil, as well as in spiced juices. Quite recently this industry has been well established in Maine, amounting to a value of millions of dollars, and there are many other parts of the country where the same work can be done with other kinds of fish. The whole subject is receiving the careful consideration of the Commission, and numerous facts bearing upon it have been announced in its reports and bulletins.

(6) The work of increasing the supply of valuable fishes and other aquatic forms in the waters of the United States, whether by artificial propagation or by transplantation, although very successful, may be considered as yet in its infancy.

It must be remembered that the agencies which have tended to diminish the abundance of the fish have been at work for many years and are increasing in an enormous ratio. This, taken in connection with the rapid multiplication of the population of the United States, makes the work an extremely difficult one. If the general conditions remained the same as they were fifty years ago, it would be a very simple thing to restore the former equilibrium.

At that time, it must be remembered, the methods of preservation and of wholesale transfer, by means of ice, were not known, while the means of quick transportation were very limited. Hence a small number of fish supplied fully the demand, with the exception, of course, of species that were salted down, like the cod, the mackerel, and the herring (including the shad). Now, however, the conditions are entirely changed. The whole country participates in the benefits of a large capture of fish, and there is no danger of glutting the market, since any surplus can be immediately frozen and shipped to a distance, or held until the occurrence of a renewed demand.

Another impediment to the rapid accomplishment of the desired result is the absence of concurrent protective legislation of a sufficiently stringent character to prevent unnecessary waste of the fish during the critical period of spawning, and the erection or maintenance of impediments to their movements in reaching the spawning-grounds. This is especially the case with the shad and the salmon, where the simple construction of an impassable dam, or the erection of a factory discharging its poisonous waste into the water, may in a few years entirely exterminate a successful and valuable fishery.

It is to be hoped that public opinion will be gradually led up to the necessity of action of the kind referred to, and that year by year a continued increase in the fisheries will be manifested. Even if this does not occur as rapidly as some may hope, the experiments so far furnish the strongest arguments in favour of continuing the work for a reasonable time. A diminution that has been going on for fifty or more years is not to be overcome in ten, in view of the increasing obstacles already referred to.

Among the species an increase of which in their appropriate places and seasons is to be hoped for, in addition to those now occupying the attention of fish-culturists, are the cod, the halibut, the common mackerel, the Spanish mackerel, the striped bass, or rockfish, &c.

One of the most important, and at the same time among the most promising, fish is the California trout, with which it is hoped to stock large areas of the country. Its special commendations are mentioned elsewhere in this Report.

Another fishery earnestly calling for assistance, and capable of receiving it, is that of the lobster, the decrease of which has been very marked. The experiments of the Fish Commission suggest methods by which the number can be greatly increased. Something, too, may be done with the common crab of the Atlantic coast and its transfer to the Pacific. Some kinds might also be advantageously brought to the eastern portion of the United States from the Pacific coast and from the European seas.

A subject of as much importance as any other that now occupies the attention of the Fish Commission is an increase in the supply of oysters. In no department of the American fisheries has there been so rapid and alarming a decrease, and the boasted abundance of this mollusk on the Atlantic coast, especially in Chesapeake Bay, is rapidly being changed to a condition of scarcity which threatens practical extermination, as is almost the case in England A fishing industry producing millions of dollars is menaced with extinction, and needs the most stringent measures for its protection.

The U.S. Fish Commission has been very fortunate, through its agents and assistants, in making important discoveries in connection with the propagation of the oyster, which are referred to hereafter in this Report; and it is proposed to establish several experimental stations for applying the discoveries thus made, so as to constitute a school of instruction and information to persons practically engaged in the business.

There are other shell-fish besides the oyster that will well repay the trouble of transplantation and multiplication. Among these are several species of clams belonging to the Pacific coast of the United States, which are much superior in size, in tenderness, and in excellence of flavour to those on the eastern coast. Most of these are natives of Puget Sound, and the completion of the Northern Pacific Railway is looked forward to as a convenient means of transferring them to Eastern waters. The common clams of the Atlantic coast are also fair subjects of experiment.

VOLCANIC DUST FROM NEW ZEALAND

A SHORT time since, Sir Julius von Haast sent me a small packet of volcanic dust from New Zealand, and requested me to examine it. The dust fell on June 10 of the present year (the day of the Tarawera eruption) at Matakava, Hicks Bay, 115 miles from the scene of the eruption. This dust is very fine, and, when regarded in the mass, is a dull, darkish gray colour. When examined under the microscope, it may be divided into—

(*a*) Bits of a more or less scoriaceous aspect—tiny lapilli, commonly almost opaque, being only translucent on thin edges—consisting of a somewhat brownish glass containing much disseminated ferrite. With reflected light they are a light to a darkish gray in colour, sometimes slightly reddish or brownish, with moderately rough surfaces. In size they usually vary from about ·005 to ·008 inch in the longest diameter; the former being the more common measurement; the latter is but rarely exceeded, the largest fragment in the portion which I have examined being ·012 inch in diameter; lapilli also occur of less than ·005 inch.

(*b*) Chips, more or less transparent, generally not exceeding ·005 inch in diameter, and of all dimensions downwards to the finest dust. The majority of these chips are glass, commonly quite colourless; some of them contain bubbles, spherical, spheroidal, or more or less cylindrical. Sometimes these are quite ·001 inch in longest diameter. Many chips show a ridgy surface, and are evidently formed by the destruction of a very frothy pumice like that of Krakatão. Some of the glass is of a light brown colour; occasionally it contains microliths of feldspar or trichites. The mineral chips are much less numerous than those of glass; the great majority of them are feldspar. Many of these are flat flakes apparently detached from a basal plane, but a few exhibit twinning. Some may be sanidine, but a plagioclastic feldspar is certainly present. The chips, however, are ill-suited for optical measurements, and the results which I have obtained are rather discordant. So far as I can come to a conclusion, I should say that the extinction-angles seem to indicate the not unfrequent presence of a feldspar which belongs rather to the oligoclase-albite group than to the labradorite-anorthite. I find very few indications of the presence of a pyroxenic constituent. One or two fragments are a greenish hornblende; three or four in general appearance resemble small flakes of magnesia-iron mica—lying on their basal faces, but some of these show dichroism, and only extinguish in certain positions between crossed Nicols, so that they cannot be this mineral. As to their true nature I have not yet been able to decide; however, I think it probable some of the brown flakes are mica.

The result of my examination leads me to conclude that the dust is formed of material which was a glass wherein a porphyritic structure, on a large or a minute scale, was inconspicuous. This Matakava dust appears to agree generally with, though it differs varietally from, that described by Prof. Joly in NATURE (vol. xxxiv. p. 595), the main difference being that the biotite, which seems rather common in his samples, is rare in, if not absent from, the present one. I have not noticed sulphur, pyrite, or magnetite in a recognisable form.

It may be interesting to compare this dust with some samples projected from Cotopaxi, and described by myself (*Proc. Roy. Soc.* No. 231). These specimens came from various distances, ranging from twenty to sixty-five miles from the volcano. That which fell on the summit of Chimborazo (the most distant locality) consists of lapilli and chips; the majority of the grains range from about ·001 to ·003 inch; a very few only attain to a diameter of ·01 inch, and this is barely exceeded. In this dust, however, the lapilli are comparatively rare, the chips of glass and mineral dominating, with a fair proportion of the latter. A reference to the above paper will show the difference between this ash, that from Krakatão, and the above-described from New Zealand. This may be explained by the fact that a porphyritic structure is common in the lavas of Cotopaxi (as in the other summits of the district).

T. G. BONNEY

NOTES

WE have to record the death of General John Theophilus Beaulieu, F.R.S., at the age of eighty-one years. He entered the Indian Army in 1820 as a lieutenant in the Bengal Engineers, and was for some time Superintending Engineer in the Public Works Department for the N.W. Provinces. Among other services to science and to India, General Beaulieu inaugurated the system of magnetic observations in India, and was the author of a book of logarithms of wide reputation. General Beaulieu was elected a Fellow of the Royal Society fifty years ago, and has served on its Council.

THE death is announced, at Berlin, of Dr. A. Fischer, so long resident at Zanzibar, and who has done so much for the exploration of the Kilimanjaro region.

M. CHANCOURTOIS, General Inspector of Mines in France, author of several works on geology, and Professor in the School of Mines, has died suddenly at Paris at the age of sixty-seven.

THE following are the probable arrangements for the meetings of the Society of Arts before Christmas:—"November 24, William Anderson, M.Inst.C.E., "Purification of Water by Agitation with Iron and by Sand Filtration." December 1, adjourned discussion on the paper by Dr. C. Meymott Tidy, on "Sewage Disposal" (read April 14, 1886). December 8, Major-General C. E. Webber, R.E., C.B., "Glow-Lamps, their Use and Manufacture." December 15, J. B. Marsh, "Cameo-cutting as an Occupation." There will be five courses of Cantor Lectures during the session:—(1) "Principles and Practice of Ornamental Design," by Lewis Foreman Day. (2) "Diseases of Plants, with special reference to Agriculture and Forestry," by T. L. W. Thudichum, M.D. (3) "Building Materials," by W. Y. Dent, F.C.S., F.I.C. (4) "Machines for Testing Materials, especially Iron and Steel," by Prof. W. C. Unwin. (5) "The Structure of Textile Fibres," by Dr. Frederick H. Bowman, F.L.S., F.G.S. Two Juvenile Lectures on "Soap Bubbles," by A. W. Reinold, F.R.S., will be given on Wednesday evenings, January 5 and 12, 1887. The meetings of the Colonial Section and of the Indian Section will not commence till after Christmas.

THE General Committee of the Society for the Prevention of Hydrophobia and Reform of the Dog Laws held its fourth meeting on Friday last at the offices, 50, Leicester Square, London, W.C., to consider a programme which had been drafted by a sub-committee and circulated among members and supporters. Mr. Victor Horsley, B.S., F.R.S., Secretary to the Commission on Hydrophobia, attended this meeting by invitation, and was unanimously elected a Member of Committee. Many letters were read expressing approval of the programme and regretting that the writers were unable to attend. Among the gentlemen who thus wrote were Dr. Drysdale, Prof. Fleming, Dr. Norman Kerr, Prof. E. Ray Lankester, Mr. Arthur Nicols, and Prof. Pritchard. The Honorary Secretary having made a financial statement of a satisfactory nature, the Chairman, Colonel R. H. Rosser, briefly explained the care and time given by the sub-committee to the programme, which was then discussed in detail, and ordered to be printed with some additions and alterations.

IT is intended, in Section III. of the Manchester Jubilee Exhibition, to exhibit the historical and modern methods adopted in the several branches of electro-metallurgy, such as gold, silver, platinum, nickel and copper plating, the purification of metals by electrolysis, and generally to illustrate the connection between electricity and chemistry.

INFORMATION has been received by the Board of Trade respecting the oyster fisheries of the Isle of Wight. The oyster grounds and breeding ponds of the Isle of Wight are as follows:—(1) Medina River; (2) Brading Harbour; (3) Newtown; (4) Fishhouse or Fishbourne. In 1867 the Isle of Wight Oyster Fishery Company was started, having the Medina River and Newtown Creek for its grounds. This oyster fishery is said to have done well until 1871, when it was troubled by refuse and sewage discharged by mills at Newport, and a large quantity of the broods were destroyed. The Medina fishery is now for sale. About the year 1873 Major Boyle started a system for breeding oysters at Brading, but in 1876 the harbour works were commenced, and Major Boyle had to relinquish his ponds. The Harbour Board, however, still carry on the oyster breeding at Brading. They have six or seven ponds near St. Helens, which are estimated to contain five or six millions of oysters at the present time. As Brading is not suitable for fattening, the young oysters are sold to various growers to be laid down for one or two years to render them fit for food. Newtown Creek is the best fattening ground in the Isle of Wight, but the fishery company is now in liquidation through want of funds, and the business is in abeyance. At Fishhouse there has been a good fall of spat this last summer, but the ground is much disturbed by barges, which prevents the fishery from being fully developed. The oyster fishery is consequently not very prosperous. The creek, it may be mentioned, was recently cleared, and some sixty or seventy thousand oysters were transferred to Newtown to fatten in a pond placed at their disposal by the Newtown Company. Besides the oyster fishery companies above referred to, there are several fishermen who dredge for oysters at sea in the Solent, and particularly in Osborne Bay. The oysters dredged up are seldom fit for the market, and have usually been sold to one of the companies to be laid down and fattened.

IN Dr. B. W. Richardson's recent Cantor Lectures on "Animal Mechanics," speaking of the mechanism of the heart, he described the number of the pulsations of the heart in different animals—in fish, frog, bird, rabbit, cat, dog, sheep, horse—and made a few comments on the remarkable slowness of the heart—40 strokes per minute—in the horse. Then the number of pulsations in man at various periods of life, and at different levels, from the level of the sea up to 4000 feet above sea-level, was brought under review, and was followed by a computation of the average work performed by the heart in a healthy adult man. The work was traced out by the minute, the hour, and the day, and was shown to equal the feat of raising 5 tons 4 cwt. one foot per hour, or 125 tons in twenty-four hours. The excess of this work under alcohol in varying quantities formed a corollary to the history of the work of the heart, Parkes's calculation showing an excess of 24 foot-tons from the imbibition of eight fluid ounces of alcohol. The facts relating to the work of the heart by the weight of work accomplished was supplemented by a new calculation, in which the course of calculation was explained by mileage. Presuming that the blood was thrown out of the heart at each pulsation in the proportion of 69 strokes per minute, and at the assumed force of 9 feet, the mileage of the blood through the body might be taken at 207 yards per minute, 7 miles per hour, 168 miles per day, 61,320 miles per year, or 5,150,880 miles in a lifetime of eighty-four years. The number of *beats* of the heart in the same long life would reach the grand total of 2,869,776,000.

A METEOR of unusual splendour was seen from the Oxford Road, Banbury, on Tuesday, November 2, at about 8.5 p.m. The fall became visible at about mid-distance between zenith and horizon in a direction west by north. At first the meteor burned with a faint, apparently reddish, light, but when a third of its path had been passed, it burst into an intense blue flame, and, increasing in brilliancy during the next third of its course, it finally died away before reaching the horizon, leaving behind it a long red trail distinguishable for several seconds afterwards. The fall was at an angle of 60°, and during the middle third of the flight the flame was of such intensity as to light up the surrounding country.

REFERRING to a paper at the Paris Geographical Society by Dr. Hamy on "The Interpretation of one of the Monuments at Copan, Honduras," in which an inference is drawn as to early Chinese intercourse with America, Dr. W. H. Dall, writing to *Science*, states his belief that the very wide hypothesis thus broached, and which in one form or another has had a certain currency for more than a century, rests upon a totally insufficient foundation. That wrecked Japanese, and possibly Chinese, from time to time were cast on the shores of America, is beyond question. But there is every reason to believe that the wrecked people were (1) nearly always males, and incapable of colonising; (2) were either killed or enslaved by the Americans in accordance with a general usage; and (3) that neither in arts nor language have they left any appreciable trace on American anthropology. "The statement of Brooks, that the Japanese and Aleuts could communicate without an interpreter, is true to this extent. I was present when the aforesaid Japanese, three males, were brought to the port of Unala-hka, and took pains to inquire into the assertion which was made to me at the time. I found that the communications were wholly by signs, and not by spoken language, as the Aleuts could not understand a word of Japanese without its accompanying signs. Secondly, Brooks, who was long Consul in Japan, informed me that he had particularly searched into the matter of the voyage to Fu-sang, and that he had conclusive evidence that the voyage which actually took place was to the well-known and still existing province of Fusang in Korea (see Griffis' work), and had no connection whatever with America. Lastly, the mere presence of two simple curved lines on a circular stone, taken by itself, proves nothing as to their meaning, and still less that they had any connection with the Chinese symbol." Dr. Dall concludes by stating that such unbridled hypotheses are the "curse of anthropology."

WE have received a fresh instalment of the very valuable work emanating from the firm of Eduard Trewendt, Breslau, the now well-known "Encyclopædia of Natural Sciences," namely, Nos. 48 and 49 of the first part, and 37 and 38 of the second part. These four numbers deal with three different branches of science. No. 48 of the first part is botanical, a continuation of Drude's masterly work, "Systematic and Geographical Arrangement of the Phanerogams," copiously illustrated with woodcuts, as was the earlier portion of the same publication. No. 49 of the "Alphabetical Manual of Zoology, Anthropology, and Ethnology," takes the reader from "Landschaf" to "Lithodina." Of the abundant material embraced in this compass may be particularly mentioned "Landschnecken," by E. von Martens; "Larven," by Dr. Griesbach; two especially interesting contributions, "Larynx," by von Mojsi ovics, and "La Tène-Zeit," by Mehlis; treatises by Jaeger on "Life, and its Conditions, Phenomena, and Stimulations, &c.," as also "Linsen," and "Lippen," by Dewitz. Nos. 18 and 19 of the second part belong to the "Alphabetical Manual of Chemistry," containing the papers—Furfurangruppe (conclusion), Gährung, Galle, Gallium, Gehirn, Gerberei, Gerbsäuren or Gerbstoffe, Germanium, Glas, Glycerin, Glycidsäuren, Glycocoll, Glycoside. As of most general interest in this list of articles may be specially cited "Glycoside," by Prof. Oskar Jacobsen, of which the firm is preparing a separate impression; as also "Gährung," by Tollens; and "Glas," by Engler. The newly-discovered "Germanium" is treated by the editor, Herr Ladenburg himself. In Prof. Liebrich's contribution, "Gehirn," we have a valuable paper on this subject by a recognised authority.

TWO remarkable specimens of deformed fish were taken from a rearing-pond at Delaford this week, and brought to the South Kensington Aquarium. One is a trout about three years old, whose tail is bent to such an extreme that it stands at right angles with the body of the fish. Its mode of progress is laboured, and its appearance is very peculiar. The other specimen is an ordinary stickleback, measuring 4 inches in length, whose body is swollen through dropsy to the extent of 1 inch in diameter. At first sight its appearance is similar to a young mouse, and it requires close inspection to grasp the fact that it is a fish. It moves very slowly, with great expenditure of force, the weight of the contorted body being considerable.

THERE has been a great demand for the German carp lately imported into this country by the Marquess of Exeter and others for the purpose of acclimatisation in this country, and a numerously expressed desire to stock waters that are useless for the purpose of maintaining other fish than carp has been made for the German species.

THE operations in connection with the new Observatory and Marine Fish-Culture Station at Lochbuie, Isle of Mull, to be established under the auspices of the National Fish-Culture Association, are to be commenced forthwith. At a meeting of the Council, held last week, it was arranged to form ponds for the propagation of lobsters, and to make certain observations upon marine fauna. The details of the scheme were placed in the hands of a special scientific committee.

WE have received from Messrs. Horne, Thornthwaite, and Wood, opticians, a map of the moon, 12 inches in diameter, mounted on good stout cardboard, represented as she appears when viewed through a telescope with an astronomical eyepiece. This will be a very useful companion and guide for those amateurs who are studying the lunar surface; it can be conveniently held in the hand while observations are being made. About 300 craters and walled plains are marked very clearly, and the names of the different seas are given in larger letters. At the back is added a short description of some of the chief features.

A SHOCK of earthquake was felt in the district of Beira Alta on the 11th inst.

THE additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus patas*), from West Africa, presented by Mr. Thomas Baily; a Yellow Baboon (*Cynocephalus babouin*), from West Africa, presented by Capt. J. Henderson Smith, R.A.; two Goshawks (*Astur palumbarius*), European, presented by the Baron d'Epremesnil; a Hobby (*Falco subbuteo*), caught in the Indian Ocean, presented by Dr. Rivis Mead; two Java Sparrows (*Padda oryzivora*) from Java, four St. Helena Seed-Eaters (*Crithagra butyracea*), from South Africa, presented by Mrs. Conrad Pile; two Sing-sing Antelopes (*Cobus sing-sing* ♂ ♀), from West Africa, received in exchange; a Woodcock (*Scolopax rusticula*), European, purchased; an Ocelot (*Felis pardalis*) from America, a Bactrian Camel (*Camelus bactrianus* ♀) from Central Asia, two White-backed Piping Crows (*Gymnorhina leuconota*) from South Australia; a Banded Parrakeet (*Palæornis fasciatus* ♂), from India, deposited; a Vinaceous Turtle Dove (*Turtus vinaceus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE INFLUENCE OF ASTIGMATISM IN THE EYE ON ASTRONOMICAL OBSERVATIONS.—Prof. Seeliger has published, in the *Abhandlungen der k. bayer Akademie der Wiss.*, ii. Cl., xv. Bd., 3 Abth., an interesting paper on this subject. The paper is divided into four sections. The first part treats of certain details connected with the refraction of light which are used in the subsequent investigations. The second part gives the theory of the formation of images in an astigmatic eye, and its application to measures made with an altitude instrument. In the third and fourth parts the author treats of the application of his theory to the heliometer and wire-micrometer respectively. It appears, from Prof. Seeliger's researches, that this malformation in the eye, which is far from uncommon, exerts a larger influence on astronomical measurements than is commonly supposed. Thus, he shows that a systematic error in a series of observed declinations amounting to 0″·26 may very well be due to it. And it appears that the discordances in observed position-angles of double stars, depending on the inclination of the line joining the components to the vertical, with which the measures of some observers are affected, may in part be referred to the same cause. Prof. Seeliger's paper is one which may be profitably studied by those who aspire to the attainment of greater accuracy in astronomical observations.

THE KALOCSA OBSERVATORY.—Dr. C. Braun has recently published a report of this Observatory, founded by Cardinal Haynald, Archbishop of Kalocsa. The equipment of the Observatory consists of a refractor, by Merz, of 7 inches aperture; another of 4 inches; a transit by Cooke, aperture 2·3 inches; an altazimuth by Breithaupt, of Cassel; a chronograph, three clocks, and a chronometer; several spectroscopes, of which a large solar spectroscope with automatic adjustment to minimum deviation is the principal; a star photometer by Zöllner, and a spectro photometer by Vogel and Glan. The two most important works effected at the Observatory have been the determination of the geographical position of the Observatory, and the observation of sunspots. A special value attaches to the former, as hitherto the position of no place in Hungary had been fixed by direct astronomical methods. The latitude of the standard pillar of the Observatory was determined by geodetic observations to be 46° 31′ 41″·92; the astronomical methods made it 0″·07 greater. The longitude was found to be 1h. 15m. 54·343s. east of Greenwich. The observations of sunspots extend from May 14, 1880, to January 31, 1884, and form a useful record of an interesting period. The method of projection was employed in observing; the observations were reduced first by means of a projection of the sun, and secondly by calculation. In the latter method Dr. Braun employed an instrument of his own device, which he terms a trigonometer, for the direct solution of spherical triangles. From his observations Dr. Braun deduces the following expression for the velocity of rotation:—$865°·33 - 209°·86 \sin^2 \lambda$. He also shows the downward tendency in latitude of the mean spotted area, and points out the curious partial effort at recovery which shows itself at tolerably regular intervals. The observations of each rotation are grouped together and given in short tables, and diagrams similar to Carrington's, showing the spots of each rotation in shape and position, are also added. The volume concludes with full descriptions of a number of ingenious instrumental devices, some actually employed at Kalocsa, others still only projected. Amongst these is an ingenious transit micrometer for eliminating personal equation in the observation of transits.

ζ CASSIOPEIÆ.—The *Sidereal Messenger* reports, on the authority of Prof. Colbert of Chicago, that this star appeared to increase its brilliancy by quite half a magnitude on the night of August 20. The most remarkable point of the observation was the shortness of duration of the phenomenon: for, about half an hour after it was first noticed, the star began to return to its normal magnitude. It will be interesting to learn if the change was observed elsewhere.

NEW MINOR PLANETS.—Prof. Peters discovered a new minor planet, No. 261, on October 31, and Herr Palisa two—Nos. 262 and 263—on November 3.

COMET FINLAY.—The following ephemeris for Berlin midnight is in continuation of that given in NATURE for November 4 (p. 17):—

1886	R.A. h. m. s.	Decl. ° ′	log r	log Δ
Nov. 16	19 59 51	23 22·7 S.	0·0697	0·0899
18	20 8 42	22 53·7		
20	20 17 38	22 22·4	0·0589	0·0874
22	20 26 37	21 48·8		
24	20 35 39	21 13·0 S.	0·0693	0·0856

COMET BARNARD.—The following ephemeris for Berlin midnight is given by Dr. E. Lamp (*Astr. Nachr.*, No. 2753):—

1886	R.A. h. m. s.	Decl. ° ′	log r	log Δ	Brightness
Nov. 18	13 16 59	13 12·4 N.	9·9433	0·0637	10·8
20	13 31 51	14 5·9	9·9306	0·0485	12·3
22	13 47 57	14 58·3	9·9180	0·0340	13·9
24	14 5 20	15 47·7	9·9055	0·0207	15·7
26	14 24 2	16 32·5	9·8934	0·0089	17·5
28	14 43 57	17 10·3	9·8817	9·9990	19·3

The brightness at the time of discovery is taken as unity.

GOULD'S "ASTRONOMICAL JOURNAL."—The first number of the new issue of this journal appeared on November 2. It contains the following papers:—On the light-variations of Sawyer's variable in Vulpecula, by S. C. Chandler, Jun., in which the elements of the star are given as Max. = 1885 Nov. 2d. 20h. 35m. G.M.T + (4d. 10h. 29·0m.) E. The minimum is 1·060d. earlier. The rapidity of the rise is a striking characteristic of this star.—A new short-period variable, by E. F. Sawyer. The star, 57 Sagittarii, has a period of not more than 6 days; the variation is from 5·6 mag. to 6·6. Place for 1875·0, R.A. 18h. 14m. 2s.; Decl. 18° 54′·8.—Elements and ephemerides and observations of Comets Finlay and Barnard, by Profs. Winlock, Boss, and Frisby.—Observations of U Ophiuchi, by E. F. Sawyer; and the first part of a paper on the lunar theory, by Prof. Stockwell.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 *NOVEMBER* 21–27

(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 November 21

Sun rises, 7h. 31m.; souths, 11h. 46m. 3·1s.; sets, 16h. 1m.; decl. on meridian, 19° 58′ S.: Sidereal Time at Sunset, 20h. 3m.

Moon (New on November 25) rises, 1h. 37m.; souths, 8h. 4m.; sets, 14h. 18m.; decl. on meridian, 1° 57′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	9 30	13 11	16 52	25 2 S.
Venus	7 12	11 35	15 58	18 46 S.
Mars	10 33	14 18	18 3	24 32 S.
Jupiter	4 15	9 34	14 53	8 45 S.
Saturn	19 34*	3 36	11 38	21 22 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich)

Nov.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image	
			h. m.	h. m.	°	°
22	46 Virginis	6	4 42	5 31	72	176
22	*k* Virginis	6	4 58	near approach	304	—
22	48 Virginis	6	6 36	7 8	104	163
23	B.A.C. 4647	6	4 42	5 40	24	222

Nov.	h.	
23	1	Jupiter in conjunction with and 3° 0′ south of the Moon.
23	14	Mercury stationary.

Variable Stars

Star	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·2	81 16 N.	Nov. 28,	2 27 *m*
ζ Geminorum	6 57·4	20 44 N.	,, 23,	21 30 *M*
S Canis Minoris	7 26·5	8 34 N.	,, 24,	*M*
S Sagittarii	19 12·8	19 14 S.	,, 21,	*M*
η Aquilæ	19 46·7	0 43 N.	,, 24,	0 0 *M*

M signifies maximum ; *m* minimum.

Meteor Showers

The *Andromedes*, maximum November 27, R.A. 24°, Decl. 44° N., form the most interesting shower of the week. A radiant near μ Persei, R.A. 60°, Decl. 49° N., supplies very swift meteors ; swift meteors are likewise seen from a radiant near η Ursæ Majoris, R.A. 208°, Decl. 43° N.

GEOGRAPHICAL NOTES

THE *Bulletin* of the Paris Geographical Society for the present year (Nos. 1 and 2) contain several papers of interest. We need do no more than mention M. Ch. Mannoir's annual report on the progress of geography during the past year, which fills 130 pages. M. Grandidier writes on the rivers and lagoons of part of the east coast of Madagascar, and M. Gouin, of Nam-dinh, contributes a long paper on Tonquin, which deals with the commercial geography of the country rather than with the geography proper. No. 2 opens with the report of a strong Committee of the Society on the orthography of geographical names, which will be read with interest. No elaborate or exhaustive reforms are proposed ; the suggestions are rather a series of simple modifications "based on good sense rather than on high philological science, which is only accessible to the few initiated." The Committee take up the programme of the Royal Geographical Society, "completing it in some respects, and making some additions sensible to musical ears." The bases of the proposals are the same as those of our own Society : (1) not to seek an absolute perfection in the representation of sounds ; (2) to preserve in European names the form of the country of their origin ; (3) to retain in the case of other places the mode commonly employed. M. Rolland contributes a long paper on the hydrography and orography of the Algerian Sahara ; and M. Marteil examines the map of the French establishments on the Senegal recently issued by the Ministry of Marine. Lieut. Baudens describes a trip which he made last year along the Black River of Tonquin ; and finally there is an account written by Dr. Potagos in 1880 of a journey which he made in the Pamir in 1870, including a visit to the famous Yakub Beg of Kashgar.

WE have received the *Verhandlungen* (Bd. xiii. No. 8) and the *Zeitschrift* (Bd. xxi. Nos. 4 and 5) of the Geographical Society of Berlin. The first contains only one paper, but that an interesting one, by Dr. Sievers, on a recent journey in the Sierra Nevada de Santa Marta, in Columbia. The object of the journey was to study the geology and physical geography of the region, and especially to ascertain whether these mountains belong to the system of the Andes or not. As Dr. Sievers has only been back for a short time, he was unable to give any definite results, and he confines himself to describing the course of the journey, and to mentioning important points necessary for a proper understanding of the physical geography of the region. In the *Zeitschrift*, Herr Jung continues and concludes his analysis of the Indian census of 1881 ; this is followed by a translation, from the *Proceedings* of the Russian Geographical Society, of Dr. Iwanow's paper on certain ancient monuments discovered by him in the course of a geological examination of Turkestan. Prof. Gelcich has a highly technical history of the methods of ascertaining the area of a country, and Dr. von Danckelmann one on the frequency of rain in the Indian Ocean. Herr Sandler makes a contribution to the history of cartography by giving an account of the life and works of Johann Baptista Homann, a geographer of the latter part of the seventeenth century. A curious map appended to this paper (which is of considerable length) shows, by means of white and red outlines, the world according to present cartography and according to Homann's maps. The number concludes with a short paper on the hot springs of Kamchatka.

IN a recent work on the geology and geography of Sumatra, M. Verbeek, a Dutch engineer, says that sixty-seven volcanoes are known to exist on that island. There may be more even than this, for parts of the north-west, which are covered with primæval forests, have never been penetrated. Two only of these are active, Merapi and Talang (or Soclau), the former being 2892, and the latter 2542 metres in height.

THE October issue of the *Bollettino* of the Italian Geographical Society contains an account of an excursion made during the summer by E. Modigliani to the rarely visited island of Nias, which lies some thirty miles from the west coast of Sumatra, a little north of the equator. The explorer spent two months in the place, but owing to local feuds was unable to penetrate beyond Fadoro, a large village near Telok Dalam Bay on the south side. The natives, apparently of Malayan or Indonesian stock, but speaking a language quite different from Malay, and by Crawfurd described as "a simple, mild, and primitive people," he found on the contrary to be fierce and treacherous savages, everywhere addicted to head-hunting. Their hostility was such, that he failed to make any botanical or zoological collections ; but fortunately secured eleven human skulls from the southern districts, which have been sent to the Anthropological Museum of Florence. No similar specimens appear to have hitherto reached Europe, nor are any found even in the Batavian collection. Head-hunting is taken so much as a matter of course, that on Sig. Modigliani offering to purchase some skulls, the rajah of Bavolovalani on the south coast quietly remarked that it would be rather an expensive business, as an expedition would have to be specially fitted out and sent to the hills to raid upon some neighbouring tribes and carry off the required number of heads. He had no idea of craniological specimens being collected except from the living subject. The interior of Nias still presents a promising field of exploration, never having been visited by any European travellers.

LIGHTHOUSE ILLUMINANTS[1]

II.

V.—*Range of Lights in Hazy Weather*

THE observations on this subject of the Trinity House Committee have served to confirm the conclusions announced by M. Allard in his "Mémoire sur l'intensité et la portée des phares," 1876, and in his more recent "Notes sur quelques objections relatives à l'emploi de la lumière électrique dans les phares." The Committee find that the gas and oil lights which are equal in clear weather are equal also in fogs ; that in rather dense fog the more powerful light had but little advantage over the less powerful, for example, "the triform electric appearing at 1500 feet, while the quadriform gas and triform oil showed up together a little before the observers reached 1400 feet," and that the electric light, while suffering, according to the photometric results, a somewhat greater loss in hazy weather than the flame lights, is "visible at a greater distance than the highest powers tried in gas or oil." Using M. Allard's formula, which appears to rest on well-established physical and physiological data, I have calculated the range in fogs of various degrees of thickness of some of the lights exhibited at the South Foreland. The range of a light, or the limit at which it is just lost or just picked up, is that limit at which its intensity is diminished by distance and haze to the minimum intensity perceptible by a good eye, such as the practised eyes of seamen are. M. Allard gives this minimum intensity, on the authority, of "des expériences qui ont été faites sur ce sujet au Champ de Mars," as that of 1/100 Carcel at a distance of one kilometre on a perfectly clear night. This corresponds to ⅓ candle at a distance of one nautical mile. When the air is not perfectly clear,

[1] Further Report of Mr. Vernon Harcourt to the Board of Trade on the Experimental Lights exhibited at the South Foreland. Continued from p. 46.

its degree of transparency may be expressed by stating the fraction of light which escapes obstruction in passing through a certain length. Of this fraction the same fraction escapes obstruction in passing through another equal length of air, and so forth. Thus, if this fraction is called a, and l is the intensity at any point of a beam of parallel rays, such as a beam of sunlight reflected from a plane mirror, after the beam has traversed a mile of hazy air its intensity is diminished to la, at two miles its intensity is diminished to la^2, and at a distance of d miles to la^d. But divergent light, such as is even the most condensed beam from a lighthouse, diminishes also as the square of the distance. Thus, if L is a lighthouse light whose intensity at one mile is La, its intensity at any number of miles, d, is $La^d \times \frac{1}{d^2}$, and when the combined effect of haze and distance is such that its intensity is only equal to that of ½ candle at one mile, at that point the light ceases to be visible. Thus it is possible to calculate for any particular degree of haze what will be the range of any given light. To give some examples:—In a moderate uniform haze such that a single 108-jet gas-burner, showing as a fixed light of about 14,000 candles, was lost at a distance of 10·9 miles, the same light shown in biform would be lost at 11·8 miles, while the corresponding triform and quadriform lights would be lost at 12·5 and 12·8 miles respectively. In a rather thicker haze, in which a single 108-jet gas-burner, showing as a revolving light of 60,000 candles, was visible up to 10 miles, but no further, the extreme range of the biform would be 10·73 miles, of the triform 11·16 miles, of the quadriform 11·48 miles. In still thicker haze the increase of range obtained by increasing the power of the lighthouse light becomes not only absolutely but relatively less.

Light shown	Range in Nautical Miles		
Single 108-jet, M. I. lens	2	1	0·5
Biform ,, ,,	2·11	1·05	0·52
Triform ,, ,,	2·17	1·08	0·54
Quadriform ,, ,,	2·22	1·1	0·55

The above results represent the maximum range of the direct beam through uniform haze of lights of the same kind but varying in power. But in certain cases the increase of range gained by increasing the power of the light may be either less or greater than it is in the foregoing case.

In the first place, the light which has suffered obstruction is diverted from its direct course but is not lost; and a portion of this light may reach the eye from a direction slightly different from that of the source of light, producing the impression of a halo or burr. Prof. Stokes, Pres. R.S., who has kindly given me much help in considering this subject, concludes that, especially in a fog in which the particles of water are not very minute, the burr might be seen at a substantially greater distance than that at which the direct light could be seen. "The intensity of this diffused light will not decrease in geometric progression as the distance from the source increases, but rather will tend ultimately to decrease inversely as the square of the distance; but being so widely spread, there will be danger of its being unperceived unless it be flashy." In this case the more powerful lights would retain in fog more nearly the advantage which they possess in clear weather, in which a fourfold light has double the range of a single light. But since the eye can distinguish between the point of light which is seen by the direct rays and the blurred nimbus, whose properties Prof. Stokes has investigated, the question whether the range of powerful lights is materially increased by the diverted and re-diverted light which surrounds the principal beam could be solved by an appeal to experience.

In answer to my inquiry whether it often happens that in approaching a lighthouse on a hazy night that which is first seen is an indistinct brightness or halo, not the light itself, and whether this effect is seen at considerable distances or only at short distances, and in what kind of fog, the Deputy Master of the Trinity House tells me that it happens, occasionally, at short ranges, in thick fog or mist, when nothing of the light is seen beyond 1 or 2 miles. "In clearer weather (*i.e.*, slight haze) this peculiarity is not observable at any range; it is the direct beam from the lantern (of course lessened and indistinct by reason of the density of the atmosphere) which then comes to the eye of the observer when approaching from seaward." I think, therefore, it may be concluded that at short distances powerful lights may occasionally have an advantage over feebler lights greater than is indicated by M. Allard's formula, in consequence of the scattered light being less diminished by fog than the direct light.

There are two other cases in which the formula is not immediately applicable; the second exception being, if the haze is not uniform. This case may be illustrated by taking an extreme example. Suppose that the amount of fog extending 5 miles from a lighthouse were just sufficient to extinguish a light of 60,000 candles, and that beyond this distance the air were perfectly clear, a light of four times the initial power would have at the margin of the fog four times the minimum visible brightness, and would only disappear altogether at a distance of 10 miles. But, on the other hand, if the fog were thicker further to seaward, the larger light would have scarcely any advantage over the smaller light.

Thirdly, if the lights compared differ in quality, of which the visible sign is colour, as well as in power. In this case the particles of water of which haze at sea consists (differing from the coloured particles of a London fog) are only likely to exercise a selective action on lights of different refrangibilities when the particles are so small as to be comparable with a wave of light. In a thick mist, in which the particles of water have often a visible magnitude, this effect is probably absent. Clouds are of this character, and sunlight is not reddened by passing through them. But the red colour of the sun when near the horizon, and the assimilation in colour of the electric light to gas-light when seen from a distance through slight atmospheric haze, shows that such haze does interfere with the more refrangible blue rays, to a greater extent than it does with the yellow, orange, and red rays. It is therefore certain that the electric light, which contains a relatively large proportion of the more refrangible rays, suffers a greater loss than the light from gas or oil flames in certain states of the atmosphere. The larger particles of mist or rain probably obstruct light of all sorts in the same degree. If we suppose that the effect of haze is to cut off all the blue and violet rays, the loss to the flame lights would not exceed 1 or 2 per cent., while that of the electric light, which is perhaps rather bluer than sunlight, may amount to 20 per cent. But this loss, though considerable, would not materially affect the range of the electric light in hazy weather.[1]

It has been claimed as an advantage of multiform lights, compared with the electric arc behind a small lens, that the larger surface of illuminated lens is more favourable to visibility in hazy weather; and the late Sir W. Siemens gave some countenance to this view. Speaking in the discussion of Sir James Douglass's paper "On the electric light applied to lighthouse illumination," 1879, he said:—"He had held that, in order to get more penetrating power, not intensity alone, but intensity with quantity as represented by large surface, would be required." M. Allard in his "Note," pp. 10 and 11, makes some interesting observations on this subject, but deals rather with visibility at great distances in clear weather than with visibility through haze. I do not know on what grounds, either of theory or observation, the opinion formed by Sir W. Siemens is based. Desiring further information on this and some other points, I have consulted Lord Rayleigh, Sec. R.S., who with Prof. Stokes joined me in a visit to the South Foreland lights a year ago. I may quote his opinion:—"With the same total brightness of source, and angle of divergence, it can make no difference at a distance (at which the apparent magnitude of the lens is inappreciable), whether the lens be large or small. At smaller distances the advantage might be with the smaller lens. So far as I see, the only advantage that the large lens could ever have would be more room for a bulky light, which, with a small lens, might give too great a divergence."

While referring to the assistance I have received from Prof. Stokes and Lord Rayleigh, to whom I desire to accord my thanks, I should mention that I have received from both the same emphatic suggestion, that further trial should be made of sudden flashes in fog. Lord Rayleigh writes: "I should like to see proper experiments tried on sudden and periodic flashes, such as might be produced by gunpowder, the periodicity serving for identification and the intermittence being necessary to

[1] Since this report was written, a paper has been communicated to the Royal Society by Captain Abney, R.E., F.R.S., and Major-General Festing, R.E., F.R.S., giving the result of measurements of the illuminating power of different parts of the spectrum of the electric arc. According to these measurements, the illuminating power of that part of the light from the electric arc which lies beyond the line "E" in the spectrum, including the greater part of the green rays as well as the blue and violet rays, is rather less than one-sixth of the total illuminating power.

get the necessary contrast, which is here between appearances at consecutive *times*, instead of as in ordinary vision between appearances in neighbouring *directions*."

Prof. Stokes writes, on various occasions:—"The diffused light of a powerful lamp would be weak and perfectly steady, and might thus escape notice, while a diffused light, even though no stronger, if almost momentary like lightning might be sufficient to attract attention, and you could afford to throw great chemical force into the formation of a flash which was to last only, as it were, for a moment." "If there are to be any more experiments, I think it would be well worth while to try explosions." "The more I think of it the more disposed I am to think that it would be worth while to try some experiments with flashes, I mean with explosions. The plant would not be at all costly; in fact, it would hardly cost anything. The chemicals would not cost very much. Preliminary experiments on a small scale, which could be tried anywhere, would show what chemicals were good to use in order to get a flash as bright as may conveniently be. But when the actual experiments in fog are tried, the quantity used should not be by any means very small. It should be enough to make the quantity of light for the moment much greater than what was kept up in your most powerful burners; but that would not require a very large quantity."

I should add that Prof. Stokes is aware that "flares" have been tried and used, but is clearly of opinion that further trials should be made in that direction. As to the great effect of sudden illumination in attracting attention there can be no doubt; but I find it hard to believe that in a fog in which an observer at a small distance was unconscious when the beam from the electric arc, through such a lens as the Mew Island lenses, passed quickly across him, any "flare" would be perceptible at the same distance. I do not know whether the experiment has been tried of combining a light-and-sound signal, by placing the materials used to produce a flash together with a charge of compressed gun-cotton in the head of a rocket, and so firing the rocket that it would explode at a fixed distance and bearing from the station and at a moderate altitude. A rocket will go through any fog, and might be used to give a lift seaward to the light and sound.

VI.—*Cost of each System*

I have not the data which are necessary for forming an independent opinion upon the estimates furnished in the Trinity House report. These estimates rest on unexceptionable authority; and I only venture to make any remarks on the subject because I am not entirely satisfied with the position assigned to the electric light.

I gather from the reference to the photometric results of Mr. Valentin that in a former estimate the cost was considered in relation to the yield of light by each illuminant. And I think that an estimate of what light can be had on any system for a given expenditure, or what the cost is on any system of a desired quantity of light, is needed to make the comparison of the relative cost complete. For example, a comparison is made in the Report (Tables I. and II., pp. 62-63), between a "first order oil-lighted tower for one six-wick burner only," and a "gas station to show a quadriform light of four times 68-jet power." The annual cost of maintenance, including interest on capital outlay, is estimated at 724*l.* for the first, and 1687*l.* for the second. But the light from a six-wick burner shown as a fixed light is probably rather less than 7000 candles, while that from the 68-jet quadriform is 35,000 candles. Thus, if such a quadriform were substituted for or adopted instead of the single light, both the cost and the light would be largely increased, but the increase of light would be more than double the increase of cost. Comparing the two as revolving lights, the increase of light would be one-third greater than the increase of cost. So in the comparison between the cost of the electric light and the other illuminants. The expense of annual maintenance, with interest on capital outlay, is estimated as being one-seventh greater for the electric light than for the quadriform 68-jet gas; but if the yield of light is taken into account, the figures appear very differently. The electric light is ten times as powerful as the quadriform. The actual cost of light, which is the commodity produced, may be compared in the two cases by stating the cost of maintaining for one year each 1000 candles intensity of light in the beam sent forth. By the electric arc the cost is about 1*l.*, by the quadriform gas 8*l.* 10*s.*

As to the actual estimate of the cost of the electric light, I venture to suggest that some reduction may be found practicable. Two engines of 30 horse-power, at a cost of 1250*l.*, seems a very full provision for the unfailing maintenance of one arc light. The actual horse-power absorbed by one of the De Méritens machines is given by Prof. Adams as 10·4. M. Allard states the price of one of M. de Méritens' machines, tested and recommended by him in 1880, as 350*l.* Dynamo-machines are, I believe, less costly. I think all that is needful and best of machines, lamps, and cables, may now be obtainable for less than 2154*l.*; and the estimate of 1217*l.* a year for wages, clothing, coke, oil, carbons, &c., and repairs and renewals, may perhaps be found in excess of the necessary cost. M. Allard gives the following details of the cost of the apparatus and expenses at Grisnez:—

	£
Two steam-engines of from five to six horse-power ...	488
Two magneto-electric machines	852
Four lamps	240
Total	1580
Annual expenses	537

He also furnishes an estimate, which I transcribe, of the average cost of converting an oil lighthouse into an electric lighthouse:—

	£
Engine-house, keepers' dwellings, water-supply, &c. ...	1600
Two steam engines, of from six to eight horse-power, with shafting and straps	640
Two electric machines with cables, &c.	560
Four lamps	240
First order lantern with oblique framing	800
Optical apparatus, two feet in diameter, with arrangements for revolving light	520
Sundries, packing, carriage, and setting up	240
Contingencies	400
Total	5000

Add "First cost of an ordinary first order oil-lighted tower" (T. H. Report, Part I., p. 75), less "Apparatus, lantern and glass," viz. 4834*l.*; and it would seem to follow that the first cost of an electric light station need not exceed 10,000*l.*

The rather large difference between this estimate and that of 17,749*l.* furnished by the Trinity House Committee has no doubt already received the consideration of the Elder Brethren, who have had the details of M. Allard's scheme before them.

In order to obtain some further information, I have inquired of a London firm of electric light contractors as to the cost of such an installation, and have received the following figures:—

	£
Two 30 horse-power *effective* engines	700
Three lamps, necessary cable, and labour	400
Two dynamos and spare armature to fit either ...	480

It will be seen that these figures are not very different from those of M. Allard's estimate.

In concluding this report I desire to thank the Elder Brethren of the Trinity House, and especially the Members of the Committee, for the kindness which has made the duty of co-operating with them in this inquiry a continual pleasure.

March 9, 1886 A. VERNON HARCOURT

Letter to the Board of Trade from the Commissioners of Northern Lighthouses

Northern Lighthouse Board, 84, *George Street*, *Edinburgh*, *December* 10, 1885

SIR,—I am directed by the Commissioners of Northern Lighthouses to acknowledge the receipt of your letter of the 3rd instant, in which you request to know whether the Commissioners propose to make any general report or observations relative to the recent Lighthouse Illuminants Experiments at the South Foreland.

In reply I am directed to state that in view of the very able and exhaustive Report (Part I.) by the Committee of the Elder Brethren on this subject, in the conclusions of which the Commissioners concur, and of the report by their engineers dated October 14 last, which accompanied my letter to the Elder Brethren of the following day, and copy of which is herewith transmitted for the information of the Board of Trade, the

Commissioners do not propose, at least in the meantime, to make any general report or further observations in regard to these experiments.—I am, &c.,

(Signed) J. M. DUNCAN

The Assistant Secretary, Harbour Department, Board of Trade

Report on Part I. of Report by the Special Committee of the Elder Brethren on Lighthouse Illuminants, by T. and D. Stevenson, Engineers, to the Northern Lighthouse Board

On September 2 last, the Board remitted to us, for consideration and report, Part I. of the Report of the Committee of the Trinity House, on the experiments at South Foreland, and having perused the reports of the experiments, we now beg to report as follows:—

As the Board is aware, the object of these experiments was to determine the relative merits of gas, oil, and electricity, as lighthouse illuminants, especially as regards their penetrative power in fog; also to ascertain the merits of certain optical arrangements, and test certain improved oil and gas burners patented by Sir James Douglass and Mr. Wigham. We think that these experiments embrace all the suggestions that were brought under the notice of the Elder Brethren for trial by the Board of Trade, and which led that Board to suggest that this train of experiments should be entered upon.

We have had several opportunities of inspecting the various kinds of apparatus, and also of witnessing the experiments, and in our opinion these experiments have been of an exhaustive character, and have been conducted with great care, and, we believe, in a spirit of the most perfect fairness and impartiality to all parties; and we have further to express our concurrence in the conclusions of the Committee as expressed in the following terms:—

"(1) That the electric light, as exhibited in the A experimental tower at South Foreland, has proved to be the most powerful light under all conditions of weather, and to have the greatest penetrative power in fog.

"(2) That for all practical purposes the gaslight, as exemplified by Mr. Wigham's multiform system in B experimental tower, and the oil light, as exemplified by the Trinity House Douglass six-wick burners in multiform arrangement up to triform in C experimental tower, when shown through revolving lenses, are equal, light for light, in all conditions of weather; but that quadriform gas is a little better than triform oil.

"(3) That when shown through fixed lenses, as arranged in the experimental towers, the superiority of the superposed gas light is unquestionable. The larger diameter of the gas flames and the lights being much nearer to each other in the gas lantern, give the beam a more compact and intense appearance than that issuing from the more widely separated oil burners.

"(4) That for lighthouse illumination with gas, the Douglass patent gas-burners are much more efficient and economical than the Wigham gas-burners.

"(5) That for the ordinary necessities of lighthouse illumination, mineral oil is the most suitable and economical illuminant, and that for salient headlands, important landfalls, and places where a very powerful light is required, electricity offers the greatest advantages."

We may explain that so far back as 1869 we had also occasion to conduct a series of experiments at Edinburgh on certain large burners patented by Mr. Wigham, and brought before the Board, with the view of increasing the power of sea lights, and again in 1870 a further series of experiments was carried out by us, by the direction of the Board of Trade, on the merits of the electric light, and certain important results were obtained during these sets of experiments at Edinburgh. The general result of the gas experiments then made was that the large 52-jet gas-burner was in no way superior to the 4-wick oil-burners when used in connection with the ordinary annular lens, as the "greater portion of the 7-inch gas flame" was with that size of lens necessarily ex-focal. The large burner was slightly superior, however, when used in a fixed light apparatus. Our opinion, which has been corroborated by the recent experiments, therefore, was that in order to bring out the full power of these large flames, an apparatus of larger focal distance than usual must be employed, and hence we designed the lens of 1,330$^{m/m}$ radius, which has recently been tried at the South Foreland with the most satisfactory results.

The electric light experiments made at Edinburgh in 1867–1869, showed that if the electric light beam was made to diverge artificially to the same extent as the 4-wick oil flame, it was in no way superior in brilliancy, and pointed to the advisability of adopting for the electric light the azimuthal condensing system of Mr. Thomas Stevenson. This system has been adopted for the electric light apparatus at the South Foreland experiments. The Edinburgh experiments further showed that it was necessary in any test of the intrinsic merits of electricity, gas, or oil, that the maximum condensation consistent with the requirements of navigation for each should be employed. At the South Foreland experiments, however, the condensation of the electric light was only 30° into 5°, that is only 6 times, whereas the Isle of May apparatus, which was exhibited at the South Foreland, condensed 45° into 3°, that is 15 times. Had this or a still more condensed light been used in the experiments, the electric light would have shown even greater superiority than it did.

We shall afterwards report as to what, in our opinion, is the arrangement of optical apparatus best suited for each illuminant. (Signed) T. and D. STEVENSON

Edinburgh, October 14, 1885

OFFICIAL REPORT ON THE USE OF OIL AT SEA FOR MODIFYING THE EFFECT OF BREAKING WAVES[1]

THE following Memorandum, dated June 16 last, on the use of oil at sea for modifying the effect of breaking waves, has recently been printed and circulated by the Admiralty:—

"Many further practical experiments at sea have been made since the report by Capt. Chetwynd, R.N., to the Royal National Lifeboat Institution, dated September 30, 1884, on the use of oil for smoothing broken or troubled waters, which report was communicated to Commanders-in-Chief in Admiralty Circular Letter of December 1, 1884, N.S. $\frac{3206}{8305}$.

"As these further experiences go to show that the use of oil, under different circumstances, is of very extended and simple application, my Lords Commissioners of the Admiralty consider it desirable, in order that the facts may be generally known, to re-issue the report above mentioned, together with such other information as may serve for the guidance of officers, whose attention is hereby called to the fact that a very small quantity of oil skilfully applied may prevent much damage both to ships (especially the smaller classes) and to boats by modifying the action of breaking seas.

"The principal facts as to the use of oil are as follow:—

"On free waves, *i.e.* waves in deep water, the effect is greatest.

"In a surf, or waves breaking on a bar, where a mass of liquid is in actual motion in shallow water, the effect of the oil is uncertain, as nothing can prevent the larger waves from breaking under such circumstances; but even here it is of some service.

"The heaviest and thickest oils are the most effectual: refined kerosene is of little use; crude petroleum is serviceable when nothing else is obtainable; but all animal and vegetable oils, such as waste oil from the engines, have great effect.

"A small quantity of oil suffices, if applied in such a manner as to spread to windward.

"It is useful in a ship or boat, both when running, or lying-to, or in wearing.

"No experiences are related of its use when hoisting a boat up in a seaway at sea, but it is highly probable that much time and injury to the boat would be saved by its application on such occasions.

"In cold water, the oil being thickened by the lower temperature, and not being able to spread freely, will have its effect much reduced. This will vary with the description of oil used.

"The best method of application in a ship at sea appears to be hanging over the side, in such a manner as to be in the water, small canvas bags capable of holding from one to two gallons of oil, such bags being pricked with a sail needle to facilitate leakage of the oil.

"The position of these bags should vary with the circumstances. Running before the wind, they should be hung on

[1] From the *Board of Trade Journal*.

either bow, *e.g.* from the cathead, and allowed to tow in the water.

"With the wind on the quarter, the effect seems to be less than in any other position, as the oil goes astern while the waves come up on the quarter.

"Lying-to, the weather bow and another position farther aft seem the best places from which to hang the bags, with a sufficient length of line to permit them to draw to windward while the ship drifts.

"Crossing a bar with a flood tide, oil poured overboard and allowed to float in ahead of the boat, which would follow with a bag towing astern, would appear to be the best plan. As before remarked, under these circumstances the effect cannot be so much trusted.

"On a bar with the ebb tide, it would seem to be useless to try oil for the purpose of entering.

"For boarding a wreck, it is recommended to pour oil overboard to windward of her before going alongside. The effect in this case must greatly depend upon the set of the current and the circumstances of the depth of water.

"For a boat riding in bad weather from a sea-anchor, it is recommended to fasten the bag to an endless line rove through a block on the sea-anchor, by which means the oil is diffused well ahead of the boat, and the bag can be readily hauled on board for refilling if necessary."

ON THE INTENSITY OF REFLECTION FROM GLASS AND OTHER SURFACES[1]

THE author pointed out that most previous experimenters, especially Rood, had measured the amount of the transmitted light, and that any percentage of error in this measurement was greatly multiplied when the results were used to calculate the amount of reflected light. In his experiments the amount of reflected light was measured directly. The method was as follows. Light from a cloud was passed through ground glass in the window of a darkened room, and made to fall at the polarising angle on a plate of glass. The transmitted and reflected rays were conducted along different paths by a series of reflectors, but finally emerged side by side and of equal intensity. One of the reflectors in the path of the reflected ray was the glass surface to be tested, the light falling on it at an almost perpendicular incidence. This glass was now removed, and a single mirror was shifted so as to make the angles and points of incidence of the reflected ray on the several mirrors the same as before. The reflected ray was now brighter than the transmitted. To re-establish equality a disk with holes in a ring round the centre was rotated in the path. The ratio of the sum of the breadths of the holes to the whole circumference of the ring gave the percentage of the light that was reflected. For a piece of optically-worked blackened glass the amount reflected was ·058 of the total incident light. It was found that the amount of reflection depended greatly on the clearness and polish of the surface. Thus in one case re-polishing increased the amount from ·04095 to ·0445. Fresnel's formula gave in this case ·04514. Generally it appeared that the amount reflected was less than according to Fresnel's formula—a result contrary to that of Rood's. The numbers for polished glass and for silver on glass were ·94 and ·83.

ON THE NATURE OF SOLUTION[2]

IN connection with the discussion on the "Nature of Solution," in Section B, at the Birmingham meeting of the British Association, the following paper was read by Spencer Umfreville Pickering, Professor of Chemistry at Bedford College:—

The "hydrate" theory attributes dissolution to the existence, in a stable or partially dissociated condition, of definite liquid compounds (generally unknown in the solid form) of the substance dissolved and its solvent, and the mixing of these compounds with excess of the solvent.

In certain special instances we have direct evidence of the reality of such compounds,[3] but it is on general grounds rather than on any special experiments that I would seek to establish their existence.

[1] Abstract of a Paper read at the Birmingham meeting, 1886, of the British Association, by Lord Rayleigh.

[2] Continued from p. 22. From the *Chemical News*

[3] See especially Berthelot, *Ann. Chim. Phys.* (5), 4, 445 to 537.

There is, in the first place, a strong *primâ facie* improbability that substances such as copper sulphate, potassium hydrate, &c., which possess such an intense affinity for water, should be capable of existing in the anhydrous condition in the presence of an unlimited amount of water.

We know, moreover, that in a great number of cases—where a dehydrated salt is placed in water—hydration does undeniably precede dissolution,[1] and in such cases the salt can only exist in the liquid in the uncombined state if the continued action of the solvent is to decompose the hydrate which it has just formed. The only two forces by which such a decomposition might be supposed to be effected are (1) the attraction of the bulk of the water present for the few molecules of water combined with the salt; (2) the attraction of this same bulk of water for the (anhydrous) salt molecules. On the one hand, however, it is absurd to imagine that the mass of water molecules possess such a strong attraction for the few contained in the hydrate as to decompose this latter, or, even if they did, that they would ever have given them up to the salt in the first instance; and, on the other hand, it is equally absurd to urge the intensity of the attraction of the salt molecules for the water molecules as a reason for these two parting company.

Another general fact, which lends considerable support to the view that the dissolution of a salt is due to the formation of a hydrate, is, that those salts which *combine* with water always *dissolve* in that liquid, and, as a rule, the greater the energy with which they do combine with it, the greater is their solubility.

The thermal phenomena attending the act of dissolution point incontestably to the same conclusion. When a dehydrated salt (say $MgSO_4$) is dissolved in water a considerable evolution of heat occurs: and by the simplest experiment it can be established, beyond any possibility of doubt, that all, or the greater portion of this heat is due to the hydration of the salt. If the salt be taken in the hydrated condition less heat is evolved, and, without a single known exception, this evolution diminishes continuously as the salt taken is more and more highly hydrated; but even when taken in its most highly hydrated condition the evolution of heat is in many cases still very considerable.[2] Now, unless we can reconcile ourselves to attribute the heat evolution in this latter case to a cause entirely different from that which exists in the other cases,—unless we are content to shut our eyes to the proportionality between the heat evolved and the degree of hydration of the salt taken,—we must admit that even with a fully hydrated solid salt the heat evolved is due to further hydration; that not only do hydrates exist in solution, but that they are often of a higher order than the highest known in the solid condition.

Coming now to the other side of the question, we find many general considerations, as well as special results, brought forward against the hydrate theory of solution. The latter, however, are for the most part, I consider, urged on mistaken notions, and prove nothing *pro* or *con*.

Thus Dr. Nicol's study of the molecular volumes of salts in solution shows that their volumes are entirely uninfluenced by the presence or absence of water of crystallisation in the solid salt; that if any water is still combined with the salt when dissolved it acts in the same way, and is quite indistinguishable from the rest of the solvent present. In so far as his conclusion that these molecular volumes afford no evidence in support of the existence of combined water, I entirely agree with Dr. Nicol; but in concluding that therefore no water is combined, he has pushed his conclusions far beyond legitimate limits. The same reasoning that leads to the belief that the water and the salt bear no chemical relationship towards each other in solution would hold equally good with reference to the radicles of which the salts themselves are constituted, as Favre and Valson indicated in 1875 (*Comptes Rendus*, lxxv. 1000). Each radicle possesses its own specific volume entirely uninfluenced by the

[1] Dr. Nicol (*Phil. Mag.* 1885, i. 453, and ii. 295) quotes experiments with sodium sulphate in opposition to this view. He shows that the dehydrated salt may dissolve in water under certain circumstances without any signs of previous hydration. When it does so, however, it forms a supersaturated solution, which is certainly very different from a normal solution, being, according to Dr. Nicol's determination of the solubility, due to the extension at lower temperatures of conditions which exist naturally only above 33°: but when it dissolves to form a *normal* solution it is with evident signs of hydration. Whatever this may prove as to the supersaturated solution, it certainly does not prove that the normal solution contains the anhydrous salt, —rather the opposite.

[2] Thus the "true" heat of dissolution of $MgSO_4 7H_2O$ is +7000 cal., and even this number is probably 1000 to 3000 cal. too low, as it contains no allowance for the heat of fusion of the $MgSO_4$ molecule. (See *Chem. Soc. Trans.* 1886, 279.)

nature of the other radicle with which it is combined: the radicles behave independently, and as if there were no combination between them.

Nor is it only from a study of the volumes of salts in the dissolved state that such results are obtained. Numerous determinations of the extent to which the vapour-pressure, the freezing-point, and the temperature of maximum density of water is influenced by the presence of various salts in it, have been made by Wüllner, Blagden, Dufour, Depretz, Rüdorff, and De Coppet,[1] with the general results that certain hydrates of the salt are in some cases present, and in others the salt is anhydrous; but these conclusions, which would tell more against the hydrate theory than for it, are eminently unsatisfactory. The whole question, however, has been re-opened by Raoult (*Ann. Chim. Phys.* (5), 28, 133; (6), 2, 66, 4, 401); and by an exhaustive extension of the work, and by including solvents other than water, and solids other than salts, he has thrown a new light on the subject. Not only does the salt, in its influence on the freezing-point, show no signs of the presence of combined water, but it shows no signs of itself being a single compound; each of the radicles contained in it acts independently of the other, and in precisely the same manner as a *molecule* of a non-saline substance (see especially *loc. cit.* 4, 426). Precisely similar conclusions as to the apparent non-combined state of radicles in a dissolved salt were arrived at by Valson in his work on capillarity (*ibid.* 1870), and by Hugo and Vrié (*ibid.* 1883) in their examination of the effect of membranes on salt-solutions. Other instances of a similar nature, physical and thermo-chemical, might be quoted.

That atoms or molecules which are undoubtedly united may retain their individuality so far as to act towards certain agents as if they were free, is surely not surprising; and from such methods as would lead us to conclude that the very radicles composing a salt are uncombined, it would be useless to look for evidence of the more feeble combination of the salt with its water, and inconsistent to argue, from the absence of such evidence, that no combined water is present.

Although I am not inclined to attribute any weight to these special experiments brought forward against the hydrate theory, it is otherwise with more general considerations.

The formation of hydrates cannot explain the absorption of heat which in many cases accompanies dissolution. The phenomena of solution are too universal to permit of imagining the existence of some definite compound of the dissolved substance with the solvent in every case. There is a continuous influence exerted by the salt on its solvent too extensive to be accounted for by the effect of mass on partially dissociated hydrates; there is a continuity between the fused and dissolved states in many cases, and a regularity in the variation of solubility with change of temperature, &c., which cannot be thoroughly explained on the hydrate theory.

However undeniable the existence of these compounds may be in many cases, they do not give an adequate explanation of all the facts of dissolution.

The hydrate theory can be neither rejected nor accepted.

The explanation of this contradiction is not, I think, very difficult to find. We are talking about molecules of solids and liquids, not as they exist, but as they do *not* exist. Our chemical formulæ for them represent but the results of analysis, or, at the most, the constitution of the substance in that transitory state of simplification which immediately precedes entire decomposition; what their composition may be when in the free state, and removed from all decomposing forces, we know not; all we do know or believe about them is, that they are then far more complex than chemical formulæ represent.

Crystalline form alone would show that a number, probably a very great number, of our so-called molecules combine together, bear certain definite relations and hold certain definite positions towards each other, producing a molecular aggregate or physical unit, which alone should receive the name of molecule.[2]

Just as a number of similar particles unite to form an aggregate or true molecule of any simple substance, so will dissimilar particles unite to form aggregates of a more complex nature.

It is but natural that our prejudices in favour of the "laws" of chemical combination and atomic valency, to which we owe so much, should lead us to attribute the variable composition of certain substances to our imperfect means of investigation rather than to the nonconformity of these substances to our laws. Whether we be right or not in our explanation, we must acknowledge that apparent *in*constancy in composition is one of the most marked features of immense classes of substances which cannot be termed other than chemical compounds.

The varied composition of minerals is said to receive an explanation in the statement that isomorphous substances may displace each other in definite proportions, but to an indefinite extent. This is undoubtedly true, but it does not obviate the necessity of recognising the existence of some form of attraction between these isomorphous substances. No purely mechanical or physical cause can explain this phenomenon; mere similarity of crystalline form has been proved to be incompetent to produce such results. A selective power is exhibited by the substances which thus unite,[1] as well defined as that selective power which in the case of simpler substances has received the name of chemical affinity, and the resulting compounds are characterised by the same uniformity in composition and physical properties[2] which is the attribute of acknowledged chemical compounds.

Nor is it with minerals and artificial crystals only that we find ourselves in what would appear to be a wide border-land between chemical compounds and mixtures. Whether we study the formation of alloys, the occlusion of gases by solids, ranging from the most mechanical action by insensible gradations to the formation of a substance having every appearance of a definite compound, or the decomposition of some of the firmest chemical bodies by so-called mechanical means (filtration), or the constant change in composition of many basic salts with change in the circumstances of their formation,—we are forced to admit that the definiteness which characterises the combination of atoms may be absent from, or at any rate unrecognisable in, the combination of our so-called molecules to form complex aggregates.

When we examine the constituents of these apparently indefinite compounds, it becomes clear that it is only substances which resemble each other which can combine in this manner; and one of the most striking features of dissolution offers such a strict parallel to this, that its meaning can scarcely be mistaken.

A certain degree of similarity in nature between the solvent and the substance dissolved is the invariable accompaniment of dissolution.

Dissolution, I believe, is but one of the many results of apparently indefinite chemical combination.

We cannot obtain a satisfactory explanation of the composition of minerals by admitting the existence of *definite* double salts only, nor can we explain the phenomena of dissolution by confining our attention to *definite* hydrates only. These may, and in all probability do, exist in solution, but they are only small circles within the larger ones; their successive formation and decomposition would give rise to irregularities and effects such as those which are observed in some cases; but these irregularities would form but ripples on the more regular changes which would accompany the variations in the molecular aggregates,—variations which, as in the case of minerals, would be so dependent on physical conditions as to obliterate their chemical nature when examined from many points of view.

The evolution of heat accompanying dissolution will still be attributable, as on the ordinary hydrate theory, to the formation of chemical compounds, but the far greater complexity, and, consequently, instability of these, than of atomic hydrates, if I may so call them, will remove all difficulty in comprehending the continuous effect of the mass of the solvent upon them, even when the latter exceeds that of the salt many hundred-fold; where heat is evolved, therefore, the evolution will be increased, though at a diminishing rate, by dilution.

The rapid increase in the heat of dissolution, produced by a rise of temperature, is but a necessary consequence of the formation of a chemical compound possessing a specific heat less

[1] For a general summary and discussion of the results from the point of view of these physicists, see De Coppet, *Ann. Chim. Phys.* (4), 23, 366; 25, 502; and 26, 98.

[2] In a Paper read before this Section last year (Report, p. 989), I argued that our formulæ adequately represented the molecules of solids and liquids with which chemical reactions deal, although I fully recognised the existence of far more complex aggregates; my opinions have so far altered that at present I consider these aggregates to be recognisable in many operations which must be termed chemical, although in the great bulk of ordinary reactions the simpler or ultimate molecules need alone be considered.

[1] A power or "affinity" so strong that it will sometimes induce a salt to separate out in a crystalline form and with a proportion of water foreign to its nature, as well as from a solution too weak to yield it of its own accord (Aston and Pickering, "Multiple Sulphates," *Chem. Soc. Trans.*, 1886).

[2] J. M. Thomson, on the "Double Sulphates of Nickel and Cobalt" (*Brit. Assoc. Rep.* 1877, 209).

than the sum of those of its components, and would of itself go far to prove that a solution did in reality contain such a compound. But a rise of temperature would also undoubtedly have another and opposing effect, for, being inimical to the complexity of these hydrates, they would be more dissociated at higher than at lower temperatures, and hence the heat of dissolution would not be so great as it should be according to the various specific heats. This is precisely what Dr. Tilden has proved to be the case (*Proc. Roy. Soc.* 1885, 401).

There is, however, another action which I believe accompanies every act of dissolution resulting in the *absorption* of heat.

The heat absorbed by a large number of salts in dissolving cannot be freely accounted for by the mere physical change of the solid into the liquid salt. Thus, the heat of dissolution of potassium nitrate is −8500 cal., and that of sodium nitrate −5000 cal., whereas the heat of fusion of these salts at the same temperature is but −1300 and −2300 cal. respectively. There must be some other heat-absorbing action besides the fusion of the salt. The amount of heat thus absorbed increases also with the dilution of the liquid. Moreover, we cannot, I think, account for the manner in which heat is evolved in one case and absorbed in another, or the way in which an absorption of heat sometimes gives place to an evolution, as the temperature or other conditions are changed, but by admitting the constant co-existence of two actions producing opposite thermal effects, and being influenced to different extents by an alteration of circumstances.[1]

On the theory which I am here advocating, this absorption of heat receives a ready explanation. Whatever be the complexity of the molecular aggregates of a liquid, those of a solid will be still more complex. Fusion would, therefore, entail their simplification; it would be but a chemical decomposition absorbing heat; this simplification would be pushed much further, however, when the salt is dissolved instead of being merely fused, for the particles of the liquid act chemically (*ex hyp.*) on those of the solid and combine with them themselves; the cold absorbed on dissolution would exceed that absorbed on fusion, and would, moreover, be increased by increasing the amount of the solvent. This accords fully with the facts observed.

All the phenomena attending dissolution are, therefore, I contend, accounted for by a full recognition of the real complexity of the units of matter, and by taking the more liberal view of chemical combination which is inculcated by a study of minerals and other substances. Every act of dissolution involves two actions. The chemical decomposition of the more complex aggregates of the solid into a simpler form, absorbing heat, and a chemical combination of these with the liquid, evolving heat; the only quantity which we can at present measure is the algebraic sum of these two.

Mr. Durham next gave a short statement of his own theory of solution:—

When, for example, common salt (NaCl) is placed in water, all the atoms act upon each other. The sodium of the salt acts upon the oxygen of the water, and the chlorine of the salt upon the hydrogen of the water; and the result is a definite compound, which we call a solution. The heat of formation of the acid is neutralised by the heat of formation of the oxide. If they be not equal, the difference is the heat of the solution; if they be equal, the heat is of course *nil*. If the former be the greater, the heat of the solution is negative; if the latter, it is positive. Solution arises from chemical affinity, and takes place inversely as the attraction between the positive element and the oxygen—and the negative element and the hydrogen—of water. But chemical affinity is itself physical; the atoms are physical, and all forces which act upon them must be physical forces. In a chemical mixture every atom is acting upon every other atom, but such action can be nothing else than physical; and we are therefore led to the conclusion that there is really no difference between chemical and physical action, and, consequently, that the alternative between the two does not exist.

In the course of the discussion, and preceding the reading of Mr. Pickering's paper, the following remarks were made:—

Dr. Armstrong said that, from the summary given by Prof. Tilden, it appeared that the two important questions for discussion were—(1) Does water of crystallisation exist in solution combined with the salt as it did prior to dissolution? and (2) What distinction is to be drawn between chemical combination and mechanical association or adhesion? In short, are the phenomena of dissolution of a chemical or of a mechanical character? But Prof. Tilden had made an important omission, inasmuch as he had not discussed the possible simplification of the molecules on dissolution: in discussing the evidence afforded by the various phenomena, everything turned upon the question whether the crystal molecules are of the composition represented by our ordinary formulæ, or are more or less complex.

As regards the first question, Prof. Tilden appeared to differ from Dr. Nicol, and to think that water of crystallisation did exist in solution. (Prof. Tilden, interposing, desired to explain that what he had said was that it was impossible, in the case of any solution, to say that one portion of the water is in combination with the salt and that another is not; all the phenomena of dissolution and dilution being continuous, no point can be found at which such a distinction can be set up. He believed that the salt was attached to all the water present without exception.)

Dr. Armstrong, resuming, said that much of the evidence appeared, he thought, to favour the conclusion that in certain cases water of crystallisation did exist in solution; *e.g.* the difference in colour between many hydrated and dehydrated salts taken in conjunction with the colour of their solutions. Again, many dehydrated salts dissolved much less readily than the corresponding hydrated salts: instances of this kind were not common among inorganic salts, but were often met with among organic salts, and the speaker cited calcium butyrate and certain naphthalene- and napthol-sulphonates as examples. Dextrose, again, ordinarily crystallises with two molecules of water, but if dehydrated and carefully dissolved in water at a low temperature it may be crystallised out from the solution in the anhydrous state. T. Thomsen's recent experiments, however, appeared to show that when two substances were dissolved in water they appropriated the water in the proportions in which they were present, thus favouring a purely mechanical interpretation of the phenomena of dissolution: but, on the other hand, it was to be noted that in the case of citric and sulphuric acids, for example, Thomsen's results were in accord with this conclusion only when it was assumed that the citric acid was present as the dihydrate, and sulphuric acid as the monohydrate, $H_2SO_4.OH_2$. In fine, the speaker was of opinion that while the question could not be regarded as settled, yet there was a considerable amount of evidence that the water was not evenly distributed, but was, in some cases at least, in part directly combined with the dissolved substance. Dr. Nicol had deduced an ingenious argument from J. Thomsen's observations on heats of neutralisation. As a criticism of Dr. Nicol's argument from the existence of neutralisation constants he would venture to say "Put not your faith in constants." If the views which he held—views which probably were at present peculiar to himself—were correct, the quantities in question ought to have a constant value. According to Helmholtz, all atoms hold a positive or negative electrical charge, a single charge being associated with a monad, two with a dyad, and so on. If when combination takes place these charges exactly neutralised each other, all compounds would be neutral and saturated; but actually this is not the case: in point of fact, there is no such thing as a saturated compound. Helmholtz seems to think that the charges may be held by different atoms with different degrees of force, but the speaker took a somewhat different view, and thought that probably when two atoms combined, in consequence perhaps of peculiarities of structure, their charges were not completely used up; the resulting molecules therefore possessed a certain residual charge or affinity, and were consequently in a position to enter into combination with other molecules. Thus water, he thought, was not a saturated compound; its oxygen atom was still possessed of residual affinity. The same was true of sulphuric acid. Consequently the two could combine together to form a hydrate. On neutralising a dilute solution of alkali by a dilute solution of acid, a stable condition is finally attained, and it is to be assumed that the affinities are fully satisfied, or very nearly so—that the charges practically neutralise each other: hence it may be expected that the heat of neutralisation will have nearly a constant value provided there be no disturbance such as the separation of a precipitate would produce. But the value of each of the several processes which go to make up the heat of neutralisation are entirely unknown to us, and in the absence of such knowledge it

[1] A study of the thermal results attending the dilution of salt-solutions, established by Thomsen ("Thermo-chem.," iii., especially plate iv., and also the curves given by formic and acetic acids and by potassium and sodium hydrates), impresses very forcibly the co-existence of these two actions, although Thomsen himself does not seem to have noticed it.

is impossible to place much confidence in arguments based upon the study of such complex phenomena.

As regards the question of chemical *versus* mechanical action, the speaker could only imagine one form of mechanical action attending dissolution, viz. that of the water molecules bombarding the surfaces of the solid, and as it were chipping off particles. All other actions, in so far as they could be regarded as involving the attraction of the molecules of the dissolved substance by those of the solvent, he was inclined to class as chemical. Nothing was more certain than that dissolution depended on the nature both of the solvent and of the substance dissolved. Like dissolves like—water is *the* solvent for bodies containing oxygen; sulphur compounds are dissolved by carbon bisulphide; phosphorus compounds by chloride of phosphorus; shale spirit, which is rich in olefines, and especially rosin spirit, which is rich in acetylenes and benzenes, were far better solvents of hydrocarbons and resinous bodies than petroleum, which consisted of saturated inert hydrocarbons, and was the worst of solvents. Facts such as these spoke strongly in favour of the conclusion that the phenomena of dissolution are largely of a chemical character.

Prof. W. N. Hartley was understood to base the argument in favour of the hydration theory chiefly on the changes of colour observed in the solution of certain salts in various proportions of water. The chlorides, bromides, and iodides of cobalt, nickel, and copper exhibit these phenomena most plainly. Thus the iodide of cobalt in the anhydrous state is black, its dihydrate is green, the hexhydrate a reddish brown. If this last be dissolved in water a pink solution is formed, which probably contains a richer hydrate. The brown saturated solution of the hexhydrate is a very dense liquid, of specific gravity about 3, and when water is added to it the formation of the pink liquid is attended by a large evolution of heat, and this affords evidence that the hydrate exists in the solution. Again, hydrated cupric chloride contains two molecules of water, and when quite dry is of a pale blue colour. Its solution in water has the same colour unless it be heated, and then it turns green. Nickel salts behave similarly. So that the evidence, on the whole, warrants the belief that when a hydrated salt is dissolved in water the water of crystallisation remains a constituent part of the molecule.

Dr. Gladstone commenced his remarks by a discussion of the question, What is a salt in solution? Is the solution of a salt in water a process analogous in any degree to the decomposition which takes place when one salt is mixed with another? Take, for instance, chloride of sodium and water. Many years ago the speaker had endeavoured to determine whether any chemical decomposition of the salt by the water occurred so as to give rise to sodium hydrate and hydrochloric acid, but he had come to the conclusion that this decomposition took place, if at all, only to a very small extent. Many salts, as had already been stated, combine with water to form coloured hydrates, and the hydrate is of a colour different from that of anhydrous salt. But a coloured hydrate, when dissolved in a sufficient quantity of water, is never changed by further dilution. The speaker had endeavoured to ascertain whether the specific refraction of substances was altered by solution. He had found that no alteration could be detected, and this result was afterwards confirmed by the experiments of other chemists. The refraction equivalent of a solution is equal to the sum of the refraction equivalents of the salt and the water present. In an alum solution, the water of crystallisation supposed to be in combination with the salt is not distinguishable by its refractive power from the water of solution outside it. It seems impossible, however, to arrive at a conclusion with regard to the constituents of a solution. The idea of reciprocal decomposition is not supported by experimental evidence, save in some exceptional cases, and the actual condition of a dissolved salt seems beyond expression by formulæ.

TEN YEARS' PROGRESS IN ASTRONOMY[1]

THE Earth.—In what may be called the astronomy of the earth there is no very great discovery, nothing extremely new and brilliant to record during the past decade; but there has been considerable and steady progress.

(*a*) As regards the earth's form and dimensions, it has become quite certain that Bessel's ellipticity (1/300) is too small. Clarke's value of 1/294 is now admitted and employed on the U.S. Coast Survey with a decided improvement of accordance. A slightly larger value even is suggested by the most recent pendulum observations, and 1/292 is now adopted in Europe.

One of the most important steps in this branch of investigation is the discovery by Mr. Peirce (of our own Coast Survey), of the large correction required in many former pendulum determinations, on account of the yielding of the stand from which the pendulum is suspended.

During the past ten or fifteen years a great amount of material has been collected towards a complete gravitational survey of the earth, by the work of Lieut.-Col. Herschel in India, and of the officers of the Coast Survey in this country and elsewhere, and a very important part of it has consisted in connecting the older work with the new, by Peirce's operations in Europe, and those of Herschel in this country.

At the same time it has become increasingly evident that very little is now to be gained by endeavouring to find a spheroid fitting the earth's actual form more closely. It will be best simply to adopt some standard (say that of Clarke, but it makes very little difference what), and to investigate hereafter the local deviations from it. These deviations seem to be larger and more extensive than used to be supposed, the station errors in latitude and longitude being at least quantities of the same order as the variations of elevation.

We mention, in passing, the investigations of Fergola, based on observations at Pulkowa and Greenwich, and leading to a suspicion that the axis of the earth is slightly changing its position and shifting the place of the Poles on the earth's surface. Operations have been organised to determine the question by co-operation between different observatories in nearly the same latitude, but widely differing in longitude.

Nor ought we to pass unnoticed an elaborate paper by Kapteyn, of Groningen, on the determination of latitude by a method depending upon time-observation of stars, at equal altitudes, though in widely different parts of the sky; the stars being so selected that all errors of star-places, instrument, and clock, are almost perfectly eliminated. In the same connection we ought to mention also the new equal-altitude instrument, the Almucan'ar, invented by Chandler, of Cambridge, and his development of the method of determining time by its use. It may possibly supersede the transit instrument for this purpose, as he seems to expect, though we think it hardly likely.

Rapid progress has been made in determining the difference of longitude between all the principal parts of the earth. There now remain very few stations of much importance which have not their longitude from Greenwich telegraphically settled within a small fraction of a second. In Europe Albrecht has combined into a consistent whole all the different data for more than one hundred points. Our American system has been similarly worked out by Schott, and is connected with the European by no less than four different and independent cable-determinations. South America is connected with the United States by the recent operations of our naval officers in the West Indies and along the eastern and western coasts of the continent; and with Europe by a cable connection between Lisbon and Pernambuco, also effected by them. It is worth noting that two large errors in European longitudes owe their detection to American astronomers. The difference of longitude between Greenwich and Paris was corrected by our Coast Survey in 1872 to the extent of nearly half a second of time, and our naval officers in 1878 showed that the then received longitude of Lisbon was 8·54s. too small! It is a less surprising fact that an error of 35s. was found in the longitude of Rio.

Our navy has also determined an important series of telegraphic longitudes along the eastern coast of Asia and through the East Indies. The French have been doing similar work in the same regions, especially in connection with the transits of Venus; and the English have determined a large number of longitudes in India. These Asiatic longitudes have been recently connected with Australia and New Zealand by English astronomers, and a telegraphic longitude connection has been effected down the eastern coast of Africa from Aden to the Cape; so that now it is perfectly practicable, if it is desirable, to have one standard of time in all the civilised world.

A word perhaps is here in place as to this question of standard time and the beginning of the day. The adoption by our railroads of the system of standards differing from Greenwich time only by entire hours has, I think, been admittedly a great step in advance, as regards public convenience and safety in travelling. At a few points, where standard and local time happen

[1] "Ten Years' Progress in Astronomy, 1876-86," by Prof. C. A. Young. Read May 17, 1886, before the New York Academy of Sciences.

to differ by nearly the maximum possible amount of half an hour, some annoyance is felt, and there is still some opposition; but it seems quite clear that, in this country at least, all resistance will soon die out.

As regards the more purely astronomical question of making the astronomical day coincide with the civil day, by beginning at midnight, instead of noon, as it does at present, there is more difference of opinion. For my own part, I am frankly in favour of the change, because I see no use in perpetuating an anomaly which is sometimes annoying and confusing. At the same time the change would, of course, involve some inconvenience to computers and night-observers, and it must be admitted that at present a large number, and possibly a majority, of the most eminent astronomers, in other countries as well as in this, are opposed to it. Those of us whose work falls about as much in the day as in the night, and those, I think, who take a long look ahead, are in favour of the reform; but those whose work is mainly nocturnal, or is based on observations made chiefly at or near the "witching hour," dread the inconvenience of a change of date in the midst of the record, and the risk of confusion in the interpretation of old observations.

The question, however, seems to me not a very important one. I notice that the visitors of the Royal Observatory have just recommended that the change be introduced into the British *Nautical Almanac* for 1891.

Before passing to the moon, a word should be added as to the outcome of the most recent investigations regarding the steadiness of the earth's rotation. Some irregularities in the lunar motions have appeared to justify a suspicion, at least, that they might be caused by irregularities in the length of the day. The researches of Newcomb upon ancient eclipses and occultations of stars give results not necessarily inconsistent with this hypothesis, perhaps even slightly in its favour, but his careful examination of the past transits of Mercury contra-indicates it pretty decidedly.

The Moon.—During the past ten years there has been no work upon the lunar theory quite on a level with that of Hansen, Delaunay, Plantamour, and Adams in the years preceding; but the labours of Neison, Hill, and Newcomb well deserve mention. The former especially has carried his approximations to a considerably higher point than any of his predecessors, though not without making a few numerical mistakes, which have been detected and corrected by Hill. The investigation of ancient and mediæval observations of the moon by Newcomb is also a very important work, as showing clearly that the lunar theory is still incomplete, and that it is impossible by any tables yet made to represent accurately the whole series of observations. A value of the secular acceleration which suits the observations of the last 200 years will not fit the Arabian observations made 1000 years ago, nor will it satisfy the eclipse observations of still more ancient date, unless at least the received interpretation of those ancient eclipses be admitted to be wrong, as Prof. Newcomb seems to consider rather probable. From his discussion he derives for the secular acceleration a value of 8″·4, as against the value of 12″·1, deduced by Hansen.

It will be remembered probably by every one present that the *theoretical* value of this quantity is about 6″, and that Ferrel, Adams, Delaunay, and others, attributed its apparent increase to 12″ to the action of the tides in retarding the earth's rotation and so lengthening the day; if Newcomb's value is correct, this tidal retardation is cut down from 6″ to about 2″·5.

The study of the moon's surface has been carried on with assiduity, but I do not know that any remarkable results have been reached, though Klein's observation, in 1877, of what he supposed to be a newly-formed crater (Hyginus N.), excited a good deal of interest and discussion for a number of years; and the most eminent selenographers are still divided in opinion on the question.

The publication by the German Government of Schmidt's great map of the moon, in 1878, unquestionably marks an epoch in selenography; and the photographic work of Pritchard, and the heliometric determination of the moon's physical libration by Hartwig, must not pass unnoticed.

Probably, however, the lunar work which has drawn to itself most attention and interest is the investigation of the moon's heat by Lord Rosse and Prof. Langley.

The earliest observations of the kind date back now forty years, when Melloni, in 1846, first detected the moon's heat by means of the then newly-invented thermopile. But the first really scientific *measurements* are only about fifteen years old, due to Lord Rosse, at Parsonstown, and to Marie Davy, at Paris; and they seemed to show that at the time of full moon we receive from our satellite, not merely *reflected* heat, but warmth *radiated* from the moon's surface; as if this surface were raised to a considerable temperature by the long insolation to which it has been exposed during the preceding fortnight. Lord Rosse estimated the probable temperature of this heated rock to be as high as from 300° to 500° F.

But within the past four or five years this conclusion has been called in question. Observations at Parsonstown, of the rapid diminution of radiation during a lunar eclipse, seem to favour the newer view that the moon's surface, like that of a lofty mountain-top on the earth, never gets very hot, since the absence of air enables the solar heat to escape nearly as fast as it is received.

Prof. Langley's recent and still progressing work upon this subject far excels in delicacy and elaborateness anything done before. At first it seemed to show that the temperature of freezing water was never reached even at the hottest parts of the lunar surface; but the later observations throw some doubt on the legitimacy of this inference. It is found that the radiation from the moon unquestionably contains a considerable percentage of rays which have a wave-length *longer* than any of the heat-rays from melting ice; and this fact has been supposed to make it probable that the moon's surface was colder than the ice. But then, within a few weeks, Prof. Langley has found the long-waved rays in the radiation from an *electric arc!* So the question still hangs debatable.

The Sun's Parallax.—I think we may say that, during the past ten years, substantial progress has been made with the problem of the solar parallax. The transit of Venus in 1882 adds whatever value its results may have to those obtained eight years before; but, on the whole, so far as can be judged from the reductions thus far completed and published, it would seem likely that the outcome of the transit observations will be simply to confirm the results obtained by other methods. It may be that the data obtained from the German heliometer measurements will prove more accordant and decisive than those derived from photographs and from the contact observations; there are flying rumours that they will, but it will be necessary to await the official publication for certain knowledge on this point. If they do not, we shall be obliged, hereafter, to relegate transit observations to a secondary rank, as a means of determining the sun's distance. From the various observations of the two transits, different computers have deduced values of the parallax all the way from 8″·6 to 8″·95, corresponding to a distance ranging from 95,000,000 to 91,500,000 miles.

The case is quite different with the heliometer observations of the opposition of Mars, in 1877, made by Mr. Gill at Ascension Island. These give, in a most definite and apparently authoritative manner, a value of 8″·783, and are apparently irreconcilable with any value much greater than 8″·81, or less than 8″·75. So far as can be judged from the number, nature, and accordance of the observations, I believe we must accept this as the most trustworthy of the geometrical methods yet employed; though the weight of the result would certainly be increased if it did not depend to such an extent upon the work of a single individual.

The confidence of astronomers in the correctness of this value is greatly fortified by the fact that the most recent and reliable determinations of the velocity of light, made by Michelson and Newcomb, in 1877, 1880, 1881, and 1882, when combined with the Pulkowa constant of aberration determined by Nyrèn from all the data available up to 1882, give a solar parallax accordant with the preceding almost to the hundredth of a second—8″·794 as against 8″·783. It is true there are possible theoretical objections to the method; as, for instance, that the result may be slighted affected by the motion of the solar system through space. Enough is not known certainly about the constitution of the medium that transmits light through space, to decide all such questions *a priori* and authoritatively; but it is unquestionable that any correction needed on account of such possible causes of error must be very minute.

We believe, therefore, that it is safe to assume pretty confidently that the solar parallax is about 8″·8 (though probably a trifle less), which makes the sun's mean distance 93,000,000 miles, with an error not likely much to exceed 150,000 miles. A larger value of the parallax (about 8″·85) still holds its ground in the nautical almanacs, and undeniably is nearer the *average* of the results given by *all* the known methods. But none of the other methods seem to us to compete at all in precision with the two whose authority we accept.

The Sun and Meteorology.—The study of the solar surface has been carried on very persistently by Spörer, in Germany, as well as by others, and a great amount of material has been collected bearing upon the theory and nature of sunspots, and their periodicity. The extensive series of photographs obtained at Kew, and at Dehra Doon, in India, constitutes almost a continuous record of the solar surface for several years. The relation between this periodicity and terrestrial conditions has been assiduously examined, but on the whole the outcome seems to me to leave this connection as doubtful as it ever was, in most cases at least. While in some parts of the earth it looks as if there were a slight but marked increase of storm and rainfall at the time of sunspot maximum, the reverse seems to be true in other countries. In South America, Dr. Gould thinks that he has demonstrated a very perceptible effect of the condition of the sun's surface in modifying the strength and direction of the winds; but thus far similar investigations elsewhere show no such result. It will evidently be necessary to wait for a longer and more widely extended collection of statistics to settle the question. We do not even know as yet whether we get more or less than the average heat from the sun during the sunspot maximum.

But I think it may be set down as certain that the condition of the sun's surface exerts, if perhaps a real, yet only a very slight effect upon our earthly meteorology. With terrestrial magnetism the case is markedly and singularly different, and one of the most interesting problems now pressing for solution is the nature of the connection between solar disturbances and magnetic storms.

Solar Heat.—A great deal of labour has been expended upon the study of the sun's heat during the last decade. The investigations that strike me on the whole as most worthy of mention are those of our own Langley and of the Italian Rosetti, whose early death a few months ago is a great loss to science. Secchi and Ericsson, on the one side, had contended for a solar temperature of some millions of degrees, basing their results on Newton's law of cooling; while, on the other, Crova and Violle, from their measures of the solar radiation, reduced according to the so-called law of Dulong and Petit, maintained that the temperature does not much exceed that of many terrestrial furnaces, somewhere from 1500° to 2500° C. Rosetti's experiments upon the radiation of the electric arc and other sources of intense heat showed pretty clearly the inapplicability of Dulong and Petit's law to high temperatures, and indicate a solar temperature not far from 10,000° C., or 18,000° F. But they also make it clear that the limits of uncertainty are still very great.

Prof. Langley, by his invention of the bolometer, has been able to investigate separately the amount of energy transmitted to the earth in the solar rays of every possible wave-length, and to determine the effect of our atmosphere in absorbing each kind of ray. He has shown that the older method of investigating this solar radiation, *in a lump* so to speak, gives fallacious results on account of atmospheric absorption; and that the necessary correction compels us to increase our estimate of the sun's energy at least 20 per cent. In my own little book upon the sun, published in 1881, I had set the so-called solar constant at twenty-five calories per square metre per minute. It is now certain that it must be put at least as high as thirty. Prof. Langley's investigations seem also to show another remarkable fact—that we do not receive from the sun any at all of the low-pitched, slowly-pulsing waves, such as we get from surfaces at or below the temperature of boiling water. The solar spectrum appears to be cut off abruptly at the lower end; and this cutting off we know cannot have been effected in the earth's atmosphere, because we receive from the moon in considerable quantity just this very sort of low-pitched rays. Langley finds them also abundant in the radiation of the electric arc, so that we can hardly suppose them to be *originally* wanting in the solar heat. It now looks as if we must admit that they have been suppressed either in the atmosphere of the sun itself, or in interplanetary space. Another striking conclusion first clearly pointed out by Langley is that, if the sun's atmosphere were removed, its light would be strongly blue.

The Solar Surface and Spots.—As regards the general make-up of the solar surface, I do not think there has been any new fact of extreme importance brought out within ten years. Janssen has, however, carried solar photography to higher excellence than ever attained before, and has obtained plates that show the "granules" and their grouping on a scale previously unknown. He thinks that his plates prove a peculiar constitution of the solar surface, consisting in collections of clearly-defined and rounded granules, separated by regions or streaks where they are ill defined and elongated; and he calls the phenomenon the "reseau photosphérique," or photospheric network. According to him the "net" remains approximately constant for some minutes at a time, as shown by plates taken in quick succession, but is subject to rapid and enormous changes in periods exceeding a quarter of an hour or so. I find some scepticism among high authorities as to the trustworthiness of his conclusions. There are suggestions that the appearances presented may be due to currents of air in the telescope tube and at the surface of the sensitive plate; but I am disposed to think he is right, for, on several occasions when the seeing has been exceptionally fine, I have observed with my own eyes something quite analogous, in our large telescope at Princeton.

The spots have been carefully studied by several observers, by Spörer especially, in a statistical way, and by Vogel, Lohse, Tacchini, and others, as to structure and detail. Spörer has brought out very clearly the connection between the number and average latitude of the spots. It appears that, speaking broadly, the disturbance which produces the sunspots begins in two belts on each side of the sun's equator in a latitude of over 30°; these belts or spot-zones then gradually move in towards the equator, the sunspot maximum occurring when their latitude is about 16°, while the disturbance gradually and finally disappears at a latitude of 8° or 10°, some twelve or fourteen years after its first appearance. But two or three years before this disappearance a new zone of disturbance shows itself in the same latitude as its predecessor, so that for a while, about the time of sunspot minimum, there are two well-marked zones of spots on each side of the sun's equator—one pair near the equator, due to the expiring disturbance which began some ten or eleven years ago; the other far from the equator, and due to the newly-arising outburst, which will reach its maximum in three or four years, and then pass away like the former.

There can be no doubt that the phenomenon is a very significant one, but its explanation, like that of the periodicity itself, is still to be found.

Nor is the problem of the spots themselves yet fully solved. Not that there is any reasonable question that they are *hollows* in the solar photosphere; but how they originate, how deep they are, and what are the causes of their darkness, and the condition and temperature of the darkening substance—these are questions to which only uncertain answers can now be given. A long and important series of observations upon the widening of the lines of certain elements in the sunspot spectra has been made by Mr. Lockyer, and establishes clearly the fact that those lines, of *iron* for instance, which are conspicuously black and wide in the sunspots, are often just those which do *not* show themselves conspicuously in the prominences; and moreover both in spots and prominences the iron lines that do show themselves are most frequently those which closely coincide with lines in the spectra of other substances. Singularly, also, and so far quite without explanation, it appears according to his observations that at the sunspot maximum those *iron* lines which at other times are conspicuous in spot-spectra entirely disappear.

Perhaps I may be allowed to mention here a recent observation of my own upon these spot-spectra: with a high dispersion the darkest part of the spot-spectrum is found to be not continuous, but made up of fine lines overlapping or almost touching each other, with here and there a clear space left, like a fine bright line. It means, I think, that the absorbing vapours which darken the interior of the spot are wholly gaseous, and tends to disprove the idea that they are mostly of the nature of smoke or steam. We mention also, in passing, another thing which has been shown by our large instrument at Princeton—that the apparently bulbous, finger-tip-like terminations of the penumbral filaments are often, under the best circumstances of vision, resolved into fine, bright, sharp-pointed hooks which look like the tips of curling flames.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At the biennial election of members of the Council of the Senate, Prof. Michael Foster and Dr. Donald MacAlister were elected to serve for four years.

SOCIETIES AND ACADEMIES

London

Mathematical Society, November 11.—Mr. J. W. L. Glaisher, F.R.S., President, in the chair.—Mr. F. S. McAulay, St. Paul's School, was elected a Member.—The following gentlemen were elected to form the Council for the ensuing session:—President: Sir J. Cockle, F.R.S.; Vice-Presidents: J. W. L. Glaisher, F.R.S., Prof. Harry Hart, and the Right Hon. Lord Rayleigh, Sec.R.S.; Treasurer: A. B. Kempe, F.R.S.; Hon. Secretaries: Messrs. M. Jenkins and R. Tucker; other Members: Prof. Cayley, F.R.S.; E. B. Elliott, Prof. Greenhill, J. Hammond, Prof. M. J. M. Hill, C. Leudersdorf, Capt. Macmahon, R.A., S. Roberts, F.R.S., and J. J. Walker, F.R.S.—The retiring President, J. W. L. Glaisher, F.R.S., delivered an address, which treated of the Mathematical Tripos Examinations at Cambridge, and of the bearings of recent changes in the same upon the advancement of mathematics.—The following communications were made:—Certain operators in connection with symmetric functions, by R. Lachlan.—The transformation of a certain quartic elliptic element, by R. Russell.—Discussion of a multilinear operator, with applications to the theories of invariants and reciprocants, by Capt. Macmahon.—The theory of screws in elliptic space (fourth note), by A. Buchheim.—The rectification of certain curves, by R. A. Roberts.—Rectification of a sphero-conic, by H. F. Burstall.—Third paper on reciprocants, by L. J. Rogers.—The "sine-triple-angle" circle, by R. Tucker.

Linnean Society, November 4.—Mr. William Carruthers, F.R.S., President, in the chair.—The President paid a passing tribute to, and commented on the loss sustained in the death of, Mr. George Busk, a former Secretary and Vice-President of the Society. Afterwards he drew attention to phosphorescent organisms obtained by him in the Firth of Clyde last September.—Mr. John Murray also made remarks on the same, alluding to his own observations of *Ceratium tripos* being found in long chains in the ocean ("Narrative of the Cruise of the *Challenger*"), and to Klebs's opinion of *Ceratium* being a genus of unicellular Algæ, and not an infusorian.—Prof. J. Macoun made remarks on a series of cones of Canadian *Piceas*. He showed that the various forms occurring from east to west of the continent, which had been hitherto considered different species, were doubtless local varieties of but one species, slightly modified according to the altitudes and region they inhabited.—Dr. F. Day exhibited a salmon parr, twenty months old, raised at Howietoun from parents which had never visited the sea. He also showed coloured drawings of hybrids raised in the same establishment—one being a cross between the American charr and the Loch Leven trout, a second between the American and the British charr, and a third between the last-mentioned hybrid and the Loch Leven trout; all were fertile.—Fresh specimens of a white variety of *Crocus nudiflorus* from Biarritz, France, were shown for Mr. W. D'Arcy Godolphin Osborne, who first observed the variety there in 1882, but since then it has been figured by Mr. G. Maw in his monograph of the genus.—Mr. E. M. Holmes exhibited examples of *Lycoperdon echinatum*, Pers., viz. the young plants, and the reticulate appearance of the peridium left by the falling off of the spines.—Mr. F. Pascoe exhibited one of the round olive green balls from Sicily, formed by the action of the sea on fragments of the *Posodonia caulinia*, and reduced, after a few days' exposure, to a flat cake-like body densely covered with minute crystals of salt. He also showed some acorn-shells (*Balanus*), where several individual animals had united their shells into a common tube, and where the outer valves of each animal had lengthened, forming a series of irregular subsidiary tubes radiating from the apex of the primary one.—Mr. E. C. Bousfield read a paper on the natural history of the genus *Dero*. In this he gives a full account of their habits and the best method of observing them. The *Naias digitata*, Müll., he rejects as a specific appellation, Müller's reference being defective. He shows wherein the Deros differ from the Naiades, viz. in the former having a respiratory apparatus at the end of the tail. He diagnoses seven species, four being new; all are figured.—Mr. S. O. Ridley gave in abstract his researches on the genus *Lophopus*, and description of a new species from Australia. This latter, *L. lendenfeldi*, obtained by Dr. Lendenfeld near Sydney, N.S.W., is distinguished from *L. crystallinus* by length of tentacles equalling the body of the polypide, and by the non-pointed outline of the statoblast. The new species is the fourth fresh-water Polyzoon recorded from Australia, and the first of its genus satisfactorily determined from the southern hemisphere. Staining with borax-carmine brings out multipolar cells in the ectocyst, indicating mesodermal elements, and denoting that the ectocyst is something more than mere epithelium. A modification of the diagnosis of the genus is necessitated from these observations.

Chemical Society, November 4.—W. Crookes, F.R.S., Vice-President, in the chair.—Messrs. H. Crompton, G. Dyson, T. B. Tyson, and S. Williamson were admitted Fellows of the Society.—The following papers were read:—The action of chlorosulphonic acid on naphthalene-α- and β-sulphonic acids, by Henry E. Armstrong and W. P. Wynne, B.Sc.—The action of bromine on (Schäfer's) β-naphtholsulphonic acid, by Henry E. Armstrong and F. W. Streatfeild.—The action of bromine on the naphthalenesulphonic acids, by Henry E. Armstrong and W. P. Wynne, B.Sc.—α-Nitro-, α-bromo-, and α-chloronaphthalenesulphonic acids, by Henry E. Armstrong and S. Williamson.—The hydrolysis of sulphonic acids, by A. K. Miller, Ph.D.—The action of bromine on tolueneparasulphonic acid, by A. K. Miller, Ph.D.—Phosphorus tetroxide, by T. E. Thorpe, F.R.S., and A. E. Tutton.—Conversion of ditolane-azotide into diphenanthrylene-azotide, by Francis R. Japp, F.R.S., and Cosmo Innes Burton, B.Sc.—A chemical study of vegetable albinism; part 3, experiments with *Quercus rubra*, by A. H. Church.—The synthetical formation of closed carbon-chains; part 2, some derivatives of tetramethylene, by W. H. Perkin, Jun., Ph.D.—The action of the halogens on the salts of organic bases; part 2, tetramethylammonium salts, by Leonard Dobbin, Ph.D., and Orme Masson, M.A., D.Sc.—Glycyphyllin, the sweet principle of *Smilax glycyphylla*, by Edward H. Rennie, M.A., D.Sc.

Entomological Society, November 3.—Mr. Robert McLachlan, F.R.S., President, in the chair.—The following gentlemen were elected Fellows:—Messrs. P. Cameron, F. Archer, H. J. S. Pryer, H. Norris, N. P. Fenwick, J. Brown, J. P. Tutt, and A. P. Green.—Mr. E. B. Poulton exhibited a mass of minute crystals of formate of lead, caused by the action of the secretion of the larva of *Dicranura vinula* upon suboxide of lead. He stated that a single drop of the secretion had produced the crystals which were exhibited; and he called attention to the excessively high percentage of formic acid which must be present in the secretion.—Mr. S. Stevens exhibited a specimen of *Laphygma exigua*, recently captured by Mr. Rogers in the Isle of Wight.—Mr. W. F. Kirby exhibited, and read notes on, a specimen of *Perilampus maurus* recently bred by Mr. Walter de Rothschild from *Antheræa tirrhea*, one of the rarer South African Saturnidæ.—Mr. T. W. Hall exhibited a number of specimens of *Xanthia fulvago* (*cerago*), somewhat remarkable in their variation, and showing a graduated series, extending from the pale variety, *flavescens* of Esper, to an almost melanic form.—Mr. Boyd exhibited, and made remarks on, the larva of a species of *Ornithoptera* from New Guinea.—Mr. H. Goss exhibited a series of *Bankia argentula* collected by him in Cambridgeshire in June last; and also, for comparison, a series of specimens of the same species taken at Killarney in June 1877. It appeared that the Irish form of the species was larger and more brightly coloured than the English form.—Mr. Eland Shaw exhibited a female specimen of *Decticus verrucivorus* taken in July last at St. Margaret's Bay, Kent.—Mr. Waterhouse recorded the recent capture of *Deiopeia pulchella* at Ramsgate, by Mr. Buckmaster; and the capture of *Anosia plexippus* at Gibraltar was also announced.—Mr. J. W. Slater read a paper on the relations of insects to flowers, in which he stated that many flowers which gave off agreeable odours appeared not so attractive to insects as some other less fragrant species; and he stated that Petunias, according to his observations, were comparatively neglected by bees, butterflies, and Diptera. Mr. Distant, Mr. Stainton, Mr. Weir, Mr. Stevens, and the President took part in the discussion which ensued, and stated that, in their experience, Petunias were often most attractive to insects. Mr. Stainton referred to the capture, by himself, of sixteen specimens of *Sphinx convolvuli* at the flowers of Petunias, in one evening in 1846.—Jonkeer May, the Dutch Consul-General, asked whether the reported occurrence of the Hessian fly (*Cecidomyia destructor*) in England had been confirmed. In reply, Mr. McLachlan stated he believed that several examples of an insect, thought to be the Hessian fly, had been bred in this country, but that everything depended upon correct specific determination in such an obscure and difficult genus as *Cecidomyia*.

EDINBURGH

Mathematical Society, November 12.—Dr. R. M. Ferguson, President, in the chair.—The President gave a retiring address, for which, and for the gratuitous use of the rooms of the Edinburgh Institution for the Society's meetings a vote of thanks was awarded to him.—Mr. J. S. Mackay read a paper on the solutions of Euclid's problems with a ruler and one fixed aperture of the compasses by the Italian geometers of the sixteenth century; and communicated a note from Mr. R. Tucker giving some novel properties connected with the triangle.—Mr. A. Y. Fraser read a note by Mr. William Renton on the equivocal sign.—The following office-bearers were elected for the session:—President: Mr. George Thom; Vice-President: Mr. W. J. Macdonald; Secretary: Mr. A. Y. Fraser; Treasurer: Mr. John Alison; Committee: Mr. R. E. Allardice, Dr. R. M. Ferguson, Mr. George A. Gibson, Mr. William Harvey, Mr. J. S. Mackay, Mr. Thomas Muir.

SYDNEY

Royal Society of New South Wales, September 1.—Mr. Ch. Rolleston, President, in the chair.—Mr. Fredk. B. Gipps, C.E., read a paper on "Our Lakes and their Uses." The lakes of New South Wales being all liable to dry up, Mr. Gipps stated that it is possible, however, to impound large artificial lakes. The leading features of Lake George were described, and a means of utilising its waters for irrigating a large area were entered into.—A very beautiful specimen of gold from calcite was exhibited by Dr. Leibius, of the Mint. The lime having been dissolved in acid, the gold was left as a network of the finest ramifying filaments.

PARIS

Academy of Sciences, November 8.—M. Jurien de la Gravière, President, in the chair.—On the relations of geodesy and geology, by M. Faye. This is a reply to M. de Lapparent's recent criticisms of the author's well-known views on the relations of the geodetic and geological sciences. M. de Lapparent's objections are treated in detail, and it is argued that the law of unequal cooling of the terrestrial crust dates back to times anterior to the astronomico-geological epoch, when the seasons began to be established. It controlled the whole series of geological evolutions from the first formation of the oceanic basins.—Thermic researches on the reactions between ammonia and the magnesian salts, by M. Berthelot. These studies tend to define the action of ammonia on the magnesian salts, determining the analytical conditions which enable magnesia to be separated from the other alkaline salts, and showing that, by union with sulphuric acid or with hydrochloric acid, the complex ammoniaco-magnesian base liberates a quantity of heat greater than pure ammonia or pure magnesia, and very near that liberated by potassa and soda.—On the incandescent substance in fusion recently reported to have fallen during a thunderstorm at Luchon, by M. A. Trécul. In connection with M. St. Meunier's remarks on this subject, the author refers to a communication made by him to the Academy in 1881 (*Comptes rendus*, xcii. p. 775), showing that in thunder-clouds there may exist an incandescent substance in fusion, which under certain conditions may fall to the ground in the form of variable drops or globules.—Report made on behalf of the Section for Chemistry on M. Moissan's researches relating to the isolation of fluor, by M. Debray. After describing the attempts made by Davy and subsequent chemists to solve this problem, the report gives a detailed account of M. Moissan's researches, and considers his final conclusion fully justified, that the gas liberated by the electrolysis from the pure anhydrous hydrofluoric acid, with which he experimented, is undoubtedly fluorine. The question consequently now enters on a new phase, and chemistry may henceforth deal directly with fluorine, and attack problems of great interest, formerly regarded as insoluble.—Observations of the new planet 261, made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan.—On an extended class of uniform transcendants, by M. H. Poincaré.—On Maclaurin's series in the case of a real variable: application to the development of a homogeneous body in series of the potential, by M. O. Callandreau.—Note on the octahedron, by M. P. Serret.—On the transport of force; a reply to M. Deprez, by M. Hippolyte Fontaine. It is admitted that the principle of a series of machines linked together is not new; but the author claims that the results of his researches obtained by the employment of special dynamos, and by a rearrangement of the mechanical elements, must be regarded as new. The transport of 50 horse-power through a resistance of 100 ohms with a loss of only 48 per cent., by employing dynamos jointly weighing only 8400 kilograms and costing only 658*l.*, is now realised for the first time.—Determination of the heats of neutralisation of the malonic, tartronic, and malic acids: remarks on the heats of neutralisation of the homologous acids of oxalic acid and of the corresponding hydroxyletted acids, by MM. H. Gal and E. Werner. The results here tabulated of the authors' researches show that the heat of neutralisation of the homologous bibasic acids under consideration diminishes in proportion as the molecular weight increases.—General methods of crystallisation by diffusion; reproduction of mineral species, by M. Ch. Er. Guignet. These experiments are described as a generalisation of M. Becquerel's ingenious researches on the slow action set up between two liquids separated by a film, a porous wall, or even a glass tube with a fissure or capillary aperture. The methods employed are applicable to a large number of bodies, and yield crystals in any required quantities.—Synthesis of conicine, by M. A. Ladenburg. The processes are described by which the author has succeeded in obtaining synthetically three bases possessing the same mutual relations as racemic and tartaric acids, one of which is absolutely identical with natural conicine.—On the chemical transformations brought about by the action of solar light, by M. E. Duclaux. Having already examined the sterilising action of solar light on microbes, the author here proceeds to show that the influence at play belongs to the order of chemical phenomena, which in this case assume a physiological character. The action of the solar rays is shown to be analogous to that of the ferments.—On a new means of preventing secondary fermentations in the alcoholic fermentations of commerce, by MM. U. Gayon and G. Dupetit. This process consists in adding to the wort antiseptic substances capable of arresting the development of the countless germs contained in the primary substances and in the yeast, without, however, impairing the activity of the leaven itself.—On the alcoholic fermentation of dextrine and of starch, by MM. U. Gayon and E. Dubourg. A new kind of mucor is described, which possesses the twofold property of fixing the water on dextrine, and even on starch, and superinducing fermentation in the products of this saccharification, without, however, affecting cane-sugar or transforming it to alcohol.—On the reduction of the sulphate of copper during vinous fermentation, by M. H. Quantin.—On the genus Cepon, by MM. A. Giard and J. Bonnier. Two new species (*C. pilula* and *C. elegans*) of this little-known genus have been discovered by the authors, the former a parasite of *Xantho floridus*, the latter of *Pilumnus hirtellus*. Both occur on the French seaboard.—On the homologies of the larvæ of Comatulæ, by M. J. Barrois.—On the cave-dwellers of Bêche-aux-Roches, by MM. Marcel de Puydt and Max. Lohest. The authors disclaim all responsibility for the recent remarks of M. de Nadaillac describing the culture of this prehistoric race in somewhat exaggerated language.—On the affinities of the Eocene floras of West France and Saxony, by M. Louis Crié.—On a serious malady analogous to scurvy observed in certain reptiles, by M. Magitot.—On a technical process for the diagnosis of Gonococcus, whereby this parasite may be distinguished from other species of cocci, by M. Gabriel Roux.

BERLIN

Meteorological Society, November 2.—Prof. von Bezold in the chair.—Prof. Spörer spoke of stormy movements in the atmosphere of the sun. He discussed a long series of details respecting the physics of the sun, which were illustrated by heliographic maps, and emphasised the fact that spots invariably appeared only in very hot luminous regions of the solar surface. Of his theoretical explanations it may be more particularly mentioned that, in the opinion of the speaker, the luminous regions originated in the ascending of gases and vapours from the hot interior of the sun. When such a thing happened at the circumference of the sun, then metallic prominences were observed. In consequence of their higher temperature the luminous regions caused ascending currents, whither cooler gases streamed from all sides. The gases, which in certain circumstances mounted to heights of 30,000 German (135,000 English) miles, cooled, sank as cooler masses endowed with greater linear velocity to the same localities, and there formed the sunspots. In the discussion following this address, Prof. Spörer stated that according to his observations the last maximum had shown itself 1884·0. He further stated that occasionally, under special conditions of illumination, he had, with the aid of the telescope, seen in clouds small, round bodies moving up and down, which he had taken for rain-drops, and commended to those interested in the study of the atmosphere such observations of clouds. Respecting the possibility of seeing the rain-drops of clouds in this manner there arose a lengthy discussion.

Physical Society, November 4.—Prof. von Helmholtz in the chair.—Prof. Spörer produced and made the subject of discussion a long series of heliographic maps which he had drawn from phenomena he had himself witnessed, and which demonstrated in a very graphic manner the occasionally very important proper motions of different spots. These self movements always occurred on the west side of the spots, and of the groups of spots. They always followed therefore in the sense of the sun's rotation. They were recognised when the spots were observed several times in the course of a day, and they sometimes attained values of from 1000 to 2000 geographical miles in one day. These movements were specially intense in the case of the formation of larger spot-groups; later on they grew slower. For the explanation of these proper motions, the speaker adduced that sunspots invariably formed themselves only over luminous surfaces, that is, at spots of the solar surface possessing a higher temperature. In his measurements of temperature, which had not yet been published, having reference to the year 1880, he made use of a thermo-element on which, through a fine opening in a thick pasteboard disk, he caused to fall the position of the sun's image which he wanted to measure. According to these observations, the emission of heat from a spot-umbra stood to the radiation of heat from a luminous surface as 10 : 18, and the radiation of a spot-umbra to the radiation of the usual solar surface as 10 : 15. Seeing that the temperatures on the sun stood probably in the same relation as did the radiations, so in the luminous surfaces which possessed a higher temperature (in the relation of 6 : 5) must an ascending gas-current develop, to which a descent of colder gas-masses must necessarily correspond. These descending colder gases it was which generated the spots, and gave them—seeing they possessed a greater linear speed of rotation than did the solar surface—a displacement towards the west in the sense of the rotation.—Dr. Pernet spoke on the determination of the air in the vacuum of the barometer, in accordance with the Arago method, connecting his observations with a publication by Dr. Schreiber, who, on comparing the barometer of the Saxon station with the normal barometer, found, after taking due account of all corrections in the latter, volumes of air far surpassing the permissible quantities. Dr. Pernet had now found that two very essential corrections were overlooked: first, the determinations of the air in vacuum under the pressures 0, 40, and 80 millimetres, were carried out in much too rapid succession, so that compensations of temperature were impossible; second, the effect of the capillarity was not observed, an effect which in the case of syphon barometers played so far a great part, as the lower surface of the quicksilver affected by oxidation and dust had a different surface-tension and different angles of rim from the upper surface of the quicksilver, which was comparatively pure. The registrations were therefore not exact if the menisci were not simultaneously measured. This tension of the surface was in the case of thermometers also very important. In consequence of it, the readings of thermometers with narrow tube and less mass of quicksilver were less exact than the readings of thermometers with wider tube and more quicksilver. It was the cause that thermometers with elliptic tubes were less exact than thermometers with circular ones. The effect of the capillarity, again, was, in the opinion of the speaker, the cause of the "dead point" of Mr. Pickering.

VIENNA

Imperial Academy of Sciences, October 7.—On Hall's phenomenon, by A. von Ettingshausen and W. Nernst.—On the data wanted for proving Avogadro's law, by L. Boltzmann.—On the theory of the electro-magnetic phenomenon discovered by Hall, by the same.—On the density of liquefied methene and liquefied oxygen, by K. Olszewski.—On the comets discovered by Mr. Finlay on September 26, and by Dr. Hartwig on October 6, by E. Weiss.—On colchicine, by S. Zeisel.—Contributions to the knowledge of the Tertiary flora of Australia, second paper, by C. von Ettingshausen.

October 14.—Researches on strychnine, especially on the action of zinc-dust on strychnine, by W. F. Loebisch and P. Schoop.—A preliminary communication on the statistics of comets, by T. Unterweger.

October 21.—To histology and physiology of mucous secretion, by W. Biedermann.—Remarks on L. Hermann's galvanotropic experiment, by E. Mach.—On hydrocarotin and carotin, by F. Reinitzer.—On the anatomy and systematics of gall-mites, by A. Nalepa.

BOOKS AND PAMPHLETS RECEIVED

Encyklopædia der Naturwissenschaften, Erste Abtheil., 48-49 Lief.; Zweite Abtheil., 37-38 Lief. (Trewendt, Breslau).—Index Catalogue of the Library of the Surgeon-General's Office, U.S. Army, vol. vii. (Washington).—Proceedings of the Linnean Society of New South Wales, 2nd series, vol. i., part 2 (Cunningham, Sydney).—Quarterly Journal of Microscopical Science, October (Churchill).—Alpine Winter, 3rd edition: Dr. A. T. Wise (Churchill).—Encyclopædia Britannica, vol. xxi. (Black, Edinburgh).—Structure and Life-History of the Cockroach: L. Miall and A. Denny (L. Reeve).—Madagascar, 2 vols.: Capt. S. P. Oliver (Macmillan).—Journal of the Anthropological Institute, November (Trübner).—First Year of Scientific Knowledge, 3rd edition: P. Bert (Relfe).—Nouvel Atlas Céleste: R. A. Proctor; translated into French by P. Gérigny (Gauthier-Villars, Paris).—Ordnance Survey of the United Kingdom: Lieut.-Col. White (Blackwood).—La Photographie sans Objectif: R. Colson (Gauthier-Villars, Paris).—L'Aurora Boréale: M. S. Lemström (Gauthier-Villars, Paris).—Les Hypothèses Cosmogoniques; Examen des Théories Scientifiques Modernes sur l'Origine des Mondes, suivi de la Traduction de la Théorie du Ciel de Kant: C. Wolf (Gauthier-Villars, Paris).—Hand-book of Jamaica for 1886-87: A. C. Sinclair and L. R. Fyfe (Stanford).—Quarterly Journal of the Royal Meteorological Society, October (Stanford).—Monthly Results of Observations made at the Stations of the Royal Meteorological Society, vol. vi., No. 22 (Stanford).

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