THURSDAY, JANUARY 12, 1888.

PHYSICAL CHEMISTRY.

Lehrbuch der Allgemeinen Chemie. Von Dr. Wilh. Ostwald. In Zwei Bänden. (Leipzig: W. Engelmann, 1885–87.)

THE larger text-books of chemistry have generally been devoted to describing and roughly classifying the facts which form the foundation of the science. These facts are so numerous, varied, and important, that when one has spent years in arranging, cataloguing, and reciting them, his chemical vision has generally acquired a fixed downward direction, and he is almost unable to lift his eyes from the foundation-stones to look on the buildings which other workers have been raising.

But, whether such a one will look at the building or not, the building is surely rising. The walls already are massive; there are adornments of conceits, perhaps sometimes too quaint; windows there are in plenty to admit light and air: the house will never be completed, because nature is inexhaustible, but even now there is promise of a goodly building. Nor shall the House Beautiful want fit interpreters, among whom an honourable place will be held by the Professor of Physical Chemistry at Leipzig.

It has generally been admitted that chemistry is a branch of physical science. Individual chemists by their researches have shown that the relation of chemistry to physics is that of the less to the greater; but most of the attempts to set forth this relationship in its entirety have failed. To treat chemistry as a branch of physics requires one who is almost as much a physicist as a chemist, but one whose physical training has waited on his chemical judgment. Some books on physical chemistry have been books on descriptive chemistry, with scraps of physical facts thrown in; others have been books on physics to which the use of chemical illustrations has given an ill-defined but not unpleasing chemical tone. Only of late years has it become possible to set forth the connections between the parent science and the greatest of her children in a fairly satisfactory manner; and this possibility has come through the recent advances made in the study of these connections.

It was therefore fitting that one of the men whose work forms no small part of all of first-class importance that has been done in recent years in the sphere of physical chemistry should be the man to write the first good text-book on general chemistry considered as a branch of physics. Ostwald prefers to call his work "Lehrbuch der Allgemeinen," rather than "physikalischen," "Chemie." The title very happily expresses the scope and character of the book; but the treatment of chemical principles in a general manner is made possible in this treatise by regarding chemistry as a special branch of physics. The book is intended for fairly advanced students who have already a tolerable knowledge both of descriptive chemistry and of physical principles. Some of the higher forms of mathematical analysis are freely employed. The form in which the author has chosen to present his treatise is the historical-critical; he justly remarks that the historical coincides with the logical development of many chemical ideas.

As the object of the work is to enable the student to gain a firm hold of the principles of chemistry, and more especially to teach him that very many of these principles have been reached by the application of physical methods to chemical phenomena, much care is taken to distinguish generalized statements of facts from hypotheses, to indicate the need of using hypotheses, to trace the merging of several hypotheses into one general theory, and to avoid mere speculation.

The first volume is devoted to stochiometry. The laws of chemical combination, which form the basis of the whole science, are laid down in a singularly clear and succinct manner; the atomic theory of Dalton is sketched; the chemical methods by which combining weights are determined are classified, and this is followed by a short critical exposition of the results obtained for each element. The second, third, and fourth books of the first volume are devoted to accounts of the properties of gaseous, liquid, and solid bodies, respectively. The relations between the volume, temperature, and pressure of gases, are considered; this leads to a statement of the law of Gay-Lussac, and a consideration of Avogadro's hypothesis; then follows an account of the kinetic theory of gases, the specific heats, and the optical properties of gases. The book on liquid bodies is devoted to a consideration of (1) the general properties of liquids; (2) the relations between the liquid and gaseous states; (3) the volume-relations of liquids; (4) solution; (5) optical properties of liquids; (6) capillarity, diffusion, and osmosis; (7) electrical conductivities and electrolysis of liquids; (8) specific heats of liquids. The book on the stochiometry of solid bodies includes the consideration of crystallography, especially in its chemical bearings, the optical and electrical properties of solids, &c. The first volume concludes with a sketch of the relations between atomic weights and chemical properties, a general account of the molecular theory as applied in chemistry, and a short but very suggestive chapter on theories of chemical composition and constitution.

The second volume deals with the vast and widely ramifying subject of chemical affinity. The first part, on chemical energy, comprises what is really a comprehensive treatise on thermo-chemistry, and also full critical accounts of photo-chemistry and electro-chemistry. The second part, dealing more distinctly with chemical affinity, begins with an historical sketch; this is followed by about 150 pages on chemical dynamics; and the whole concludes with an account of the various methods whereby measurements of the relative affinities of various bodies, especially acids and bases, have been obtained; the last chapter deals with the relations between the nature, composition, and constitution of bodies, and the values of their affinity-constants.

Ostwald has undertaken and brought to a conclusion a task of great difficulty. His book has removed the sting from the taunt so often cast at the chemist that chemistry is the pursuit of the mere fact-finder and formula-monger. If Ostwald's "Lehrbuch" had only made evident the fact that chemistry is one of the exact sciences it would have done much; but it has done more than this; it is a repository of the general and abstract truths of the science arranged in logical sequence; it is a guide to the student and the investigator (for in chemistry these two are one); and it is full of suggestions alike to the physicist and the chemist.

That part of the second volume which deals with the recent developments of the study of chemical affinity will probably be found by many to be the most interesting portion of the book. Everyone knows how unsatisfactory is the treatment of this subject in the standard text-books. Who has not been perplexed as he attempted to gain clear conceptions about affinity? Affinity is one of those terms that escape one as soon as one tries to grasp it: it is protean, and each form which it assumes scarcely lasts long enough for one to distinguish it from the others.

The work of Guldberg and Waage, published twenty years ago, did not bring forth much fruit for some time; perhaps because these naturalists were obliged to go back sixty years to find in the writings of Berthollet the germs of a really exact treatment of the subject of affinity. But within recent years great advances have been made—and made, speaking broadly, on the lines laid down by the Norwegian professors. No one has had more part in these advances than Ostwald; to him we are indebted for several new experimental methods for finding values for the affinity-constants of acids and bases—indeed the proof of the existence of a measurable affinity-constant for each acid and base is, for the most part, due to him. It is one thing to know that memoirs are to be found in the journals wherein the subject of affinity is gradually advanced stage by stage, but it is quite another thing to have a clear, logically arranged, and condensed account of these memoirs in a text-book. It is one thing to be told that the modern development of affinity is the outcome of the views which Berthollet published, in 1803, in the *Essai de Statique Chimique*; it is quite another thing to have this historical and logical development set before one in detail in a masterly manner.

The subject of affinity is largely involved in the wider conception of chemical equilibrium. Ostwald gives a short account of the attempts which have been made to formulate the laws of chemical equilibrium. He then narrows the meaning of affinity, at least as applied to acids and bases; by doing this it becomes possible to extricate the notion of affinity from the mass of more or less connected facts which had threatened to swamp it, and to give it a quantitative meaning.

The affinity-constants of acids and bases are numbers which tell how much of a definite chemical action those bodies are capable of performing under definite conditions. The formulæ of the same acids and bases exhibit the composition of definite masses of these compounds, which masses are in many respects chemically comparable. The goal of chemistry has always been to trace definite connections between the composition of bodies and their chemical properties; but of all the chemical properties of a body the most important is its affinity-constant, inasmuch as we are apparently justified in saying that this value quantitatively conditions all the chemical reactions in which the body takes part: hence the importance of accurately tracing the connections between the changes of compositions of bodies, as represented by their formulæ, and the variations in the values of the affinity-constants of these bodies, must be very great. The data are as yet insufficient to allow of more than a beginning in this direction: such a beginning is made in the last chapter of Ostwald's book.

To everyone who hopes to make chemistry the business of his life I would say—get Ostwald's "Lehrbuch," read it, study it, become acquainted with it, use it; for by doing this you must become more fitted for doing your work as a chemist.

M. M. PATTISON MUIR.

BRITISH AND IRISH SALMONIDÆ.

British and Irish Salmonidæ. By Francis Day. 12 Plates. (London and Edinburgh: Williams and Norgate, 1887.)

IN this work Mr. Day expounds in greater detail the views he made known in his "British and Irish Fishes," concerning the characters and affinities of the several British forms belonging to the genus Salmo. He also includes in the volume the consideration of many other important problems connected with the natural history of British Salmonoids. On p. 9 he gives a synopsis of the British genera of the family, viz. Salmo, Thymallus, Coregonus, Osmerus, and Argentina, and then proceeds to consider Genus 1, Salmo, while at p. 278, is the heading Genus 2, Thymallus, Cuvier. For the designation of species and varieties English names are generally used, but with each is given a copious list of the Latin Linnean synonyms, and references to the works where they occur. The species considered are as follows: the Salmon, Trout, British Char, American Char or *Salmo fontinalis*, and the Grayling. Thus Coregonus, Osmerus, and Argentina are left outside the scope of the book, notwithstanding its comprehensive title.

Very elaborate descriptions, including enumerations and dimensions, are detailed for each separate form, but concise diagnostic analysis is entirely wanting. In the synopsis of species of Salmo given in the earlier work, "British and Irish Fishes," we find that the only trustworthy specific character differentiating *Salmo salar* from *Salmo trutta* is the presence in the former of eleven rows of scales in an oblique row from the adipose fin to the lateral line, all forms of *Salmo trutta* having fourteen or more of such scales. In the work before us one has to wade through two pages and a half of description of the salmon before reaching a mention of this diagnostic feature.

The views here expressed concerning the forms of sea-trout are somewhat different from those published in the "British and Irish Fishes." In the latter work Mr. Day described *Salmo trutta* and two varieties, *S. albus* and *S. cambricus*. In the present he describes *Salmo albus* (with the same synonymy) as the immature stage or grilse of the northern sea race of trout, *S. cambricus* being the southern sea race. Here again the want of a short diagnosis of the two races is much felt by the reader. From the numerical formulæ of the two races, which are separated by several pages, it is seen that the range of variation in the number of pyloric cæca in the one race is different from that in the other. In the northern form it is 33-61, in the southern 33-52. But it is extremely difficult, by reading and comparing the two lengthy descriptions, to discover what is the exact amount of difference between the two races. However, after the descriptions we reach a discussion of this very point, and we find that most of the differences on which emphasis has been placed by other authorities are not found to be constant when a large number of specimens are examined, that the two races pass by gradual transition one into the other, but that as a rule in the southern there are fewer pyloric cæca than in the northern, and that the Sewin usually loses the teeth on the body of the vomer, at an earlier age than the northern sea trout.

The following are the different forms of non-migratory fresh-water trout which have been distinguished as distinct species, and whose synonymy is given in the present work: Brook trout, Lochleven trout, Crasspuill trout, Estuary trout, Orkney trout, Cornish trout, Great Lake trout, Gillaroo trout, and Swaledale trout. Short descriptions of these are given in footnotes, excepting the brook trout and the Lochleven trout, which are discussed at length in the text. These descriptions, though brief, are not diagnostic, and it requires the most careful reading and comparison to find in what respects the varieties differ from one another. Mr. Day believes that there is no definite line to be drawn between anadromous sea trout and non-migratory fresh-water trout, intermediate forms being common; nor between the different varieties of fresh-water trout. But granting—for we are inclined to agree with Mr. Day's conclusions—that in all these forms we have but one species, it is surely worth while to give a more lucid and more definite account of the differences between them. The arguments which Mr. Day employs to prove that all forms of trout, whether anadromous or confined to fresh water, belong to one species, may be divided into three classes, and his book would have been much easier to read if he had kept them separate. The first class are those which show that the various forms graduate into one another, or that the peculiarities of one are included in the range of variation of another; the second, those which show that removal to a different environment causes the characteristics of one form to be transmuted into those of another; the third, those which show that the several forms breed freely when crossed.

All the species of char which have been distinguished in Britain are considered in this book as belonging to one variable species which is identical with the *Salmo salvelinus*, Linn., and *S. umbla*, Linn.,—that is, with the Continental char. A similar criticism may be passed on Mr. Day's discussion of char to that made of his account of trout.

In the account of the American char, *Salmo fontinalis*, we have again a minute description, with no specific diagnosis. In a footnote to this portion of the work, it is pointed out that in the article "Salmonidæ" of the present edition of the "Encyclopædia Britannica" the erroneous statement of Dr. Günther, that the *Salmo namaycush* of America is a true trout, is repeated, but no reference is given to any work where the correct description of *S. namaycush* as a char can be found.

We have up to this point been criticizing Mr. Day's work chiefly from a speciegraphical point of view; we must now say a few words about the treatment of other branches of the subject. At the beginning of the account of the genus Salmo is a short description of the anatomy of Salmonoid fishes, followed by a discussion of the eggs and their development, the latter especially in connection with pisciculture at Sir J. Maitland's establishment at Howietoun. The description in the text of the mode of packing eggs which has been perfected at Howietoun seems to be erroneous: it is stated that the main principle is to employ thin layers of well-picked and pressed moss in trays with perforated bottoms, the eggs being separated from the moss by muslin mosquito netting, swan's down, calico, or butter cloth; while in a quotation in a footnote the correct account is given—namely, that the ova rest in direct contact with the damp moss, and are covered by another layer of the same, the muslin being only used in order that the layer of moss may be lifted and moved. Reference is made in this part of the book to the subject of hybridization between different species of Salmo, and a review of the history of the subject is given, but the full treatment of the subject occurs in a chapter specially devoted to it. In this chapter details are recorded of definite experiments in hybridization made at Howietoun. This chapter on hybrids is one of the most interesting in the book, and another on monstrosities is also well worth study.

Scattered throughout the pages are examples of that originality in sentence-construction which is familiar to all who know Mr. Day's writings. Thus in the account of artificial fertilization we read: "This is gently stirred with the hand until the eggs harden, or 'frees' as it is termed, being a period from one to three quarters of an hour according to the temperature, taking longest in cold weather." In another place we find: "One modifying circumstance in the feeding of the salmon has been observed to be connected with a muddy state of the river, possibly interfering with respiration, consequent upon the amount of mud which had been swallowed." Another passage which is worth quoting is:—"As regards thirst it would seem either to be unknown to these creatures; or, living as they do in a watery medium, it may be quenched by means of endosmosis through the skin. Were this not the case, it would be difficult to conceive how such a longing could be satisfied while residing in salt water."

But in spite of its defects the book contains a mass of new and accurate information concerning the forms of Salmonidæ of which it treats. In bibliography it is unusually rich, the results of previous writers being freely quoted in footnotes, so that several of the pages contain 90 per cent. of notes and only 10 per cent. of text. Besides the woodcuts in the course of the work, there are twelve plates of illustrations at the end, ten of which represent different forms of Salmonidæ in beautifully coloured lithographic impressions. The excellence of these is very great, and testifies to great care and skill on the part of the draughtsman (*i.e.* the author himself), the colourist, and the lithographer.

THE ECHINOIDEA.

Die Japanischen Seeigel. Von Dr. L. Döderlein. Pp. 59, Pl. I.-XI., Th. I., Fam. Cidaridæ and Saleniidæ. (Stuttgart: E. Schweizerbart'sche Verlagshandlung, E. Koch, 1887.)

DR. DÖDERLEIN has produced the first part of a very philosophical study of the beautiful Echinoidea, which are in their paradise in the Japanese seas. Some collections of considerable importance came to Dr. Döderlein from private sources, and one was the result of the collecting during the expedition of the Italian corvette *Vettor Pisani*. Descriptions of some of the species of Cidaridæ, Temnopleuridæ, Saleniidæ, and of a species of Hemipedina were published by this author in *Wieg. Archiv*, 1885, v. 51, pp. 73–112, and as some of the forms had an ancient facies they attracted attention. Dr. Döderlein seems to have been impressed with the importance of the fauna in reference to the past, and prepared the way for the present publication by studying the pre-Jurassic and Cretaceous species of Cidaridæ especially. The work now publishing in parts will evidently be worthy of a good naturalist who sees no vast biological breaks in the continuity of the Cidaridæ since the appearance of the Zechstein Eocidaris, which he shows to be inseparable from the modern family Cidaridæ. The author describes the new species, reconsiders the Cidaridæ already known, pays especial attention to the growth of the structures which are used in classification, and, after describing some peculiar structures which he discovered in the St. Cassian Cidarids, passes on to subdivide the great genus so as to identify groups of species according to sub-genera in the Secondary and existing times. The descriptions of species are accompanied by fair illustrations, but it would be as well if more of the denuded tests could be shown.

There are four new species of Cidaris, a new Porocidaris, and three species of Goniocidaris; the depths from which the specimens were derived were from 40 to 200 fathoms. *Goniocidaris mikado*, Död., is the most extraordinary of the species, and has an unusually small number of coronal plates, characteristic median groovings, and wonderful spines—outdoing any other, and that is saying a good deal. The spines are essentially according to Japanese art: the larger have umbrella-shaped disks at their top, and some another disk lower down; the disks are circular in their deeply incised or occasionally serrate outline. The commonest species of Cidaris certainly puts one in mind of the Mediterranean *C. histrix* and of the North Atlantic *papillata*, but these Japanese forms are considered to belong to a different sub-genus by Dr. Döderlein. He was impressed with the fact that some striking Cretaceous Cidaridæ have the primary tubercles of some of the coronal plates near the apical system, aborted or wanting, and that a similar condition occurs in the majority of the Japanese species. He would establish a better definition for Stereocidaris, Pomel, and thus link the Cretaceous and Japanese species together. There is something very candid and straightforward in Dr. Döderlein's method of writing, and he does not hesitate to indicate how, in a comparatively short time, he altered his opinion regarding the particular sub-genus under which his own and other species should go. A similar state of things is well illustrated in the instance of A. Agassiz, and his synonymy of the Cidaridæ shows, as in the case of Dr. Döderlein, how a mind desirous of truth has to suffer in the attempt to subdivide a good genus into groups which are not founded upon differences of structures of much physiological importance.

As a matter of fact, the tubercles of *Cidaris* (*Stereocidaris*) *grandis*, and of the species *japonica*, Död., are not much more deficient than in many specimens of the common *Cidaris* (*Dorocidaris*) *papillata* of the North Atlantic and Mediterranean; and the shape and ornamentation of the coronal plates with ill-developed or absent primary tubercles, in the well-known *Cidaris sceptifera* from the upper chalk, do not resemble those of the modern forms. The ornamentation shown on Pl. II., Fig. 4, is more like that of a Tertiary Cidaroid from Sind than of the tall-plated Cretaceous type. But there is a decided resemblance between Dr. Döderlein's *C. sceptriferoides* and the Cretaceous species.

If these unsatisfactory sub-genera were simply used to represent groups of species linked together by some unimportant but readily recognized structural peculiarities, there would be no objection to be made—indeed, the proceeding is very useful; but the groups are allowed to become of generic significance, and thus it will be noticed at the conclusion of Dr. Döderlein's work, that a list of twenty-two groups equal to genera is given; and bad sub-genera, good ones, and good genera are jumbled up together. Good old Cidaris has in fact fallen to pieces.

In considering the species which have not a Japanese habitat, Dr. Döderlein is in opposition to A. Agassiz and De Loriol in reference to the proper sub-genus under which some well-known species are grouped, and it appears to be the case that Dr. Döderlein will have to arrange the species of Cidaris proper on his own lines.

The particular structure to which Dr. Döderlein alludes in noticing the Triassic Cidaridæ, is a horizontal groove on the interradial side of each pair of pores; it seems to be very universal; moreover, there is a more decided overlap and ribbing of the coronal plates in these pre-Jurassic forms than in the Jurassic and subsequent.

The Salenia described by Dr. Döderlein is a very close neighbour of *S. hastigera*, A. Ag.

Few monographs relating to the recent Echinoidea have as much good matter and logical reasoning in them as this one of Dr. Döderlein's, and the second part of the work will be looked for with great interest.

P. MARTIN DUNCAN.

FRITSCH'S PALÆONTOLOGICAL RESEARCHES.

Fauna der Gaskohle und der Kalksteine der Permformation, Böhmens. Von Dr. Ant. Fritsch. Band II., Heft 1, pp. 32, Plates 49–60. (Prague: In Commission bei Fr. Rivac, 1885.)

THE first part of this admirable work was briefly reviewed in NATURE, vol. xxi. p. 31. It was then observed that the book was almost as interesting to the stratigraphical geologist as to the palæontologist, for the Gaskohle and its superincumbent Kalksteine rest upon Silurian rocks, and are usually not covered by other strata in vertical succession. The coals, clays, and ironstones have a Carboniferous facies, and the conformable limestones are believed to be true Permians. The palæontological evidence regarding the age of the beds is somewhat anomalous in the views of purely British fossilists, but it speaks very forcibly and in a most suggestive manner to the students of the Gondwana formations of Hindustan. The presence of *Sigillaria*, *Stigmaria*, *Calamites*, and *Lepidodendra*, in the Gaskohle, in association with Permian species of ferns and a *Walchia*, seems however to place these Bohemian beds on a lower geological horizon than the Gondwana series, which have had their palæobotany studied by the same palæontologist, Feistmantel, who investigated the plant-remains of the Permo-Carboniferous of Eastern Europe.

The rich fauna of Labyrinthodontia of the Gaskohle, which, as was explained, Fritsch prefers to study under the more comprehensive group of Stegocephali, is associated with fish of the genera *Ceratodus*, *Orthacanthus*, *Pleuracanthus*, *Acanthodes*, and *Amblypterus*, and also with many species of *Palæoniscus*, found elsewhere in true Permian beds. Amongst the Invertebrates are Arachnoidea, Julidæ, Estheriæ, and Anthracosiæ.

The part of the work now under consideration is palæontological, and refers to some of the most interesting of the many sculptured-headed, folded-toothed Amphibia which preceded the Reptilia in time. Several classificatory alterations, especially in the grouping of the genera in families, are introduced, and apparently with good reason; and at the commencement it will be noticed that the Microsaurii, Dawson, suffer, and a new family, the Dendrerpetontidæ, is founded. Fritsch considers that the structure of the teeth of such Microsaurs as *Hylonomus* and *Hylerpeton*, prevents their being associated in the same family with *Dendrerpeton*, the species of which have teeth strongly grooved from the base, with simple irregular folds, the top being smooth: the new family has, like the Microsaurs, amphicœlian vertebræ. It is certainly remarkable how widely these forms were distributed geographically during that long period when so much of the present continental areas was land. Fritsch describes two new species, and also a third about the generic position of which there may be some doubt, and which has a wonderful arrangement of cranial bones behind the orbits.

The most interesting parts of the work are now reached and the author comes to the consideration of those extraordinary Stegocephali which have such curious double and multiple developments of the vertebral centra. The first of the families of these groups is the Diplovertebridæ, and the solitary form of it is carefully described. The characters of the family are the doubly segmented vertebral centra, at the caudal end of the column, and a very decided pitting of the surfaces of the bones of the extremities for vascular canals.

Fritsch avails himself of Cope's terminology; and the peculiar condition of the vertebral centra—the anterior of the two segments carrying the spinous processes and the ribs, the posterior not having any relics of arches, and being plain—necessitates the arrangement of the species with those whose vertebræ are "embolomeri." The illustrations of the species on Plates 50 and 52 are admirable, and their comprehension is assisted by the woodcut diagrams placed in the context.

Sparagmites lacertinus, Fr., is placed amongst the Archægosauridæ, and it will be observed (Plate 50, Figs. 15, 16) how the vertebral centra differ from those of the last family. The centra appear to be broken up, and each has two lateral and an inferior component, coming under the division "rachitomi" of Cope. Miall's family Chauliodontia is represented in the Gaskohle by a species, and the preserved remains show the dissimilar teeth with a semi-Labyrinthodont structure; the genus included is a familiar one to English palæontologists, and is *Loxomma*. The last family, described in the book, has genera with highly developed crania and a parietal foramen (which also occurs in all these forms from the Gaskohle), and the vertebræ are even more remarkable than in the other families. In the Melosauridæ the caudal portion has the centra embolomerous, whilst those of the fore-part of the column are rachitomous; the teeth are dissimilar, and simply and irregularly folded. The supra-occipital bones occasionally have strongly developed, backward projecting, curved processes (*Sehnenhöckern*). The genus *Chelydosaurus*, with a well-developed tarsus and a most singular growth of chest and body plates, belongs to the family. *Sphenosaurus*, H. von Meyer, comes in here, and the species *S. Sternbergii*, elsewhere a Muschelkalk form, is found in the red sandstone of the Bohemian Permian! The new genus *Cochleosaurus* has a species which shows the posterior hooks of the supra-occipital bones in perfection.

The book which contains all this interesting matter will be found of great value by students as well as by advanced palæontologists, and the beauty of the illustrations leaves little to be desired. The Geological Society presented Dr. A. Fritsch with the Lyell Medal and Fund, and the gift was mainly owing to the appreciation of his excellent work amongst these Upper Palæozoic, Permo-Carb. fossils. The work is a great addition to the natural history of the early Vertebrata.

P. M. D.

OUR BOOK SHELF.

The Flora of Howth. By H. C. Hart (Dublin: Hodges, Figgis, and Co., 1887.)

MR. HART enthusiastically describes the parish of Howth as one with many attractions. He thinks that as a sea bathing summer retreat "its equal cannot be found in Ireland"; and he points out that it is invested with archæological interest by a great dolmen in the demesne of Lord Howth, by the ruins of an early abbey in the village of Howth, by the earlier church or chantry of St. Fintan's on the Sutton side, with its holy well, and by the ancient castle, called Corr Castle, of the Barons of Howth. A little way from the shore is Ireland's Eye, with the remains of a church of the sixth or seventh century. For the ornithologist, the entomologist, and the marine zoologist, Howth, according to Mr. Hart, provides much material for study. These things, however, he notes only by the way; it is with the flora of Howth that he is especially concerned. For this he claims attention on two grounds: (1) because several of the species found are rare; (2) because it does not often happen that so many forms exist in so small a space. Mr. Hart has taken great pains to make his account of his subject complete and readily intelligible, and the book ought to be of considerable service to local botanists and tourists.

Mineralogy. By Frank Rutley. Third Edition. (London: Thomas Murby, 1887.)

WE are glad to welcome a third edition of this excellent manual, which forms one of Murby's "Science and Art Department" series of text-books. The materials of the little work are arranged with great clearness, and the descriptions of minerals are invariably simple and precise. Nearly the whole of the chapter on crystallography has been re-written, and other alterations have been made to fit the book for the present requirements of students. More than fifty fresh woodcuts have been added.

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.*

"A Conspiracy of Silence."

MAY I ask your correspondents who have been good enough to read my article on "Darwin's Theory of Coral Islands," published in the September number of the *Nineteenth Century*, to begin addressing themselves to the merits of the scientific question there dealt with, and to cease wasting their own time and your space upon scolding me for a few words—perhaps exaggerated—respecting the wide-spread reluctance to question any theory advanced by Charles Darwin? I have already explained in your columns the sense in which I spoke, and, subject to that explanation, I have nothing to retract. I observe in Prof. Tait's notice of Dr. Balfour Stewart, published in your latest issue, a passage which shows that this very eminent man of science speaks in a tone very similar of certain "advanced" geologists who "ignore" views which "tend to dethrone" their own "pet theories." Moreover, since I last addressed you in explanation, I have observed the remarkable passage ("Darwin's Life," vol. ii. p. 186) in which my censor, Prof. Huxley, positively blasphemes against no less a distinguished body of scientific men than the French Institute for their conduct towards evolutionism. He speaks of the "ill-will of powerful members of that body producing for a long time the effect of a conspiracy of silence." This is the very same expression which I used, but without the offensive aggravations added by Prof. Huxley.

Inveraray, December 30, 1887. ARGYLL.

Mr. Seebohm on Physiological Selection.

FROM a footnote to page 23 of Mr. Seebohm's recently published and magnificent monograph on the Charadriidæ I learn that I owe him an apology for having inadvertently misrepresented his views upon a point of considerable importance in the philosophy of evolution. In his British Association paper (which he now re-publishes) he went even further than I had gone in recognizing the "swamping effects of intercrossing" upon incipient varieties, with the consequent importance of isolation in the differentiation of species. I therefore supposed that he likewise agreed with me in holding it improbable that new species arise as a result of many beneficial variations of the same kind arising at the same time and in the same place. I now find, however, that he is a strong advocate of the opposite opinion—apparently going further than Asa Gray, Nägeli, Mivart, the Duke of Argyll, or indeed any other evolutionist, in support of the doctrine of teleological variation in determinate lines. I therefore write to withdraw my previous misrepresentation of his views upon this matter, and to apologize for my inadvertency in making it.

At the same time, I may observe, it does not seem to me quite intelligible how Mr. Seebohm can reconcile his doctrine of teleological variation with his doctrine of the paramount importance of geographical isolation. For it is evident that, in whatever measure geographical isolation is found to be of importance as a condition to the origin of species (*i.e.* by preventing free intercrossing), in that measure is the doctrine of teleological variation invalidated. Indeed, Mr. Seebohm himself puts Mr. Wallace on the horns of a dilemma with regard to a precisely parallel case. In order to meet me where I draw attention to the difficulty which free intercrossing imposes upon the theory of natural selection, Mr. Wallace argued in favour of collective variation, *i.e.* of the doctrine that a considerable percentage of identical and beneficial variations may arise simultaneously in the same community. Now, Mr. Seebohm very pertinently observes (p. 13):—"It seems to me that, by the admission of this fact, Mr. Wallace has dethroned his theory of natural selection from its proud position as the main factor in the origin of species." With this, of course, I fully agree; but does it not equally follow that by *his* admission of this same "fact" Mr. Seebohm is no less effectually dethroning his own theory of the paramount importance of isolation as one of the main factors in the origin of species?

In conclusion, I cannot understand why Mr. Seebohm should have ignored my answer to the criticisms which he now re-publishes. For, as I have pointed out in these columns before, the whole brunt of his criticism (like that of Mr. Wallace) was directed against a theory which never so much as occurred to me. Both my critics took it for granted that I supposed my "physiological complements" to arise only in pairs; and therefore they both had an easy case in showing how improbable it was that the two complements should chance to come together. But even in my original paper there were passages to show that I supposed these physiological variations to occur in large numbers, or "collectively," leading to what botanists now call "prepotency," and thus explaining why hybridization is so rare in Nature. Possibly in that paper I was not sufficiently explicit in guarding against a misconception which it never occurred to me could arise. But certainly in my reply to this misconception, no further doubt as to my meaning could possibly remain. I confess, therefore, to being not a little surprised at this re-appearance of Mr. Seebohm's criticism, without allusion to my full repudiation of it a year ago. I should much like to learn his views upon the theory which I have published, but must protest against this absurd substitution being still attributed to me, after I have disclaimed it with all the emphasis of which the English language is capable. GEORGE J. ROMANES.

An Incorrect Footnote and its Consequences.

IN all the five editions of Baltzer's "Theorie und Anwendung der Determinanten" there stands at the foot of the first page an historical note, in which reference is made to a work entitled, "Demonstratio eliminationis Cramerianæ," by Mollweide (Leipzig, 1811). About a year ago it became necessary to examine this demonstration for the purpose of having it reported upon in an historical work. The University Libraries in Scotland were applied to in succession, but no copy could be heard of. Inquiries made at the more important libraries in Cambridge by friends resident there, or by letter, ended in the same unsatisfactory way. Letters, followed by an actual visit, to several libraries in London, brought no better result; and after every possible biographical scrap about Mollweide had been ferreted out in the British Museum, the suspicion began to form itself that some curious error had crept into Baltzer's footnote. In order to get to the bottom of the matter, the excellent mathematical library of Göttingen University was next applied to, and the library of Giessen University, where Baltzer was Professor; but in both cases in vain. A last effort was then made about a month ago in a letter to the University Library of Leipzig, where the reputed author Mollweide had taught, and where the "Demonstratio" (or *Demon*, as it had for more than one reason come to be called) had been published. Even here, at first, there was failure. But Prof. Virchl, who most kindly interested himself in the matter, was soon successful in his quest. What he found, however, was not a "Demonstratio" by Mollweide; the title was simply as follows: "Ad memoriam Kregelio-Sternbachianam in auditorio philosophorum die xviii. Julii, MDCCCXI. h. ix. celebrandam invitant ordinum Academiæ Lips. Decani seniores cæterique adsessores——'Demonstratio eliminationis Cramerianæ.'" Either, therefore, no author should have been mentioned by Baltzer, or an indication should have been given that Mollweide's name was an interpolation in the title. One or other of these courses would likewise have been less hurtful to Baltzer's reputation for accuracy; for, after all, Mollweide was not the author. In the Leipzig Library Catalogue the work is entered under the name of De Prasse, and Prof. Virchl had no doubt whatever, for perfectly conclusive reasons which he gave, that De Prasse was the author. The work extends to only 15 pages quarto, and is considered by the same authority to be very rare.

The point which we have now reached in the story might seem a not unfitting one to stop at; but the end is not yet. De Prasse's modesty requires explanation, and so likewise does the intrusion of Mollweide's name. Both are partly cleared up by the following facts supplied by Prof. Virchl. (1) The Kregel-Sternbach dissertation (which the "Demonstratio" was) falls to be delivered by the Dean of the Philosophical Faculty for the time being: the author's name was thus not an absolute necessity on the invitation title-page. (2) Mollweide was De Prasse's successor, and came first to Leipzig in 1811, the very year we are concerned with; so that in that year both men may have held office, and consequently if an author's name had to be supplied Baltzer might easily have made a worse guess.

Both guess-work and circumstantial evidence, however, are quite unnecessary. After these facts were received from Leipzig, the library catalogue of University College, London, was turned up at De Prasse's name. No "Demonstratio," it is true, rewarded the searcher: but as a work with the miscellaneous-looking title, "Commentationes Mathematicæ," 4to, Lips. 1804–12, was found entered, the librarian was communicated with. In a day or two an obliging reply came to hand to the effect that the lair had indeed been found, the 15 quarto pages sought (or, at least, as many as are essential) being pp. 89–102 in the second fasciculus. The full title of the whole work is "Commentationes Mathematicæ, auctore Mauricio de Prasse, Math. prof. ord. in univers. liter. Lipsiensis." The first fasciculus contains 54 pages, and is dated 1804; the second contains 66 pages, viz. pp. 55–120, and is dated 1812. Of the eight separate "Commentationes" the "Demonstratio" is the seventh. Doubtless, copies of this collection of mathematical papers are to be found at several of the libraries above referred to. The work at any rate does not appear to be rare: the writer already possesses a copy, for which he paid the not extravagant sum of 2*s.* 8*d.*

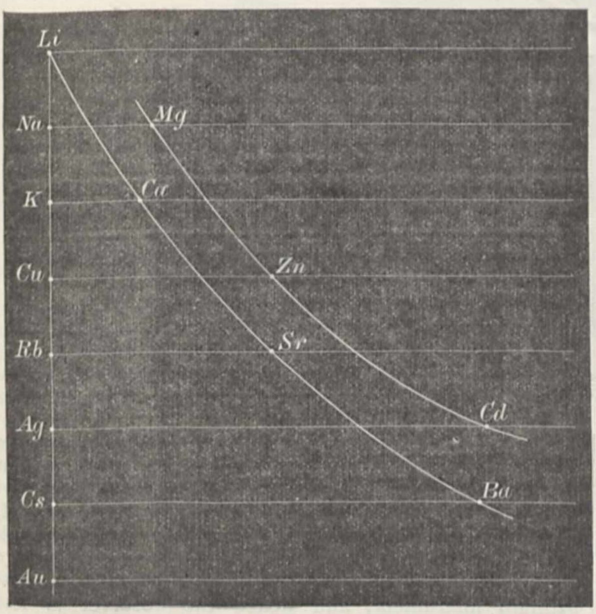
The moral on the surface of this tale may be, "Verify your references"; it is not the only moral, however. Baltzer, in his first preface, felt called upon to direct attention to the many inaccuracies and even errors ("manche ungenauigkeiten und selbst unrichtigkeiten") of Spottiswoode's pioneer treatise; yet if the leaf following the said preface be turned over, a footnote of five lines is found containing five "ungenauigkeiten" (say), one of which—being that referred to in the narrative of the "Demon"—might well be put in a worse category. *Humanum est errare.*

THOMAS MUIR.

Bothwell, Glasgow, December 26, 1887.

The Periodic Law.

In none of the chemistry books or magazines to which I have access can I find any reference to a curious property of the chemical elements in connection with the Periodic Law. If instead of placing the elements as usual in seven vertical columns we arrange them at distances corresponding to the differences of their atomic weights, it will be found that they are disposed in curious curves. The following diagram will make my meaning



clearer. Arranging the monads in a vertical column, and taking it for a base line, place Ca at a distance from K corresponding to the difference of their atomic weights; also treat Sr and Ba in the same way in relation to Rb and Cs. It will then be found that they are arranged on a curve terminating in Li, which is known to unite in itself the properties of the metals of the alkalies and those of the alkaline earths. Mg, Zn, and Cd also range themselves on a curve when measured from Na, Cu, and Ag.

Ranging the tetrads vertically, we have O, S, Cr (Se?), and Mo, in almost a straight line, also P, V (As?), Nb and Sb. Many other curious relationships develop themselves if we plot off the elements vertically as well as horizontally. Is there any explanation of these curious curves? or is it simply accident? and if already known where can I find an account of them?

DONALD MURRAY.

Herald Office, Auckland, N.Z.

[Would not the position of Be (Beryllium) rather affect the apparent parallelism in these curves?—ED.]

The Leaps of *Lepus*.

WHILE rambling in the winter-time over the snow-covered plains in this region, I have recently interested myself in ascertaining how far, on a level surface, a hare or rabbit may leap at each spring, at a time when either of these animals is put to its best speed. Two species of *Lepus* are quite abundant in this vicinity, viz. the Mexican hare (*L. callotis callotis*), and the sage hare, which is really a medium-sized rabbit (*L. sylvaticus Nuttalli*), while the first-mentioned is a big hare. It is not uncommon to find here, in certain localities, a stretch of perfectly level prairie extending for a distance of 3 or 4 miles, and when this is covered by an even layer of 1 inch or more of snow, it offers an admirable surface on which to take account of the distance which may separate any two tracks of one of these animals, either one made by a hare or one made by one of the rabbits. On such a prairie as I have just referred to, I have on numerous occasions fired at these animals when they have been running, and at the same time beyond the range of my fowling-piece; such a shot almost invariably has the effect of so alarming the game as to make it run at its very best rate of speed, and upon coming up with the tracks they have left on the snow at such times, I have been surprised at the distances they can clear at each individual leap. Under these conditions I once measured the spaces cleared by an old Mexican hare, and found the first two equalled 12 feet apiece, while the third effort was rather more than 13 feet, and I have never known this species to exceed this, although I have tested not a few of them. Of course the rabbit cannot compete with such magnificent gymnastics as this: it will, however, when thus frightened, make leaps of fully 6 feet; and on one occasion I measured one on the dead-level prairie, which was rather more than 7 feet. At their common rate of going, the hare rarely clears more than 4 feet at any single leap, while the rabbit is satisfied with rather more than 2 feet, and, when quietly feeding about the sage-brush, the tracks made by an individual of either species may actually overlap each other.

R. W. SHUFELDT.

Fort Wingate, New Mexico, December 6, 1887.

A NEW MAGNETIC SURVEY OF FRANCE.[1]

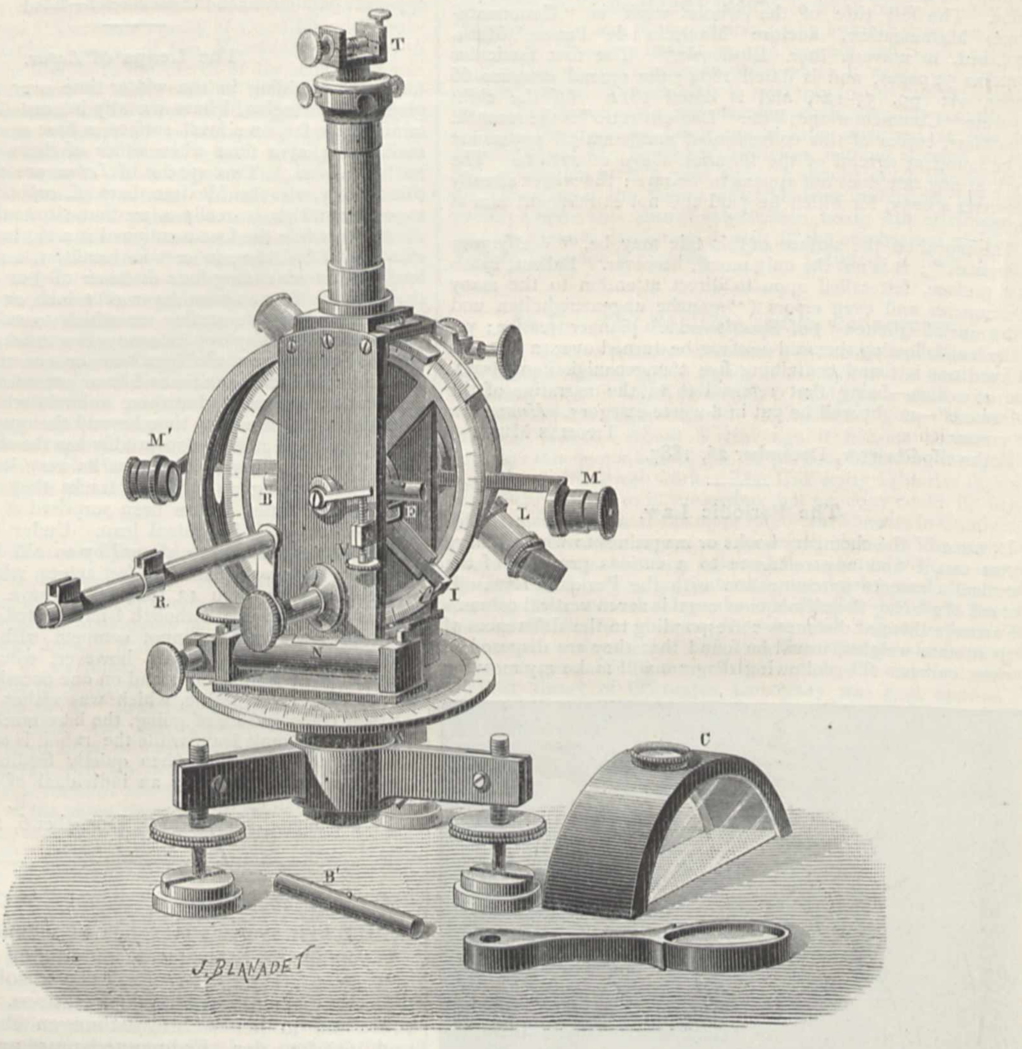
THE first systematic series of magnetic observations made in France was undertaken by Lamont, who in 1856 and 1857 determined the absolute value of the different elements at forty-four stations. The results are contained in his "Untersuchungen über die Richtung und Stärke der Erdmagnetismus an Verschiedenen Puncten des Südwestlichen Europa," and are reduced to three mean epochs: declination to March 1854; horizontal component to June 1848; and dip to the August of the same year. In 1868 and 1869 the Rev. Father Perry made a second series of observations of the intensity and direction of the earth's magnetic force at thirty-three stations in France (Phil. Trans., vols. clx. and clxii.). Determinations of declination have also been made at about twenty stations by MM. Marié-Davy and Descroix in 1875; and declination, dip, and intensity have been observed by M. de Bernardières at various points along

[1] "Détermination des Éléments Magnétiques en France." Ouvrage accompagné de nouvelles Cartes Magnétiques dressées pour le 1er Janvier, 1885. Par M. Th. Moureaux, Météorologiste-Adjoint au Bureau Central, Chargé du Service Magnétique à l'Observatoire du Parc Saint-Maur. (Paris: Gauthier-Villars, 1886.)

the Mediterranean littoral. These observations comprised all that was known respecting the distribution of the magnetic elements and rate of secular change in France prior to the appearance of the important work which forms the subject of this notice.

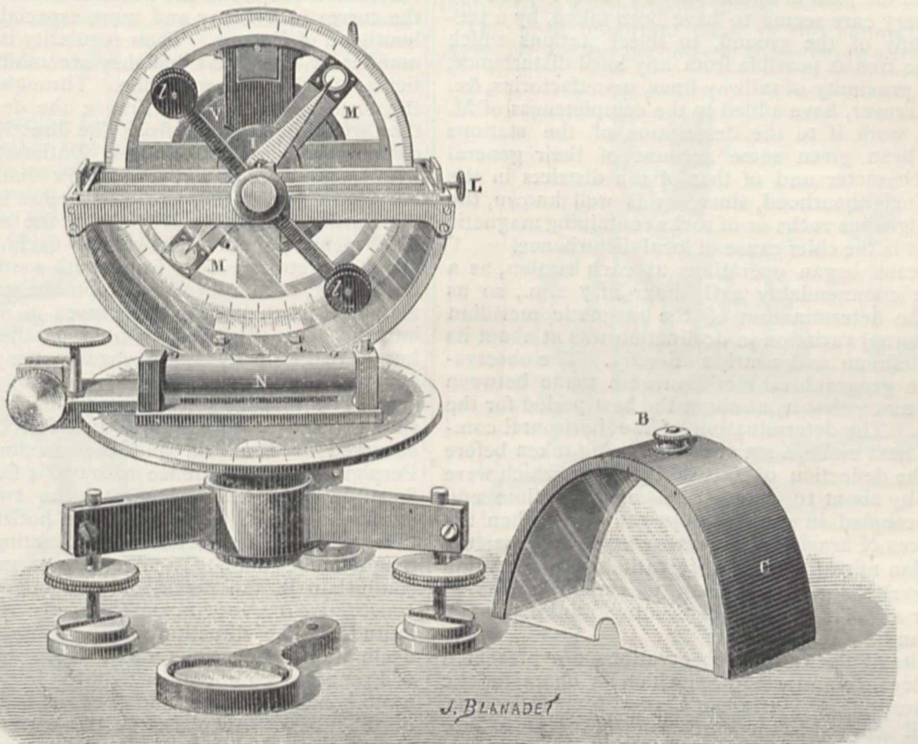
The observations of M. Moureaux were undertaken at the instigation of M. Mascart, the Director of the Meteorological Observatory of the Parc Saint-Maur, and were made during the years 1884 and 1885. A few observations made in 1882 by M. Mascart and M. Moureaux in the neighbourhood of the Pyrenees are also included. A description of the instruments employed, of the methods of observation, together with a detailed account of the results obtained from about eighty stations, fairly well distributed over France, constitute the subject-matter of this memoir.

As the instruments employed by M. Moureaux differ in some important particulars from those which are ordinarily employed for field-work by us, it may be desirable to point out their peculiarities. The instruments which are mainly made use of in this country, and which have been employed by English observers who have made magnetic surveys in other parts of the world during the last quarter of a century, are of what is known as the Kew pattern,



Portable Magnetometer. B, magnet; E, apparatus for steadying magnet; N, level; M M', reading microscopes; L, telescope; T, torsion head; R, bar for deflection experiments.

and embody the results of the experience of such practical magneticians as Lloyd, Sabine, Airy, Welsh, Balfour Stewart, Whipple, and others. Indeed it may be said that almost every observer who has made any extensive series of measurements of terrestrial magnetism has influenced the construction of the Kew magnetometer, and there is no question that this instrument, although not absolutely perfect, has now reached a very high degree of excellence. In some respects, however, the magnetometer employed by M. Moureaux possesses advantages over the Kew pattern, and these are especially evident in surveys over rough and difficult country, and where the means of transport are limited. In the matter of weight alone there is a considerable difference. A Kew magnetometer, in its box complete, and exclusive of the deflection bar, which is now usually carried in a hollow leg of the tripod, weighs nearly 50 pounds, whereas that of the French observers weighs only about 9 pounds. A further advantage possessed by the French model is that it is also an altazimuth instrument, and hence the observer is less dependent upon the knowledge of true time, afforded by his chronometer, in determining the geographical meridian in a declination observation than he is with the English instrument. In the magnetic survey of Scotland made by Welsh in 1857-58 it was necessary to make use of a special altazimuth instrument, or of a sextant and artificial horizon, in order to determine the sun's altitude at the time of observation, and a similar method was employed by the Rev. Father Perry in the course of the magnetic survey of France to which reference has already been made. Thanks to the admirable arrangement of our Post Office by which signals giving Greenwich mean time are sent to all the postal telegraph stations in the United Kingdom, it is possible for an observer engaged in magnetic work in the British Isles to determine the error and rate of his chronometer with an accuracy sufficient to enable him to dispense with the labour and trouble involved in the use of an altazimuth instrument. But unfortunately Greenwich mean time is not yet flashed all over the world, and a surveyor making use of the Kew magnetometer in distant countries would be under the necessity of making independent observations for solar altitude, and hence of adding to his *impedimenta* some such arrangement as those used in former surveys. Nor does this diminution in weight of the French instrument materially influence the accuracy of the observations, at all events so far as declination is concerned. It is hardly possible with the English instrument, even under favourable conditions, to obtain a declination observation which shall be accurate to within 2'. And yet, so far as an analysis of the data given by M. Moureaux enables us to judge, his instrument, of which the circle is only 0·08 m. in diameter, gives results which are in at least as close accordance with the truth. The method of observation which M. Moureaux adopts in determining the magnetic meridian allows him to read the position of both ends of the magnet both when erect and inverted in its stirrup. The magnets are solid and cylindrical in form, 6·5 cm. in length and 0·4 cm. in diameter, and weigh about 7½ grammes, and are suspended by a single thread



Iclinometer. M M, reading microscopes; L, lifting apparatus for needle; I, dipping needle; C, cover; N, level.

of silk 0·11 m. in length. The ends of the magnets are made slightly concave, and are polished so as to reflect the cross-wire placed in each of the microscopes, through which the readings for position are made. Each determination of geographical meridian is the mean result of from four to six independent observations, which rarely differ among themselves by more than 1' of arc.

The same magnet which serves for the observation of declination is used as in the Kew instrument for the determination of the horizontal component, which is done, as with us, by finding the relation $\frac{H}{M}$ by Gauss's method of deflections, and the product HM by the method of vibrations, whence H can be deduced. For this particular determination it seems to us that the Kew model is distinctly to be preferred. Indeed, in the observation of deflections the Kew instrument leaves very little to be desired, provided that care is taken to avoid sudden alterations of temperature, say by exposure to sunshine. The main error in the estimation of the period of vibration of the magnet also arises from the uncertainty of its temperature when observing in the field. But in the French instrument no special pains are taken either to ascertain or to correct for temperature. M. Moureaux indeed is of opinion that, under the conditions of observation, the error committed by neglecting the correction is not greater than that which results from the difficulty of knowing whether the temperature of the magnet is represented by that of the outside thermometer. This is no doubt true of the instrument employed by the French observer, but in the Kew pattern special attention is paid to this point, and, although the arrangement leaves something to be desired, there is no doubt that with care the temperature may be determined with a fairly close approximation to truth. Moreover, the method of determining the time of vibration of the magnet as generally practised by English observers also appears to us to be preferable to that adopted by M. Moureaux, although this has the advantage of occupying little time and therefore of minimizing the effect of any alteration in temperature during these observations and those of the deflections.

As regards inclination, there can, we think, be little doubt that the Kew pattern of dip circle, as made by Dover, is distinctly preferable to that used in the French survey. Indeed in the latter instrument it would seem to be difficult to avoid draughts and dust, the two great enemies to accuracy in field work. Only one needle of 0·065 m. in length was used by M. Moureaux, and the memoir gives no direct evidence of the degree of accuracy of which it was capable. Still M. Moureaux's instrument has the merit of portability, since, when packed in its box, it weighs less than 2 kilos.

As regards the plan of operations, we cannot speak too highly. Every care seems to have been taken, by a preliminary study of the ground, to select stations which should be as free as possible from any local disturbance, such as the proximity of railway-lines, manufactories, &c. It would, however, have added to the completeness of M. Moureaux's work if to the description of the stations there had been given some account of their general geological character and of that of the districts in the immediate neighbourhood, since, as is well known, the presence of igneous rocks or of rocks containing magnetic oxide of iron is the chief cause of local disturbance.

M. Moureaux began operations at each station, as a rule, at the commendably early hour of 7 a.m., so as to secure the determination of the magnetic meridian when the diurnal variation in declination was at about its morning minimum and nearly stationary. The observations for the geographical meridian were made between 8.30 and 9 a.m.; that is, at about the best period for the observation. The determination of the horizontal component was next made, a set of swings being taken before and after the deflection observations, all of which were completed by about 10.30 a.m. Between this time and noon was occupied in the dip observations. When the circumstances of travel or of weather made a departure from this plan necessary, the observations of declination were made either at the time of maximum of diurnal variation or at about the time of evening minimum—say between 5 and 6 p.m.

The results of the various observations are presented with that elegance and clearness which is characteristic of the publications of the Bureau Central Météorologique. They are all referred to the Parc Saint-Maur as a base station, by direct comparisons with the photographic curves of the registering apparatus at work in the magnetic observatory; and are reduced to the mean epoch January 1, 1885, by adding the difference between the values obtained at the different stations and Parc Saint-Maur at the time of observation to the corresponding values at Parc Saint-Maur on January 1, 1885, obtained from the mean of the observations made there in December 1884 and January 1885. This method presupposes that the diurnal variation is of the same order throughout the whole of France, which is not strictly true, but the error resulting from this mode of treatment is probably not greater than the errors of the observations themselves.

The final values are then tabulated and compared with the values obtained for the same places as deduced from the curves given by Lamont, and in this way a measure of the secular change is obtained. The results are finally plotted in the form of maps on Mercator's projection, giving lines of equal declination, force (horizontal component), and dip, and there is, lastly, a map of magnetic meridians. As to the methods employed in the construction of these maps there are unfortunately no details. It would seem that the lines are simply freehand curves, so drawn as to best represent the observational results. There is at least no evidence that the results have been combined, as is the practice among English magneticians, so as to obtain the most probable direction of the lines by calculation, and therefore independently of bias on the part of the map-maker. M. Moureaux moreover offers us no direct means of comparing the values as taken out from his curves with the actual values obtained at the various stations. The maps, however, show certain points of interest which may be thus briefly summarized:—

(1) In the north of France the declination varies about 30′ for each degree of longitude; this proportion decreases in the south. The difference in declination between two points at a given distance apart on the same parallel increases with the latitude, and the isogonal lines are closer together in the north than in the south. The most remarkable feature in the declination map is the form of the curves in Brittany and more especially in the neighbourhood of Rennes. Their regularity is broken in such manner as to suggest that they are modified by the particular trend of the coast-line. Throughout the whole of the north-west portion of France the declination is less than would be expected from the direction and character of the lines over the rest of the Continent. A comparison with Lamont's map for 1854 shows that the declination has diminished during the thirty years by about 3° 58′ in the north, and by about 3° 19′ in the south of France. The mean annual decrease in declination seems to increase pretty regularly from south-south-east to north-north-west, or in a direction approximating to that of the magnetic north; hence the curves of equal declination have not been displaced, by time, parallel to themselves, but have gradually approached to the direction of the geographical meridian.

(2) The map of lines of equal horizontal component shows that the minimum, 0·18460 (C.G.S. units), is observed at Dunkirk, and the maximum, 0·22124, at Perpignan, or a difference of 0·03664 for the interval of 8° of latitude which separates the two points. The maximum rate of decrease of the horizontal component takes place in a direction approximating to that of the magnetic meridian. The decrease is more rapid in the south than in the north, and the interval between two consecutive curves increases pretty regularly with the latitude. The direction of these lines, like those of declination, seems to be modified towards the north-west of France, in such manner that the line corresponding to 0·190 is nearly straight and does not bend to the south as do the others. At places in the extreme north-west of France the value of the horizontal component is therefore greater than the general direction of the other lines would indicate should be the case. A comparison with Lamont's map for 1848 shows that the horizontal component has increased from about 0·008 to 0·010 in absolute value during the thirty-six years. The lines of equal horizontal component have not been displaced parallel to themselves, but are more inclined towards the east, so as to approach the direction of the geographical parallels. The secular change is at its maximum in the west, and diminishes slightly towards the east.

(3) The map of isoclinals shows that these lines have sensibly the same orientation as the lines of equal horizontal component; *i.e.* these are very nearly normal to the direction of the magnetic needle. Whilst the inclination diminishes in general towards the south, the interval between two consecutive curves decreases pretty regularly with the latitude. The direction of the lines corresponding to 66° and 67° seems to be slightly modified as they cross the north-west part of France, as are the lines of equal horizontal component. During the thirty-six years which have intervened since the date of Lamont's map, the dip has decreased by about 1° 35′ in

the north and by about 2° in the south, and, like the lines of horizontal component, the isoclinals have not been displaced parallel to themselves, but in a direction approximating to that of the parallels of latitude. The secular change is least in the north east and gradually increases towards the south, and attains its maximum along the Pyrenees and towards the Gulf of Genoa.

M. Moureaux is to be congratulated on the results of his work, for his countrymen have hitherto scarcely contributed their fair share to our knowledge of terrestrial magnetism. Even the surveys of their own country have been made for them by Germans and Englishmen. Now that Frenchmen themselves have made a beginning, it is to be hoped that the continuity of the work will not be interrupted, for it is only by systematic survey work of the kind so successfully accomplished by M. Moureaux that our knowledge of the magnetic state of the earth and of the laws which regulate its changes can be elucidated.

T. E. THORPE.

TIMBER, AND SOME OF ITS DISEASES.[1]

IV.

BEFORE proceeding further it will be of advantage to describe another tree-killing fungus, which has long been well known to mycologists as one of the commonest of our toadstools growing from rotten stumps, and decaying wood-work such as old water-pipes, bridges, &c. This is *Agaricus melleus* (Fig. 15), a tawny yellow toadstool with

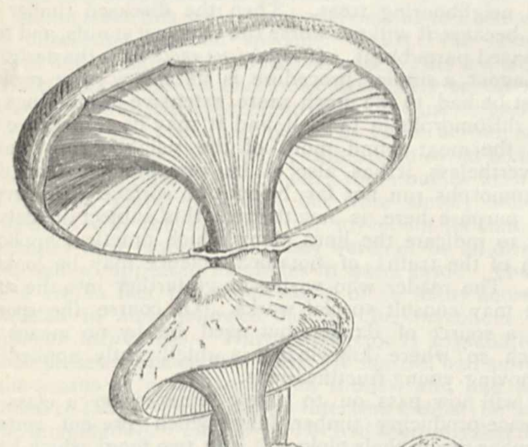


FIG. 15.—A small group of *Agaricus* (*Armillaria*) *melleus*. The toad-stool is tawny-yellow, and produces white spores; the gills are decurrent, and the stem bears a ring. The fine hair-like appendages on the pileus should be bolder.

a ring round its stem, and its gills running down on the stem and bearing white spores, and which springs in tufts from the base of dead and dying trees during September and October. It is very common in this country, and I have often found it on beeches and other trees in Surrey, but it has been regarded as simply springing from the dead rotten wood, &c., at the base of the tree. As a matter of fact, however, this toadstool is traced to a series of dark shining strings, looking almost like the purple-black leaf-stalks of the maidenhair fern, and these strings branch and meander in the wood of the tree, and in the soil, and may attain even great lengths—several feet, for instance. The interest of all this is enhanced when we know that until the last few years these long black cords were supposed to be a peculiar form of fungus, and were known as *Rhizomorpha*. They are, however, the subterranean vegetative parts (mycelium) of the Agaric we are concerned with, and they can be traced without break of continuity from the base of the toadstool into the soil and tree (Fig. 16). I have several times followed these dark mycelial cords into the timber of old beeches and spruce-fir stumps, but they are also to be found in oaks, plums, various Conifers, and probably may occur in most of our timber-trees if opportunity offers.

The most important point in this connection is that *Agaricus melleus* becomes in these cases a true parasite,

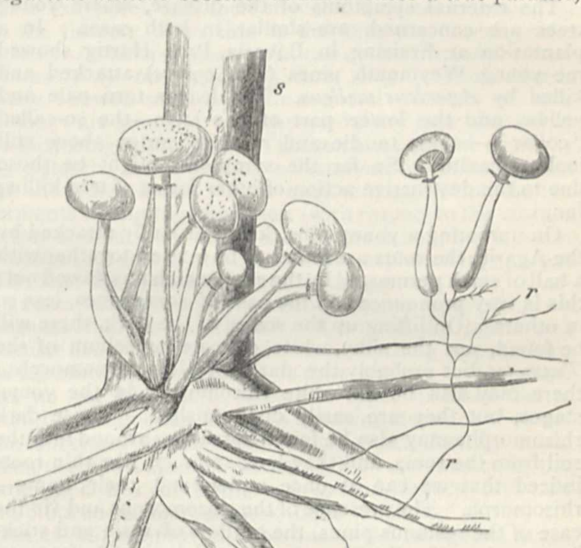


FIG. 16.—Sketch of the base of a young tree (*s*), killed by *Agaricus melleus*, which has attacked the roots, and developed rhizomorphs at *r*, and fructifications. To the right the fructifications have been traced by dissection to the rhizomorph strands which produced them.

producing fatal disease in the attacked timber-trees, and, as Hartig has conclusively proved, spreading from one tree to another by means of the rhizomorphs underground. Only this last summer I had an opportunity of witnessing, on a large scale, the damage that can be done to timber by this fungus. Hundreds of spruce-firs with fine tall stems, growing on the hill sides of a valley in the Bavarian Alps, were shown to me as "victims to a kind of rot." In most cases the trees (which at first sight appeared only slightly unhealthy) gave a hollow sound when struck, and the foresters told me that nearly every tree was rotten at the core. I had found the mycelium of *Agaricus melleus* in the rotting stumps of previously felled trees all up and down the same valley, but it was not satisfactory to simply assume that the "rot" was the same in both cases, though the foresters assured me it was so.

By the kindness of the forest manager I was allowed to fell one of these trees. It was chosen at hazard, after the men had struck a large number, to show me how easily the hollow trees could be detected by the sound.

[1] Continued from p. 229.

The tree was felled by sawing close to the roots: the interior was hollow for several feet up the stem, and two of the main roots were hollow as far as we could poke canes, and no doubt further. The dark-coloured rotting mass around the hollow was wet and spongy, and consisted of disintegrated wood held together by a mesh-work of the rhizomorphs. Further outwards the wood was yellow, with white patches scattered in the yellow matrix, and, again, the rhizomorph-strands were seen running in all directions through the mass.

Not to follow this particular case further—since we are concerned with the general features of the diseases of timber—I may pass to the consideration of the diagnosis of this disease caused by *Agaricus melleus*, as contrasted with that due to *Trametes radiciperda*.

Of course no botanist would confound the fructification of the *Trametes* with that of the *Agaricus*; but the fructifications of such fungi only appear at certain seasons, and that of *Trametes radiciperda* may be underground, and it is important to be able to distinguish such forms in the absence of the fructifications.

The external symptoms of the disease, where young trees are concerned, are similar in both cases. In a plantation at Freising, in Bavaria, Prof. Hartig showed me young Weymouth pines (*P. Strobus*) attacked and killed by *Agaricus melleus*. The leaves turn pale and yellow, and the lower part of the stem—the so-called "collar"—begins to die and rot, the cortex above still looking healthy. So far the symptoms might be those due to the destructive action of other forms of tree-killing fungi.

On uprooting a young pine, killed or badly attacked by the Agaric, the roots are found to be matted together with a ball of earth permeated by the resin which has flowed out: this is very pronounced in the case of some pines, less so in others. On lifting up the scales of the bark, there will be found, not the silky, white, delicate mycelium of the *Trametes*, but probably the dark cord-like rhizomorphs: there may also be flat white rhizomorphs in the young stages, but they are easily distinguished. These dark rhizomorphs may also be found spreading around into the soil from the roots, and they look so much like thin roots indeed that we can at once understand their name—rhizomorph. The presence of the rhizomorphs and (in the case of the resinous pines) the outflow of resin and sticking together of soil and roots are good distinctive features. No less evident are the differences to be found on examining the diseased timber, as exemplified by Prof. Hartig's magnificent specimens. The wood attacked assumes brown and bright yellow colours, and is marked by sharp brown or nearly black lines, bounding areas of one colour and separating them from areas of another colour. In some cases the yellow colour is quite bright—canary yellow, or nearly so. The white areas scattered in this yellow matrix have no black specks in them, and can thus be distinguished from those due to the *Trametes*. In advanced stages the purple-black rhizomorphs will be found in the soft, spongy wood.

The great danger of *Agaricus melleus* is its power of extending itself beneath the soil by means of the spreading rhizomorphs: these are known to reach lengths of several feet, and to pass from root to root, keeping a more or less horizontal course at a depth of 6 or 8 inches or so in the ground. On reaching the root of another tree, the tips of the branched rhizomorph penetrate the living cortex, and grow forward in the plane of the cambium, sending off smaller ramifications into the medullary rays and (in the case of the pines, &c.) into the resin passages. The hyphæ of the ultimate twigs enter the tracheides, vessels, &c., of the wood, and delignify them, with changes of colour and substance as described. Reference must be made to Prof. Hartig's publications for the details which serve to distinguish histologically between timber attacked by *Agaricus melleus* and by *Trametes* or other fungi. Enough has been said to show that diagnosis is possible, and indeed, to an expert, not difficult.

It is at least clear from the above sketch that we can distinguish these two kinds of diseases of timber, and it will be seen on reflection that this depends on knowledge of the structure and functions of the timber and cambium on the one hand, and proper acquaintance with the biology of the fungi on the other. It is the victory of the fungus over the timber in the struggle for existence which brings about the disease; and one who is ignorant of these points will be apt to go astray in any reasoning which concerns the whole question. Anyone knowing the facts and understanding their bearings, on the contrary, possesses the key to a reasonable treatment of the timber; and this is important, because the two diseases referred to can be eradicated from young plantations and the areas of their ravages limited in older forests.

Suppose, for example, a plantation presents the following case. A tree is found to turn sickly and die, with the symptoms described, and trees immediately surrounding it are turning yellow. The first tree is at once cut down, and its roots and timber examined, and the diagnosis shows the presence of *Agaricus melleus* or of *Trametes radiciperda*, as the case may be. Knowing this, the expert also knows more. If the timber is being destroyed by the *Trametes*, he knows that the ravaging agent can travel from tree to tree by means of roots in contact, and he at once cuts a ditch around the diseased area, taking care to include the recently-infected and neighbouring trees. Then the diseased timber is cut, because it will get worse the longer it stands, and the diseased parts burnt. If *Agaricus melleus* is the destroying agent, a similar procedure is necessary; but regard must be had to the much more extensive wanderings of the rhizomorphs in the soil, and it may be imperative to cut the moat round more of the neighbouring trees. Nevertheless, it has also to be remembered that the rhizomorphs run not far below the surface. However, my purpose here is not to treat this subject in detail, but to indicate the lines along which practical application of the truths of botanical science may be looked for. The reader who wishes to go further into the subject may consult special works. Of course the spores are a source of danger, but need be by no means so much so where knowledge is intelligently applied in removing young fructifications.

I will now pass on to a few remarks on a class of disease-producing timber fungi which present certain peculiarities in their biology. The two fungi which have been described are true parasites, attacking the roots of living trees, and causing disease in the timber by travelling up the cambium, &c., into the stem: the fungi I am about to refer to are termed wound-parasites, because they attack the timber of trees at the surfaces of wounds, such as cut branches, torn bark, frost-cracks, &c., and spread from thence into the sound timber. When we are reminded how many sources of danger are here open in the shape of wounds, there is no room for wonder that such fungi as these are so widely spread. Squirrels, rats, cattle, &c., nibble or rub off bark; snow and dew break branches; insects bore into stems; wind, hail, &c., injure young parts of trees; and in fact small wounds are formed in such quantities that if the fructifications of such fungi as those referred to are permitted to ripen indiscriminately, the wonder is not that access to the timber is gained, but rather that a tree of any considerable age escapes at all.

One of the commonest of these is *Polyporus sulphureus*, which does great injury to all kinds of standing timber, especially the oak, poplar, willow, hazel, pear, larch, and others. It is probably well known to all foresters, as its fructification projects horizontally from the diseased trunks as tiers of bracket-shaped bodies of a cheese-like consistency; bright yellow below, where the numerous minute pores are, and orange or somewhat vermilion above, giving the substance a coral-like appearance. I have often seen it in the neighbourhood of Englefield Green and Windsor, and it is very common in England generally.

If the spore of this *Polyporus* lodges on a wound which exposes the cambium and young wood, the filaments grow into the medullary rays and the vessels, and soon spread in all directions in the timber, especially longitudinally, causing the latter to assume a warm brown colour and to undergo decay. In the infested timber are to be observed radial and other crevices filled with the dense felt-like mycelium formed by the common growth of the innumerable branched filaments. In bad cases it is possible to strip sheets of this yellowish white felt-work out of the cracks, and on looking at the timber more closely (of the oak, for instance) the vessels are found to be filled with the fungus filaments, and look like long white streaks in longitudinal sections of the wood—showing as white dots in transverse sections.

It is not necessary to dwell on the details of the histology of the diseased timber: the ultimate filaments of the fungus penetrate the walls of all the cells and vessels, dissolve and destroy the starch in the medullary rays, and convert the lignified walls of the wood elements back again into cellulose. This evidently occurs by some solvent action, and is due to a ferment excreted from the fungus filaments, and the destroyed timber becomes reduced to a brown mass of powder.

I cannot leave this subject without referring to a remarkably interesting museum specimen which Prof. Hartig showed and explained to me this summer. This is a block of wood containing an enormous irregularly spheroidal mass of the white felted mycelium of this fungus, *Polyporus sulphureus*. The mass had been cut clean across, and the section exposed a number of thin brown ovoid bodies embedded in the closely-woven felt: these bodies were of the size and shape of acorns, but were simply hollow shells filled with the same felt-like mycelium as that in which they were embedded. They were cut in all directions, and so appeared as circles in some cases. These bodies are, in fact, the outer shells of so many acorns, embedded in and hollowed out by the mycelium of *Polyporus sulphureus*. Hartig's ingenious explanation of their presence speaks for itself. A squirrel had stored up the acorns in a hollow in the timber, and had not returned to them—what tragedy intervenes must be left to the imagination. The *Polyporus* had then invaded the hollow, and the acorns, and had dissolved and destroyed the cellular and starchy contents of the latter, leaving only the cuticularized and corky shells, looking exactly like fossil eggs in the matrix. I hardly think geology can beat this for a true story.

The three diseases so far described serve very well as types of a number of others known to be due to the invasion of timber and the dissolution of the walls of its cells, fibres, and vessels by Hymenomycetous fungi, *i.e.* by fungi allied to the toadstools and polypores. They all "rot" the timber by destroying its structure and substance, starting from the cambium and medullary rays.

To mention one or two additional forms, *Trametes Pini* is common on pines, but, unlike its truly parasitic ally, *Tr. radiciperda*, which attacks sound roots, it is a wound-parasite, and seems able to gain access to the timber only if the spores germinate on exposed surfaces. The disease it produces is very like that caused by its ally: probably none but an expert could distinguish between them, though the differences are clear when the histology is understood.

Polyporus fulvus is remarkable because its hyphæ destroy the middle-lamella, and thus isolate the tracheides in the timber of firs; *Polyporus borealis* also produces disease in the timber of standing Conifers; *Polyporus igniarius* is one of the commonest parasites on trees such as the oak, &c., and produces in them a disease not unlike that due to the last form mentioned; *Polyporus dryadeus* also destroys oaks, and is again remarkable because its hyphæ destroy the middle-lamella.

With reference to the two fungi last mentioned I cannot avoid describing a specimen in the Museum of Forest Botany in Münich, since it seems to have a possible bearing on a very important question of biology, viz. the action of soluble ferments.

It has already been stated that some of these tree-killing fungi excrete ferments which attack and dissolve starch-grains, and it is well known that starch-grains are stored up in the cells of the medullary rays found in timber. Now, *Polyporus dryadeus* and *P. igniarius* are such fungi; their hyphæ excrete a ferment which completely destroys the starch-grains in the cells of the medullary rays of the oak, a tree very apt to be attacked by these two parasites, though *P. igniarius*, at any rate, attacks many other dicotyledonous trees as well. It occasionally happens that an oak is attacked by both of these Polyporei, and their mycelia become intermingled in the timber: when this is the case the *starch-grains remain intact in those cells which are invaded simultaneously by the hyphæ of both fungi*. Prof. Hartig lately showed me longitudinal radial sections of oak-timber thus attacked, and the medullary rays showed up as glistening white plates. These plates consist of nearly pure starch: the hyphæ have destroyed the cell-walls, but left the starch intact. It is easy to suggest that the two ferments acting together exert (with respect to the starch), a sort of inhibitory action one on the other; but it is also obvious that this is not the ultimate explanation, and one feels that the matter deserves investigation.

It now becomes a question—What other types of timber-diseases shall be described? Of course the limits of a popular article are too narrow for anything approaching an exhaustive treatment of such a subject, and nothing has as yet been said of several other diseases due to crust-like fungi often found on decaying stems, or of others due to certain minute fungi which attack healthy roots. Then there is a class of diseases which commence in the bark or cortex of trees, and extend thence into the cambium and timber: some of these "cankers," as they are often called, are proved to be due to the ravages of fungi, though there is another series of apparently similar "cankers" which are caused by variations in the environment—the atmosphere and weather generally.

It would need a long article to place the reader *au courant* with the chief results of what is known of these diseases, and I must be content here with the bare statement that these "cankers" are in the main due to local injury or destruction of the cambium. If the normal cylindrical sheet of cambium is locally irritated or destroyed, no one can wonder that the thickening layers of wood are not continued normally at the locality in question: the uninjured cells are also influenced, and abnormal cushions of tissue formed which vary in different cases. Now, in "cankers" this is—put shortly—what happens: it may be, and often is, due to the local action of a parasitic fungus; or it may be—and, again, often is—owing to injuries produced by the weather, in the broad sense, and saprophytic organisms may subsequently invade the wounds.

The details as to how the injury thus set up is propagated to other parts—how the "canker" spreads into the bark and wood around—*are* details, and would require considerable space for their description: the chief point here is again the destructive action of mycelia of various fungi, which by means of their powers of pervading the cells and vessels of the wood, and of secreting soluble ferments which break down the structure of the timber, render the latter diseased and unfit for use. The only too well known larch-disease is a case in point; but, since this is a subject which needs a chapter to itself, I may pass on to more general remarks on what we have learnt so far.

It will be noticed that, whereas such fungi as *Trametes radiciperda* and *Agaricus melleus* are true parasites which can attack the living roots of trees, the other fungi referred to can only reach the interior of the timber from the exposed surfaces of wounds. It has been pointed out along what lines the special treatment of the former diseases must be followed, and it only remains to say of the latter: take care of the cortex and cambium of the tree, and the timber will take care of itself. It is unquestionably true that the diseases due to wound-parasites can be avoided if no open wounds are allowed to exist. Many a fine oak and beech perishes before its time, or its timber becomes diseased and a high wind blows the tree down, because the spores of one of these fungi alight on the cut or torn surface of a pruned or broken branch. Of course it is not always possible to carry out the surgical operations, so to speak, which are necessary to protect a tree which has lost a limb, and in other cases no doubt those responsible have to discuss whether it costs more to perform the operations on a large scale than to risk the timber. With these matters I have nothing to do here, but the fact remains that by properly closing over open wounds, and allowing the surrounding cambium to cover them up, as it will naturally do, the term of life of many a valuable tree can be prolonged, and its timber not only prevented from becoming diseased and deteriorating, but actually increased in value.

There is no need probably for me to repeat that, although the present essay deals with certain diseases of timber due to fungi, there are other diseases brought about entirely by inorganic agencies. Some of these were touched upon in the last article, and I have already put before the readers of NATURE some remarks as to how trees and their timber may suffer from the roots being in an unsuitable medium.

In the next paper it is proposed to deal with the so-called "dry-rot" in timber which has been felled and cut up—a disease which has produced much distress at various times and in various countries.

H. MARSHALL WARD.

(*To be continued.*)

PERPETUAL MOTION.[1]

IF we study the past in order to trace the development of machines, we cannot help being astonished at the long centuries during which man was content to employ only his own muscular effort and that of animals, instead of utilizing the other forces of Nature to do his work; for it is a striking fact that it is during little more than the last quarter of a century that the power of the steam-engine has in the aggregate become twice as great as that of the whole working population of the world.

Although the early history of the subject is shrouded in obscurity, there is little doubt that the power of water was the first to be employed. We can easily imagine that, in those early days when the laws of Nature were so little understood, the idea would arise that, if some machine could be contrived which would not get tired like man or animal, as machines appeared to do when left to themselves, and, moreover, one which did not depend upon a capricious and variable supply of water, such a machine would go on for ever—in short, would have perpetual motion. As a matter of fact, Geiger, the German philologist, has adduced strong grounds for believing the Buddhist praying-wheels—on which the prayers of the worshippers were fastened, and which were turned by water power—to be probably the first kind of water motor; and at the same time the first record of a proposal for a perpetual motion machine appears to be in the "Siddhânta Ciromani," a Sanskrit text-book on astronomy, in which a wheel for this purpose is suggested, having a number of closed equidistant holes half filled with mercury upon a zigzag line round its rim. No doubt other suggestions of this kind were made from time to time, but writers and literary men did not condescend to notice them, or even the progress of the really practical and useful machines. We are thus brought from that distant date down to the thirteenth century, when we find in the sketch-book of an architect, Wilars de Honecort (the original being now in the École des Chartres, at Paris), a drawing of a proposed perpetual motion machine, with the statement which, translated, runs:—"Many a time have skilful workmen tried to contrive a wheel that shall turn of itself: here is a way to make such by means of an uneven number of mallets or by quicksilver." The engraving shows four mallets upon what is evidently meant to be the descending side of the wheel, and three upon the ascending side, the former therefore overbalancing the latter. To get the mallets into this desirable position the top one on the descending side has evidently been made to fall over before its time; but independently of this there is to the ordinary mind a strong suggestion of speedy dissolution in any structure a greater number of whose parts are going in one direction than in the other, but this little difficulty M. de Honecort does not allude to or discuss. The unevenly weighted wheel in which the action of gravity is to be cheated in some way or the other has appeared in a great variety of forms since, and, from the words "many a time," probably before, and is by far the most important type of proposed contrivance for perpetual motion.

About two centuries after De Honecort, the famous Leonardi da Vinci gives sketches of six designs, either due to his own fertile brain or taken from other sources, and since then there has been an incessant flow of proposals of this type of machine, a large number of which are given in the work of Dr. Henry Dirks, "Perpetuum Mobile," and several in vol. xii. of the *Mechanical World*.

The next class of proposed machines we may consider are those in which gravity was to be made use of in one direction and evaded in the opposite, by the agency of falling water, amongst these being the devices of Schott, Scheiner, Böckler, and others. The idea in all these was that a quantity of water might be kept circulating between two tanks, one above, and one below; being raised to the upper one by means of pumps driven by a water-wheel which derived its motion from the selfsame water in falling the same distance, there being a balance to the good in the form of extra work to be done by the wheel.

A third class of proposals suggests the application of capillary action to raise the water instead of employing pumps, one of the earliest being that of a Professor of of Philosophy in Glasgow about 200 years ago. In this case and others the drawings show (in anticipation) the water thus raised flowing out at the top in a good substantial stream, as, for instance, in the scheme of Branca about the date of the Professor's production.

The fourth and last class, which partook more of a philosophic nature, proposed to employ magnets, the attraction of which is to be effective in one position, and masked in another. There are many proposed ways of effecting this, all equally futile, although one contrived by a shoemaker of Linlithgow actually deceived for a time Sir David Brewster, who communicated an account of it to the *Annales de Chimie*. In the simplest a ball is to fall through a certain distance, so as to come into a position where it can be raised up an inclined plane by magnetic attraction. The first part is carried out in strict

[1] Abstract of a Lecture delivered by Prof. Hele Shaw, University College, on December 21, 1887, in St. George's Hall, Liverpool.

accordance with the programme, but the ball refuses to go through the second part without coercion.

Now most of these schemes had a very definite object in view, which was to obtain motive power, and not at all the innocent philosophic notion of delighting future ages by the sight of a machine which, like the sacred flame Mark Twain tells of, had been going for so many centuries; in short, it was not to benefit posterity but themselves that perpetual motion seekers worked and patented their inventions; and thus the question naturally arises, Did any of their inventions appear to work? Well, they did; and here we may divide these machines into two classes, those which did not succeed, and those which did. The former are in a strong majority, but the latter are important; and I will briefly give an account of one case, perhaps the most celebrated, of the latter. About the year 1712 a great stir was made on the Continent by the appearance of a wonderful machine contrived by a German Pole, by name Jean Ernst Elie-Bessler, who apparently (not perhaps having enough names) had assumed the additional surname Orffyreus. This Orffyreus had, it was said, contrived upwards of 300 perpetual motion machines, and at last had got one that worked. Kings, princes, landgraves, not to say professors and learned men, were all convinced of the absolute certainty of the action of the machine, and Baron Fischer writes to the celebrated Dr. Desaguliers as seriously as Prof. s'Gravesande did to Sir Isaac Newton about it as follows, concerning a visit paid to this machine in the castle of Wissenstein, in Cassel:—"The wheel turns with astonishing rapidity. Having tied a cord to the axle, to turn an Archimedian screw to raise water, the wheel then made twenty turns a minute. This I noted several times by my watch, and I always found the same regularity. An attempt to stop it suddenly would raise a man from the ground. Having stopped it in this manner it remained stationary (and here is the greatest proof of a perpetual motion). I commenced the movements very gently to see if it would of itself regain its former rapidity, which I doubted; but to my great astonishment I observed that the rapidity of the wheel augmented little by little until it made two turns, and then it regained its former speed. This experiment, showing the rapidity of the wheel augmented from the very slow movement that I gave it to an extraordinary rapid one, convinces me more than if I had only seen the wheel moving a whole year, which would not have persuaded me that it was perpetual motion, because it might have diminished little by little until it ceased altogether; but to gain speed instead of losing it, and to increase that speed to a certain degree in spite of the resistance of the air and the friction of the axles, I do not see how any one can doubt the truth of this action." The inventor himself wrote various pamphlets—with dedications 60 pages in length in German—entitled, "Das Triumphirende Perpetuum Mobile Orffyreanum," and in Latin, "Triumphans Perpetuum Mobile Orffyreanum." This machine worked hard, raising and lowering stones or water as required, being locked in a room; the people outside could see the work done by means of a rope which passed through an opening in the wall, and this ought to have satisfied them. Still, there were disbelievers, and amongst others we find a M. Crousaz writing as follows:—"First, Orffyreus is a fool; second, it is impossible that a fool can have discovered what such a number of clever people have searched for without success; third, I do not believe in impossibilities; . . . fifth, the servant who ran away from his house for fear of being strangled, has in her possession, in writing, the terrible oath that Orffyreus made her swear; sixth, he only had to have asked in order to have had this girl imprisoned, until he had time to finish this machine; . . . eighth, it is true that there is a machine at his house, to which they give the name of perpetual motion, but that is a small one and cannot be removed." These are serious charges even if not in logical sequence, and before we conclude the history of this invention we will examine a machine which has been made at University College, which has certainly surprising properties, although very simple. It is now locked, for we may say of it what was said of a machine about twenty years ago by the *Boston Journal*:—"It will not, nay cannot, stop without a brake, as it is so fixed by means of balls and arms that the descending side of the wheel is perpetually farther from the centre of motion than the opposition ascending." That is just our machine, which, started, behaves exactly as Baron Fischer describes, and raises a weight or does other work. This machine is so constructed as to enable complete examination to be made, and all possibility of unfair play apparently detected, and yet it is a fraud,[1] as was that of Mr. Orffyreus, which was afterwards exposed.

The conclusion we arrive at is, that it would have been well for a great number of folks if the saying due to Lucretius nearly 2000 years ago, "Ex nihilo nihil fit,"[2] had been appreciated and believed in by them. Thus the waste of many lives of fruitless work might have been avoided not only in the past but even in the present day, for it is an astonishing fact that during the last twenty years more than 100 English and French patents for perpetual motion machines have been obtained; in one case a gentleman not very far from Liverpool having spent a very large sum on this profitable subject. The lecturer stated that the other day he had a visit *in propria persona* from an inventor of, and of course believer in, such a machine, and after having for an hour and a half discussed the question with this gentleman as calmly as was possible under the circumstances, he had grounds for feeling that his lecture would be utterly incomplete if he left the subject content with raising a laugh at the whole matter: not so very long ago it was easy enough to do this at the expense of railways and ocean steamers. He would therefore briefly and simply, but he hoped conclusively, state the general nature of the problem of perpetual motion. Firstly, all machines such as we have seen projected for creating power are as impossible as the idea of creating matter. Secondly, many machines have been projected for using sources of energy, such as heat, as proposed by Desaguliers, and many others since, in which known sources of power were to be rendered available. Such machines continue to work only while the supply of energy lasts, therefore have not perpetual motion. Thirdly, since, just as energy cannot be created, so it cannot be destroyed, but can only take another form, the question arises, Cannot the causes retarding a body's motion be removed and the body go on moving for ever? In order to answer this reasonable question, he proposed for a few moments to search for perpetual motion. He then proceeded to illustrate, by means of a variety of machines, what efforts had been made to reduce frictional resistance. In one case, an inventor working on the principle that in a wheel of half the size the friction was reduced in the same proportion proposed to employ two in this ratio; no doubt with the same idea as the man who, seeing a stove advertised to save half the usual quantity of coal, bought two with the idea of saving it all. Many people thought that, theoretically, friction was entirely removed by means of rolling contact—illustrated by roller and ball-bearings—but it was only because the theory was imperfect, and the true nature of rolling not understood; and, by means of lantern illustrations, the action of rolling surfaces was experimentally examined. The irresistible conclusion must be arrived at that friction is as universal in its action

[1] Being driven by concealed cords passing down the hollow legs and actuated by a youth beneath the platform.

[2] Propounded, indeed, in a different form by Democritus 400 years before that.

as gravitation, and to avoid it on the earth is impossible; and with this conclusion vanishes all hope of a perpetual motion machine. If we are inclined to regret this fact, a little reflection on what would occur if friction ceased to act may not be uninstructive, for the whole face of Nature would be at once changed, and much of the dry land, and, even more rapidly, most of our buildings, would disappear beneath the sea. Such inhabitants as remained for a short time alive would not only be unable to provide themselves with fire or warmth, but would find their very clothes falling back to the original fibre from which they were made; and if not destroyed in one of the many possible ways—such as by falling meteors, no longer dissipated by friction through the air, or by falling masses of water, no longer retarded by the atmosphere and descending as rain—would be unable to obtain food, from inability to move themselves by any ordinary method of locomotion, or, what would be equally serious, having once started into motion, from being unable to stop except when they came into collision with other unhappy beings or moving bodies. Before long they, with all heavier substances, would disappear for ever beneath the waters which would now cover the face of a lifeless world.

We turn to the motion of planetary bodies—is that perpetual? At first, everything seems to show that it is. The earth with its mass of 3000 trillion tons turns with a speed which enables a student to go bare-headed a good many miles without catching cold in the act of saluting a Professor, for a long time defied all attempts to detect in it loss of speed; but with the friction of the tides continually at work such loss must take place, and now it is pretty certain from the calculations of Adams, the astronomer, that the earth loses about an hour in 16,000 years, and is coming to rest, though it must be admitted rather leisurely. So, also, the hurrying up of the comets as they go round the sun is possibly accounted for by a retarding action in space which makes it necessary for them to try and make up, as it were, for lost time; and in fact the general arguments in the present day are in favour of what Sir Isaac Newton believed—that the motions of all bodies in space are suffering retardation, and that their velocity is becoming less and will ultimately cease.

Perpetual motion, then, is impossible. By no means. We have duly considered motion of matter in its visible and mechanical form, and if the foregoing remarks are true, then in this form assuredly it is; but there is, as we have seen, the great fact of indestructibility of energy, and the greatest generalization of the present century is that which accounts for the disappearance of energy in the form of mechanical and visible motion by showing that an exactly equal amount appears in the form of molecular and invisible motion. To this all outward motion tends, and friction is the agency by which the change is effected. Down to a certain point the change can be effected in either direction, and the heat-engine converts molecular motion into mechanical, again to be reconverted into molecular motion in all its working parts, as well as in connection with the useful work it does. This stage reached, there is no process known to us by which the cycle can be continued, and the term "degradation," in the sense of having gone down a step, but nevertheless a step which can never be reclaimed, is applied to the tendency of energy to assume molecular form by dissipation over a larger mass of matter, so that its effect is less intense, though equal numerically in amount. To this all Nature tends, and beyond this point we cannot go. Here, at any rate, the motion is perpetual, but it is motion that tends to approach a state unsatisfactory to the instinct of the human mind. Great intellects, such as Rankine and Siemens, have striven to conjecture ways at present unknown to us by which the energy now spreading itself over the vast expanse of space may be gathered again and regenerated, so that we may look forward not to the lowest but to the highest form of motion as that which, passing through all its cycles, shall last for ever.

THE CHAIR OF DARWINISM IN PARIS.

ONE of the most interesting evidences of the differing results of municipal organization in foreign countries, as compared with those resulting from such organization in our own, is the news that the Municipal Council of Paris intends to found (in connection with the Sorbonne, or the Jardin des Plantes, or the Collège de France, we do not know which) a Chair of Philosophical Zoology, with a special view to the propagation of the doctrine of evolution as elaborated by Darwin. It appears that the official naturalists in France—those holding the leading professorships and museum appointments—have not hitherto been very friendly to Darwinian doctrine. The Municipal Council of Paris has recognized the fact that there is an undesirable hostility to Darwin's views amongst the official group, and actually proposes to remedy the evil results of this hostility by establishing a new Chair, destined to give fair play and a full hearing to the new philosophy. It is as though the Corporation of London should propose to build and endow a laboratory of physiological experiment or of bacteriology. The imagination recoils before the task of picturing Mr. Alderman Greenfat expounding to his colleagues the importance to the community of scientific research, and carrying with him a large majority in favour of a scientific enterprise hitherto neglected and even penalized by middle-class authority.

There is very little doubt as to who is the fittest man in France at this moment to hold such a Chair as that which is now to be created. M. Giard, for many years Professor of Zoology at Lille, and only this year called to a similar Chair in Paris, has not only been the first in France to teach from an official position the doctrine of evolution in zoology, but has made many most valuable researches himself, and has created a school amongst whom are the ablest of the younger French zoologists. Every embryologist knows the works not only of Alfred Giard, but those of his pupils Barrois, Halley, Monnet, and others. Alfred Giard had to submit to some painful remonstrances, and to imperil his official career as a Professor of Zoology in France, when he determined to break with the traditions of his eminent master, Henri de Lacaze Duthiers, and to boldly accept Darwinism and the methods of the modern English and German school. It is therefore only right that his name should be the first to be considered in relation to the new Chair in Paris, and we have no hesitation in saying that, should he be appointed, a man will have been secured as the first occupant of a difficult position whose qualifications render it certain that he will not only do credit to himself, but will justify, by his successful teaching, the enlightened, patriotic, and high-minded initiative of the Municipality of Paris.

E. R. L.

NOTES.

ON the 3rd of this month there passed away a Scottish parish minister, who though not himself a scientific man has come in contact with three successive generations of men of science whom the love of travel or of geology has led to the picturesque island of Skye. The Rev. Dr. Donald Mackinnon was the third of his family who have been ministers of the parish of Strath. His grandfather was appointed to the incumbency in 1777, and held it for forty-nine years. His father took the office in 1826, and held it for thirty years, until he himself succeeded to it in 1856. The parish has thus been presided over by the same family for the long period of 110 years. Unfortunately none of the numerous family of the deceased clergyman have entered the Church, so that the interesting ecclesiastical connection of the family with the parish now comes to an end. Dr. Mackinnon was a noble type of the true old Highland gentleman, dignified, courteous, kindly, and always the same, whether conversing with crofter or countess. He was delighted to tell his reminiscences of the old geologists. It was his uncle who put into visible expression by his famous but unspeakable "device of the pots" (as Barbour has it) the universal indignation of Skye at the account of the island and its inhabitants given by the geologist Macculloch, in his book on the Highlands and Western Islands. It was in his father's house that Sedgwick and Murchison were entertained when they passed through the north-west Highlands in 1827, and he had some amusing stories about the impression made on himself and his brothers by the doings of these two great brethren of the hammer. In later years geologists and other students of science, as well as artists and distinguished men of many kinds, have enjoyed the hospitality of his home at Kilbride under the shadow of the great mountain, and in sight of the gleaming Atlantic. Only a few months ago he had an opportunity of renewing his early love for mineralogy and geology, and while riding on his favourite quiet cob, looking after his farm-servants as they harvested between the showers of a Skye September, he would stop now and again to point out geological features that had been familiar and interesting to him from boyhood. He belonged to a type of Scottish clergyman that is slowly disappearing, and carries with him the affectionate regrets of everyone who was privileged to enjoy his friendship.

THE Annual General Meeting of the Royal Meteorological Society will be held at 25 Great George Street, Westminster, on Wednesday, the 18th instant, at 8 p.m., when the Report of the Council will be read, the election of officers and Council for the ensuing year will take place, and Mr. W. Ellis, the President, will deliver his address.

ON Tuesday next (January 17), Mr. George J. Romanes will begin at the Royal Institution a course of ten lectures, being the first part of a course on "Before and After Darwin;" Mr. Hubert Herkomer will on Thursday (January 19), begin a course of three lectures on (1) "The Walker School," (2) "My Visits to America," and (3) "Art Education"; and Lord Rayleigh will on Saturday (January 21) begin a course of seven lectures on "Experimental Optics." The Friday evening meetings will begin on January 20, when Lord Rayleigh will give a discourse on "Diffraction of Sound."

THE following are the arrangements for the Penny Science Lectures at the Royal Victoria Hall for the present month:—January 10, "The Great Sea-Serpent," by Arthur Stradling; January 17, "Caves and Cave-Men," by F. W. Rudler; January 24, "The Oldest Monuments in Brittany and Britain," by Prof. Bonney, F.R.S.; January 31, "Speech made Visible, or Picture-Writing as it was, and as it is now," by Prof. Ramsay.

LECTURES will be delivered in Gresham College, Basinghall Street, E.C., on January 17, 18, 19, and 20, at 6 p.m., by Dr. E. Symes Thompson, on "Sleep, Sleeplessness, and Pain."

IN reference to the review in these columns last week (p. 218) of the second series of collected papers on Indo-China, we observe from the last Annual Report of the Council of the Straits Branch, Royal Asiatic Society, that there is at present no intention of proceeding further with the publication of selected papers on the East Indian Archipelago. The Council, however, expresses a hope that, at some future time, an effort will be made by the Society to translate and publish selected papers which have appeared in the Journals of Societies in Holland and Java, written by learned Dutch Orientalists. The Report adds that the new map of the peninsula, to which we have several times referred, was finished in 1886, but before it could be sent to England the Siamese Government gave further geographical information concerning the northern part of the peninsula, and the map will not be published till this new information is incorporated in it.

IN the November Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, attention was drawn to the subject of fruit-growing in British colonies, and an admirable report on the fruits of Canada was given. The treatment of the subject is continued in the January Bulletin, which contains full reports sent by the Governments of Victoria, South Australia, Western Australia, Tasmania, New Zealand, Cape Colony, and Mauritius. Prominence is given to the quantity of fruit actually available for export in each colony. To this the writers add the months during which the fruit is in season, and the prices usually paid for it locally. It was intended to publish the reports from the Australian colonies, Tasmania, New Zealand, and the Cape of Good Hope in one series, so as to present a general review of the fruit industries of the Southern Hemisphere; and this was to have been followed by reports dealing exclusively with the fruits of tropical colonies. So far, however, reports from New South Wales and Queensland have not been received.

AN interesting paper, by Mr. Daniel Morris, on the use of certain plants as alexipharmics, or snake-bite antidotes, has just been issued. Mr. Morris explains that his enumeration of the plants reputed to possess alexipharmic properties is offered without any expression of opinion as to their value. It is intended chiefly as an attempt to bring together for the first time a summary of information about these plants, in order that inquiry may be made to confirm or refute the popular opinion respecting them. "Opportunities," says Mr. Morris, "to test the action of these plants on a person actually bitten by a well-known poisonous snake are seldom offered to a competent investigator. But as material is being brought together which can be carefully tested by chemical and therapeutical investigations, the most prominent of these plants, such as species of *Aristolochia* and *Mikania*, deserve very careful attention."

A VALUABLE paper, by Prof. Marshall Ward, on the tubercular swellings on the roots of *Vicia Faba*, has just been printed in the Philosophical Transactions of the Royal Society.

MESSRS. SWAN SONNENSCHEIN AND CO. will publish in a few days a new work by Mr. Theodore Wood, entitled "The Farmer's Friends and Foes." The book describes in considerable detail the nature and habits of those animals, birds, and insects which exercise a good or evil influence upon the products of British agriculture, and is profusely illustrated.

WE have received the "Annuaire," for 1888, of L'Académie Royale des Sciences, des Lettres, et des Beaux-Arts de Belgique. It contains full information as to the organization, rules, and work of the Academy; and there are several rather elaborate memoirs of late members. Each of these memoirs is accompanied by a carefully-engraved portrait.

CAPT. M. RYKATSCHEW, the Assistant Director of the Central Physical Observatory at St. Petersburg, has published in the *Repertorium für Meteorologie* (Bd. xi. No. 2) a discussion of the winds and pressure of the Caspian Sea. The title is misleading, as the observations are made on land, ten stations being on the shores of the Caspian Sea, and nine in neighbouring districts. And the mean winds are deduced by Lambert's formula, which deals with the number of observations without reference to the force of the wind. Nevertheless the work is a valuable and elaborate discussion, based on trustworthy observations extending from three to forty-four years.

The mean wind-frequency and pressure are given for every month, for seasons, and for the year, and charts are drawn for the seasons and for the year. The work is a continuation of previous similar discussions of the winds of the Baltic, the White and Black Seas, and the Sea of Azov.

IN the *Archives des Sciences Physiques et Naturelles* for December 15 last, M. P. Plantamour publishes the results of observations of the periodic movements of the ground from October 1, 1886, to September 30, 1887, as shown by spirit-levels fixed in the exterior and central partition walls of his house at Sécheron, near Geneva. The oscillations are illustrated by curves, from which it is seen that the movements exhibited by the two levels are not always parallel, but vary in a regular manner, and that both curves follow the variations of temperature throughout the year. Experiments have been carried on for nine years, but a longer series is necessary to arrive at definite conclusions.

M. A. F. SUNDELL publishes in vol. xvi. of the Proceedings of the Scientific Society of Finland, the results of comparisons of the standard barometers at the principal Observatories of Europe, with the view of showing what corrections are to be applied to reduce the readings of different countries to absolute uniformity. The comparison shows the existence of considerable differences between the various standards. But as the experiments were made with an instrument which was filled with mercury at each comparison and afterwards emptied, it is a question whether the results obtained may be considered perfectly trustworthy.

A NEW compound of arsenic, containing that somewhat remarkable substance, hexiodide of sulphur, has recently been prepared by Dr. Schneider, of Berlin (*Journ. für prakt. Chemie*, No. 22). Hexiodide of sulphur, SI_6, was prepared some time ago by Landolt by evaporation of a solution of iodine and sulphur in carbon bisulphide at a low temperature: it forms pyramidal crystals, shown by Von Rath to belong to the rhombic system, and, curiously, is isomorphous with iodine itself. It is a compound of considerable theoretical interest, inasmuch as it is the only known instance in which the supposed six combining bonds or affinities of sulphur are satisfied by monad atoms. One would naturally imagine that such a compound would be eminently saturated, and it has never hitherto been known to effect any further combinations; but Dr. Schneider now shows that it is capable of forming a crystalline double compound with arsenious iodide, of the composition $2AsI_3 . SI_6$. This new compound was first incidentally obtained during a lengthy research upon the relations between arsenious sulphide and iodine, and its discovery forms another example of the happy manner in which important results are often most unexpectedly attained by following the by-paths which so frequently lead off from the high-way of systematic research. It may, however, be synthetically prepared by gently warming a mixture of SI_6 and AsI_3 in the proportion of one molecule of the former to two of the latter: the two substances melt together to a deep-brown liquid, which, on cooling, solidifies to a dark gray crystalline mass. The crystals are homogeneous, tolerably hard and brittle, yielding a reddish-brown powder on pulverization; they cannot be preserved in the air, as they lose all their iodine in twenty-four hours, but in sealed tubes may be kept any length of time. The double compound itself, however, is nothing near so interesting as the important theoretical questions which it suggests. We may well ask, Is it possible that the atoms of sulphur are still endowed with a certain amount of combining energy after their six "affinities" are satisfied? or do the iodine atoms act in this case in one of their higher capacities?

AT the last meeting of the Geographical Society of St. Petersburg, M. Kuesenoff gave an account of an interesting nomad tribe in the Ural Mountains, calling themselves Vagueles. In the winter they dwell in wooden huts, and in the summer wander among the mountains, living in tents. At the former season their clothing consists of deerskin, and at the latter of linen garments. They worship the sun and some of the stars, and have a superstitious dread of certain forests, which they deem sacred. Women hold a very inferior position, being treated as slaves. During the last few years contact with more civilized tribes has had a good influence on the Vagueles, and some of them have begun to settle down as tillers of the soil. The tribe is said to be of Finnish origin.

ON December 10, about 6 p.m., a meteor was seen at Hönefos, in Norway. It went in a north-easterly direction, emitting a brilliant bluish-white light, and lasted a few seconds.

ON December 18, about 8 p.m., a magnificent meteor was seen in several parts of the province of Stockholm, going in a direction north-west to south-east. It shone with a bluish light. It left a broad trail in the sky, and eventually burst into tiny fragments, but without any report.

WHAT is believed to be a meteorite has just been dug out of the ferry harbour of Nökjobing, in Denmark. The stone, which weighs about half a ton, was found in soft mud, and no other stones were near it. It is very dark in colour, contains iron, and is of unusual weight for its size, the work of moving it being very laborious. It has now been blasted to pieces, which will be examined scientifically.

LAST year a Phanerogam hitherto never met with in Scandinavia (*Juncus tenuis*, Willd.) was found near Vexio, in Central Sweden. In Europe this plant is found only in a few localities in Germany, Holland, and Scotland.

DURING last autumn, in October and November, ornithologists in the province of Tromsö, in the extreme north of Norway, were interested by the sudden appearance of large flocks of the so-called "Nut-crow" (*Nucifraga caryocatactes*), a bird hitherto never seen in Northern Norway, and which is scarce even in the southern part of the country. Several specimens were shot and forwarded to the Tromsö Museum. It is surmised that the birds were driven thus far north, during migration, by stormy weather.

OWING to unfavourable weather, the cultivation of oysters on the coast of Norway was not so successful last year as in previous years.

LAST autumn an attempt was made to bring live cod from Iceland to Norway on board smacks, and 6000 fish were brought over to Bergen successfully. Here, however, many of them died, on account of the basin in which they were kept until the sale could be effected being too small. This year fresh attempts will be made.

THE temptation of French architects seems to be to attend to the decorative rather than the useful parts of the buildings they design. The architect who designed the new Medical School in Paris took so little pains about the distribution of the water-pipes, that in very cold weather the laboratories (chemistry, physiology, bacteriology, experimental pathology, &c.) are wholly deprived of water. Last week the water in all the pipes was frozen, so that not a drop of water was available in a single laboratory. Of course, everyone connected with the school complains that work under such conditions is nearly impossible.

THE new Sorbonne will be a handsome building, but, unfortunately, the work is soon to be stopped owing to lack of money. The ornamental part of the building is finished, but the useful part has not yet been begun.

THE *Ceylon Observer*, writing on the great trigonometrical survey of that island, states that its connection with the coatinent of India by a network of triangles is now an accomplished fact, Mr. More, District Surveyor, having in November last finished his series of observations with the large theodolite. Nothing now remains but to reduce the observations, a work which it is anticipated will take about six months. Mr. More had enormous difficulties to overcome in his survey. The north of the island is so much covered with forests that he was compelled to erect lofty stages for his theodolite, at a height of from 40 to 70 feet above the ground; and the observed signals were in many cases 140 feet from the earth. All these stages had to be made on the spot, the appliances at hand being of the poorest description, and it was with the greatest difficulty that the structures thus made were kept at the necessary rigidity. The climate is so uncertain that the surveyors often watched for days without seeing a flash from the heliostat, and at other times every member of the working parties was prostrated by fever. As the observers approached the coast, stone towers were put up instead of timber stages, and these towers will serve not only as permanent survey stations, but as landmarks for those navigating the neighbouring waters. In all, eleven stone towers were erected, and very many wooden stages. Ceylon, by the completion of this trigonometrical survey, is now free from the reproach which it has lain under since the Indian surveyors finished their portion of the work. There is now a complete chain of triangles from Asiatic Russia to the south of Ceylon. The *Observer* adds that it is curious to note that exactly one hundred years ago (1787) a complete triangular connection was formed between Great Britain and France across the Channel under the superintendence of General Roy, R.E.

THE additions to the Zoological Society's Gardens during the past week include a Burrowing Owl (*Speotybo cunicularia*) from South America, presented by the Rev. Basil Wilberforce; a Vulpine Phalanger (*Phalangista vulpina* ♀) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

O'GYALLA SPECTROSCOPIC CATALOGUE.—The systematic survey with the spectroscope, undertaken for the northern heavens several years ago, by Prof. Vogel and Dr. Dunér, the former examining the region from Decl. 1° S. to Decl. 40° N., and the latter that from Decl. 40° N. up to the Pole, has been now carried some considerable distance into the southern hemisphere by Dr. N. de Konkoly and his assistant, Dr. Kövesligethy; and the second part of the eighth volume of the O'Gyalla observations, which has recently appeared, contains a spectroscopic catalogue of the stars down to mag. 7·5, lying between Decl. 15° S. and the equator. The work was commenced in August 1883, and was completed in August 1886, 2797 spectra having been observed on ninety nights. A number of these were observed on more than one night, so that the resulting catalogue contains only 2022 stars. Vogel's arrangement of types was followed, so that the present catalogue is on the same lines as those of Vogel and Dunér. The annexed table gives the number of stars ranged under each type.

I.*a.*	I.*b.*	I.*b*?	I.*c*?	II *a.*	II.*b.*	III.*a.*	III.*b.*
990	4	12	1	865	2	87	3

Continuous.	Monochromatic.	?
41	3	14

The three monochromatic spectra indicate the presence of minute planetary nebulæ. There was only one star spectrum suspected of showing a bright line, a star of mag. 6·5 about 50' *n* of ζ Orionis. This latter star, together with β, δ, and ε of the same constellation, Dr. Konkoly finds to be variable as to its spectrum. It is to be hoped that the details of the observations upon which so important a statement is based will be published. And it is also to be desired that the work which has been carried so far may now be taken up by some southern observer, and the remaining portion of the heavens surveyed. It is to such works as the present, and the similar labours of Vogel and Dunér, that we must look for evidence of such physical changes amongst the stars as Dr. Konkoly would seem to predicate of the principal stars of Orion.

ASTRONOMICAL PRIZES OF THE PARIS ACADEMY OF SCIENCES.—The Lalande Prize of the Academy has been decreed to M. Dunér for his micrometric measures of double stars, and for his researches on spectra of the third type. M. Périgaud, of the Observatory of Paris, receives the Valz Prize for his important astronomical labours. Amongst those specially mentioned are his determinations of the division errors of four of the circles, and of the absolute flexure of the two principal meridian instruments of the Paris Observatory. The Janssen Prize for important progress in physical astronomy —in the recent sense of the term—awarded this year for the first time, was most appropriately assigned to the late Prof. Kirchhoff. Amongst the general prizes of the Academy should be noted the Arago Medal decreed to M. Bischoffsheim for his great and generous aid to science, and especially for his magnificent foundation of the Nice Observatory. This prize also is now given for the first time. The La Caze Physical Prize is given to MM. Paul and Prosper Henry, chiefly for their great achievements in astronomical photography.

The subject for the Damoiseau Prize for 1888 is proposed in the following question: To perfect the theory of inequalities of long period caused by the planets in the movement of the moon; to see if they exist sensibly beyond those already known.

NEW OBSERVATORY IN VIENNA.—The observatory of Herr M. von Kuffner, the erection of which was commenced in the summer of 1884, has been practically completed. The building is cruciform in shape, and is 82 feet from east to west, and 61 from north to south. The meridian instrument is by Repsold, and has an aperture of 4·9 inches, and a focal length of 5 feet; the eye-piece and object-glass are interchangeable; the circle is 21·6 inches in diameter, and is divided to 2' and read by four microscopes. The principal equatorial is by the same maker, and has an aperture of 10·6 inches, and focal length of 12 feet 6 inches, with a finder of 2·6 inches aperture, and 26 inches focal length. The co-ordinates of the observatory are provisionally given as long. = 1h. 5m. 11·1s. east of Greenwich, and lat. = 48° 12' 47"·2 N.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JANUARY* 15–21.

(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 January 15

Sun rises, 8h. 2m.; souths, 12h. 9m. 34·3s.; sets, 16h. 17m.: right asc. on meridian, 19h. 47·1m.; decl. 21° 10' S. Sidereal Time at Sunset, 23h. 55m.

Moon (at First Quarter on January 21, 5h.) rises, 9h. 18m.; souths, 14h. 1m.; sets, 18h. 51m.: right asc. on meridian, 21h. 39·3m.; decl. 15° 14' S.

Planet.	Rises. h. m.		Souths. h. m.		Sets. h. m.		Right asc. and declination on meridian. h. m.		° '
Mercury..	8 9	...	12 2	...	15 55	...	19 39·4	...	23 26 S.
Venus ...	4 52	...	9 7	...	13 22	...	16 44·5	...	20 0 S.
Mars ...	0 0	...	5 36	...	11 12	...	13 12·8	...	5 17 S.
Jupiter ...	3 56	...	8 16	...	12 36	...	15 52·5	...	19 19 S.
Saturn ...	16 58*	...	0 49	...	8 40	...	8 24·9	...	19 47 N.
Uranus ...	23 56*	...	5 28	...	11 0	...	13 4·5	...	6 9 S.
Neptune..	12 23	...	20 3	...	3 43*	...	3 42·1	...	17 55 N.

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

Jan		h.		
18	...	20	...	Mercury in superior conjunction with the Sun.

Meteor-Showers.

	R.A.		Decl.		
	°		°		
Near π_1 Orionis... ...	72	...	5 N.	...	January 15–20.
From Canes Venatici.	180	...	35 N.	...	Swift; streaks.
Near θ Aurigæ	295	...	53 N.	...	January 14–17.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
T Cassiopeiæ ...	0 17·2 ...	55 10 N. ...	Jan. 19,		*M*
U Cephei	0 52·4 ...	81 16 N. ...	,, 15,	22 1	*m*
			,, 20,	21 41	*m*
Algol	3 0·9 ...	40 31 N. ...	,, 18,	2 59	*m*
			,, 20,	23 48	*m*
V Tauri	4 45·6 ...	17 21 N. ...	,, 15,		*M*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,, 19,	7 0	*m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 19,	21 45	*m*
			,, 21,	1 1	*m*
T Canis Minoris...	7 27·8 ...	11 59 N. ...	,, 19,		*M*
W Virginis	13 20·3 ...	2 48 S. ...	,, 17,	3 0	*M*
δ Libræ	14 55·0 ...	8 4 S. .	,, 18,	4 8	*m*
W Scorpii	16 5·2 ...	19 51 S. ...	,, 17,		*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 15,	17 47	*m*
		and at intervals of		20 8	
β Lyræ...	18 46·0 ...	33 14 N. ...	Jan. 16,	6 0	*M*
R Lyræ	18 51·9 ...	43 48 N. ...	,, 16,		*M*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,, 19,	21 0	*M*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 16,	20 54	*m*
			,, 19,	20 47	*m*
δ Cephei	22 25·1 ...	57 51 N. ...	,, 17,	6 0	*m*
			,, 18,	21 0	*M*

M signifies maximum ; *m* minimum.

DUNÉR ON STARS WITH SPECTRA OF CLASS III.[1]

II.

A SERIES of observations such as ours ought to add at least a little to our knowledge of the development by which the spectra of stars pass from the second class to one of the two sections of the third, especially if these observations are combined with those made of the stars of the two first classes generally, and of our sun in particular; we might even draw conclusions as to the successive development of stars after they have already reached this class. He who sees trees in a forest in different stages of development, some old, some young, some decaying, can at once form an idea of the different stages undergone by each : it is just the same with the observer of the different classes of stellar spectra.

The spectra of the first class are characterized by the almost total absence of all metallic lines excepting those of hydrogen. In spite of that, we cannot doubt for a moment the presence of metallic gases in their atmospheres, for even in the spectrum of Vega we can faintly distinguish the principal rays of sodium, magnesium, and iron. But these gases are probably at such a high temperature that their power of absorption is very slight. But as the star cools and the spectrum approaches the second class, the metallic lines become stronger and more numerous, whilst, strange to say, the lines of hydrogen diminish. Thus the spectrum becomes more and more like that of our sun in its actual state, and at length, as the metallic lines increase, it resembles that of Arcturus.

Up to this stage of development it is unnecessary to consider the two divisions of the third class separately, but after this it becomes indispensable.

In those spectra which at length become III.*a*, the change seems to operate as follows. On account, probably, of the progressive cooling, the metallic lines, especially those of iron, magnesium, calcium, and sodium, become larger, and, besides these, numerous weak narrow lines are seen grouped together, generally in the neighbourhood of the stronger lines. At this stage it is often difficult, if not impossible, to decide, with spectroscopes of small dispersion, whether one sees broad lines or real bands (or flutings). This happens in the spectrum of Aldebaran. The faint lines go on accumulating, until they cannot be separated from one another and occupy broader spaces, and now the spectrum is easily seen to belong to Class III.*a*. At first the bands in the red and orange are the only ones distinctly visible; but later the bands in the green-blue and in the blue become very strong and broad.

While the development of the stars III.*a* was very well known before my researches, former observers have known no star with a spectrum intermediate between II.*a* and III.*b*. Thus, M. Pechülé declares the hypothesis of the co-ordination of the III.*a* and III.*b* classes to be inadmissible. On the other hand, he seems disposed to think that the spectra III.*b* represent a phase, perhaps the last before its total extinction, in the development of each star, and that the passage from type III.*a* to III.*b* takes place suddenly or by a catastrophe, during which the bright lines appear ("Expedition Danoise," pp. 22-25). M. Pechülé seems, however, to consider this hypothesis doubtful, and at length declares that the physical *rôle* of the stars III.*b* is still quite a mystery.

A very simple explanation clears up at least part of this mystery. If the hypothesis which I, in full agreement with M. Vogel, have suggested be correct, the stars intermediate between the second and third classes must necessarily be comparatively rare, considering that this is only a transitory phase of their existence. The general spectroscopic observations of M. Vogel affirm this fact, for amongst the numerous stars examined by him there are only forty-eight whose spectra are denoted by II.*a* ! ! ! II.*a* ! ! or II.*a* ! But as the lines must be very distinctly visible in the spectra of the stars which are on the point of passing from the second class to Class III.*a*, we are obliged to acknowledge that almost all the stars of this category within the zone examined by M. Vogel are among these forty-eight objects. At first sight one might be disposed to seek these stars among those whose spectra are designated by M. Vogel by II.*a* (III.*a*), II.*a*? III.*a*, and III.*a* (II.*a*); but a closer examination shows that although it is not impossible that these spectra may be among these objects, they must be so rare that that is of no essential consequence as regards the question which occupies us.

Amongst these stars there are none which attain the magnitude 4·5, and only fourteen which surpass the magnitude 6·4. All the others are faint objects, and the ambiguous symbols show the difficulty M. Vogel found in recognizing with certainty the details in the spectra, and not that he could not decide with certainty to which of the two contiguous classes a spectrum of which he could easily perceive the details belongs. The correctness of this supposition is, however, proved by the circumstance that certain spectra are designated by III.*a*(III.*b*), or III.*a*? III.*b*. And none will believe that M. Vogel meant to imply that these spectra were in the act of passing from one section of the third class to the other. Besides, one of these stars is R Serpentis, whose spectrum when the star is at the maximum is one of the most strongly marked of III.*a*, according to M. Vogel's earlier researches, and according to mine. But in his general spectroscopic review M. Vogel examined it when its magnitude was only 9·0, and therefore it was easy to doubt, on account of the excessive width of the bands, whether the spectrum might not be III.*b* instead of III.*a*.

Consequently, although I think I am right in admitting that most of these stars belong to the pure type II.*a* or III.*a*, I will nevertheless suppose that a third of them really have spectra intermediate between II.*a* and III.*a*. Their number in M. Vogel's catalogue is 120, and the third is 40, so we should have therefore between the Pole and −25° declination 160 spectra intermediate between II.*a* and III.*a*. I found also by special observations that among the spectra designated by II.*a* ! ! ! II.*a* ! ! and II.*a* ! a fourth part really belong to the intermediate type. Thus there would be in all 200 such spectra, a number evidently much too great. Then, the spectra III.*b* being about fifty times rarer, we should have at most four spectra intermediate between II.*a* and III.*b*, and if only stars of a higher magnitude than 6·0 are reckoned, there would scarcely be one.

But, if we consider the differences between the spectra III.*a* and III.*b*, we shall find that in reality we can scarcely expect to find any spectrum intermediate between II.*a* and III.*b*. As we have seen above, the spectra III.*a* are formed by the exaggeration of the essential characteristics of the spectra II.*a*. There must then be a phase, especially if the star is not very bright, in which one cannot decide to which of the two classes the spectrum belongs. Thus in the spectra III.*b* there are undoubtedly well-marked Fraunhöfer lines—for instance, D, and the narrow band 8, which is probably nothing but the collection of strong lines in the neighbourhood of E, and the very narrow band 5 (λ = 576μ) which is almost like a broad line ; but all these details are only secondary. The essential characteristics are the three nebulous, very broad flutings, which owe their origin to some carbon compound. If these bands are visible, the spectrum is called III.*b*; if they are not, it is called II.*a*. The only forms intermediate between the spectra of the type of Aldebaran and the normal

[1] Continued from p. 236.

type III.*b* are those in which the bands are more or less faint, or even scarcely perceptible. In fact, I have proved not only that there are spectra in which the principal bands, and especially band 6, are weak on account of the brightness of the stars, but I have found a spectrum which is scarcely a spectrum III.*b* yet, but in which the characteristics of this class are undoubtedly present.

This star is DM. + 38° 3957 = 541 Birm. In its spectrum (Planché, Fig. 6) I have seen a rather broad and well-marked band, whose approximate wave-length is 519μ, and the spectrum terminates abruptly at 475μ. These wave-lengths are, within the limits of probable errors, the same as those of the less refrangible ends of bands 9 and 10 in the spectra III.*b*. Once I thought I perceived a very faint trace of light beyond 475μ, and in the best atmospheric conditions I caught a glimpse of faint traces of the bands 4 and 6. Unfortunately the star is only of the eighth magnitude, so that only few details of its spectrum can be seen with a telescope like ours. Nevertheless, what I did see seems to me of some importance in explaining the development of a spectrum II.*a* into III.*b*.

If this spectrum be compared with those of other stars of the same or even of a lower magnitude, such as 145 Schj. DM. + 34° 56, DM. + 36° 3168, it is at once seen that in the former the principal bands are still in a very low stage of development, and if the bands had only been a little paler nothing unusual would have been seen in the spectrum under ordinary atmospheric conditions. The aspect of this star seems to prove what I said above, that there is, properly speaking, no intermediate state between the spectra II.*a* and III.*b*, but that the passage from one to the other is already accomplished before the first traces have been perceived.

But there is still one more circumstance deserving of attention, which may perhaps lead to the knowledge of other spectra which are still nearer to the critical point; that is, the very strong absorption of the more refrangible rays, which makes the whole spectrum very short, and gives to the star itself its bright orange colour. We know that there are many stars of a deep colour and with short spectra, but otherwise not striking; they ought to be examined from time to time with very powerful microscopes, for amongst these will be found, I believe, the new spectra III.*b*.

There are other spectra, which, although they undoubtedly belong to Class III.*b*, have not, it appears, reached their full development. The least faint of these stars is that known as 7 Schj. Before my researches, nothing had been published regarding this spectrum except this short remark of D'Arrest, "Irregular spectrum, probably type IV." (*Vierteljahrschrift der Astr. Ges.* ix. Jahrg. p. 255). This spectrum presents the characteristics of III.*b* very pronounced; only band 5 is invisible, and band 6 is so faint that at first sight the spectrum has not the aspect characteristic of well-developed spectra of this class. It is for this reason that D'Arrest would say nothing positive regarding this star. If the spectrum of 541 Birm. represents the first step in the passage of a star to Class III.*b*, this star doubtless represents the second step. Band 6 is the least developed of the three principal ones. Although the spectrum of this star is pretty bright, band 5 is not visible, whilst band 4 is well visible, and is also perceived in the spectrum of 541 Birm.

In the spectrum of 19 Piscium (Fig. 4 on the map), which is one of the most magnificent in other respects, band 6 is still considerably fainter than the other two principal bands, whilst in that of 152 Schj. (Fig. 3) it is quite as pronounced as band 10, and almost as pronounced as band 9. This last spectrum is in an advanced stage of development; but in spite of that, band 4 is not stronger than in the spectrum of 7 Schj., and rather fainter than that of 19 Piscium. The same relation is repeated in other spectra of this class, so that sometimes band 4 is very visible in an otherwise less developed spectrum, but invisible in more strongly marked spectra, and in the spectra of brighter stars of this class there are in the same way very faint bands, 7 and 8. But band 4 is in itself very pale; it is the deep sodium line which makes it remarkable, and the bands 7 and 8 are probably only groups of Fraunhöfer lines.

It is therefore very probable that the more or less easy visibility of these bands is no indication as to the phase of development in which the star is. There is, on the contrary, reason to believe that the strengthening of these lines, and also of the other principal lines of the spectrum (except those of hydrogen, which grow fainter during the passage of a star to Class III.) is a process of relatively small importance which goes on whilst the star still undoubtedly belongs to Class II.*a*; and even when this is accomplished there is still nothing to show whether the star will become III.*a* or III.*b*, unless perhaps in those which tend towards Class III.*a*, the line, or rather group of lines, with wave-length 616, is very well marked, which seems not to take place in the spectra III.*b*. But in the stars which tend towards the latter class the violet rays are already very much absorbed, and the stars are therefore of a deep orange.

If we pass on to consider the ulterior development of the star, it is evident that as it cools further it at length reaches a temperature at which the carbon which must be present in abundance, either in its atmosphere or under some form in its photosphere, can combine with hydrogen or some other element to give the so-called hydrocarbon spectrum. After that, the spectrum appears cut by a broad faint band with the wave-length 516μ, and by another still paler at 473μ, and the parts of the spectrum beyond this are very faint. But gradually these two bands increase in intensity, and at the same time the band 563μ is perceived, at first very faintly, and gradually becoming stronger. At this stage the narrow band 576μ is developed, and finally the three principal bands are nearly of equal intensity, and the spectrum shows all the characteristic details. It would be useless to attempt to discuss the moment at which the secondary bands in the red and orange make their appearance, as no facts on the subject are known.

It is doubtless very remarkable that in the spectra III.*b* no trace of the carbon band with the wave-length 618·7μ is seen, which is so brilliant in Plücker's tubes containing hydrocarbon. This is, however, in perfect analogy with what is seen in the spectra of comets, which owe their appearance to the same carbon compound as the stellar spectra III.*b*, and there are analogies also for the other bands. Thus the band 563μ is often very weak even in the bright comets, and the band in the green is always the strongest both in comets and stars. The band in the blue is sometimes pretty faint in cometary spectra, whilst in the stars it is only a little fainter than the band in the green; but we must remember that it is situated in a very faint part in the spectra of the stars. It is therefore very possible that a little dimness should render the remaining light entirely imperceptible. In this perhaps there is no diversity between comets and these stars. The violet bands are very faint in Plücker's tubes, but strong in the flame of alcohol. A trace of them has been seen in the spectra of the brightest comets. In very brilliant, not too red stars III.*b*, there is also a violet zone, terminating at the wave-length 430μ, of which there is a band at the position of the first and the second of these bands in the spectra of these stars.

We will now pass on to consider the changes which take place in stars of Class III. after their spectra have completely developed. As the cooling goes on, they necessarily grow dimmer and dimmer, and at length become extinct. Either the bands in their spectra must increase in width until at last the shining intervals disappear, or else, the bands keeping their same width, the whole spectrum grows fainter. Certainly we see that there are stars whose bands are enormously broad, but none the breadth of whose bands surpasses that of the bright zones.

I think, therefore, we can hardly accept the first hypothesis, but there are reasons which give very valuable support to the second. We know that the weakness of the light in the solar spots is, in the first place, caused by a general obscuration of the spectrum, and that the enlargement of the Fraunhöfer lines has very little to do with it. Besides, I have examined, on different occasions, between the maximum and the minimum, the spectra of several variable stars of Class III., and found that there was no widening of the bands sufficient to explain the weakening of the stars. There is no doubt a remarkable analogy between the spectra of the sunspots and those of the stars of Class III., and one which we have no cause to be surprised at. For, on account of the relatively low temperature of these stars, it is very probable that their surfaces are in great part covered with formations similar to our sunspots, and the absorption-bands found in their spectra are no argument against this analogy. They prove only that chemical compounds may be formed and maintained in the atmospheres of these stars, which is not possible in our sun, not even in the masses of relatively low temperature of which the spots consist.

Before laying down my pen I must remark that the induction by which I arrived at these conclusions does not prove that the spectrum of each star commences with Class I. and finishes with

Class III. The development might just as well be in inverse order, though we have important reasons for believing it is not so.

The astronomy of the future must decide between these two alternatives. My object in undertaking this work was to facilitate this decision by giving as exact descriptions as possible of the spectra presented by the different stars of Class III. in the year 1880.

THE ART OF COMPUTATION FOR THE PURPOSES OF SCIENCE.[1]

II.

SOME few problems in astronomy and certain theories in pure mathematics require more than seven figures to be calculated. In these cases a large arithmometer is generally the most convenient. Ten-figure tables of logarithms may be obtained second-hand; or the required logarithms must be calculated.

The tables of Vlacq, re-edited by Vega in 1749, 1794, and 1797 are somewhat difficult to obtain and cumbrous to use. The logarithms of numbers up to 101,000 are given to ten figures with first and second differences. Thus to find log 10 542 482 375, from the table directly

log 10 542 =	·0229 230 119	Δ_1 = 411 946
	198 712 3	482 375
	5	
		1 647 784
log required	·0229 428 836 3	329 557 +
		8 239 +
The true log of 10 542 482 375 is		1 236 +
·022 942 883 626 562.		288 +
		20 −
		198 712 3 1 subtracted.

$$\Delta_2 = 40$$

$$\frac{\cdot 48(\cdot 48 - 1)(-40)}{2} = 4\cdot 992.$$

In default of Vega, or if more places are required, the logarithm must be calculated, and this is by no means such a serious affair as one is led to think by the ordinary books on algebra. I am much indebted in what follows to the article by Mr. J. W. L. Glaisher on logarithms in the new edition of the "Encyclopædia Britannica," to which I refer my readers for further particulars in theory, restricting myself to practical details.

The easiest way to calculate a table of logarithms absolutely *de novo* would be by the method of differences, with some mechanical assistance, such as the difference-engine of Babbage or of Scheutz. It seems unlikely that larger tables will be calculated than those already in existence, since the cost increases with great rapidity. Mr. Sang has, however, recently calculated independently the logarithms of numbers from 100000 to 200000, where the ordinary tables are weakest.

Briggs used at least two methods for the calculation of logarithms which depended upon the extraction of a succession of roots. For instance, by taking the square root of 10 fifty-four times he found log $1\cdot(0)^{15}1$ 278 191 493 to be $\cdot(0)^{15}0$ 555 111 512. Whence assuming that very small numbers vary as their logarithms, log $1\cdot(0)^{15}1$ = 555 111 512/1 278 191 493, or log $1\cdot(0)^{15}1$ = 0·43 429 448 = M, the modulus. And if x be small, log $1\cdot(0)^{15}x = x \times 0\cdot 43\ 429\cdot 448$. To find log 2 he extracted the square root of the tenth power, 1024/1000 forty-seven times, and found $1\cdot(0)^{15}1$ 685 160 570, which multiplied by M gave $\cdot(0)^{15}0$ 731 855 936. This multiplied by 2^{47} gave log 1·024; adding 3 and dividing by 10 gives log 2. Another more simple method was to find a series of geometrical means between two numbers, such as 10 and 1, the logarithms of which are known. After taking 22 of these roots, log 5 is found to be 0·69897.

It was soon found that logarithms could be more easily calculated by the summation of various series, and many great mathematicians, such as Newton, Gregory, Halley, Cotes, exercised their ingenuity in discovering those most suitable for the purpose.

Though for practical purposes the use of series has been almost superseded, three very simple ones are still occasionally useful:—

$$\log(1 \pm x) = \mathrm{M}\left(\pm x - \frac{x^2}{2} \pm \frac{x^3}{3} - \frac{x^4}{4} \pm \frac{x^5}{5} - \right)$$

which converges rapidly if x be small. M is a number depending upon the system of logarithms adopted, and constant for each system. If M be 1, the system is called the Naperian, or natural one; and if M = 0·434 &c., the system is the common one. Unless otherwise stated M will be assumed to be 1, or the logarithms will be natural ones.

Thus to calculate log 1·1 = 1 + $\frac{1}{10}$, omitting M:—

$$\log 1\cdot 1 = \frac{1}{10} - \frac{1}{200} + \frac{1}{3000} - \frac{1}{40000} + \frac{1}{500000} -, \text{\&c.,}$$

$$= 0\cdot 1003\ 3534 - 0\cdot 0050\ 2517 = 0\cdot 0953\ 1017.$$

Suppose x be small, log $(1 \pm x) = \pm\, \mathrm{M}x$ nearly. Thus if log $1\cdot(0)^{10}9$ be required to twenty decimals, it is

$$\cdot(0)^{10}9 - \frac{1}{2}(\cdot 9 \times 10^{-10})^2,$$

or the error caused by omitting this and all subsequent terms is only 4 in the twenty-first decimal place. Using common logarithms the multiplication by M reduces the error by one-half. This result is of great importance in calculating logarithms by Flower's method, since the factors which have to be dealt with are only half the number of decimal places in the required logarithm.

Writing $\frac{1}{x}$ for x in the above series, we obtain—

$$\log(1 + x) - \log x = \mathrm{M}\left(\frac{1}{x} - \frac{1}{2x^2} + \frac{1}{3x^3} - \frac{1}{4x^4} + \frac{1}{5x^5} - \right)$$

which converges rapidly when x is large. Various artifices may be used to render x large, even when the number the logarithm of which is required is small. Thus, Prof. J. C. Adams has calculated (NATURE, vol. xxxv. p. 381) log 2, log 3, log 5, log 7, $\frac{1}{\mathrm{M}}$ and M to 270 places of decimals.

Another very valuable series is—

$$\log(a \pm x) = \log a \pm 2\mathrm{M}\left\{\frac{x}{2a+x} + \frac{1}{3}\left(\frac{x}{2a+x}\right)^3 + \frac{1}{5}\left(\frac{x}{2a+x}\right)^5 + \text{\&c.}\right\}$$

Thus, supposing log 219 known, to calculate log 2198:—

$$\log 2198 = 7\cdot 6916\ 5682\ 2810 + 2\left\{\frac{4}{2194} + \frac{1}{3}\left(\frac{4}{2194}\right)^3 + \text{\&c.}\right\}$$

	·0036 4630 8113	$\frac{4}{2194}$ = ·0018 2315 40565
	4039	
log 2198 =	7·6593 0313 4962	$\frac{2}{3}\left(\frac{1823}{10^6}\right)^3$ = ·$0^8$4039

Using common logarithms, the third term of the series is $< \frac{1}{27\cdot 6}\left(\frac{x}{a}\right)^3$, that is less than 5 in the ninth place when $\frac{x}{a} < \frac{1}{200}$. Hence, with a table giving the logarithms of 100–1000 to eight figures the third term may be neglected, or the required difference is $\pm \frac{2\mathrm{M}x}{2a + x}$, or, writing $\log(a + x) - \log a = y$,

$$x = \frac{2ay}{2\mathrm{M} - y}.$$

The given numbers may also be broken up into factors by the aid of such a table as Burkhard's, which gives the factors of all numbers up to 3,036,000. The logarithms of the factors may then be found from tables and added together. Of all tables for this purpose, that of Wolfram is the most valuable; it gives the natural logarithms to forty-eight places of *all* numbers up to 2200, and of all which are not easily divisible up to 10,009.

The multiplication by M to convert into common logarithms is tedious, and it is frequently better to dispense with it in heavy calculations. If necessary, a table of the first ninety-nine multiples of M should be prepared, and Oughtred's short method of multiplication used.

If any of my readers desire to test themselves and their tables

[1] Continued from p. 239.

by a long but easy calculation, the amount of £1 laid up at 5 per cent. compound interest for a thousand years will be found not to differ very much from £1,546,318,920,731,927,238,982. An answer of this sort is of course of no practical utility whatever, but it brings vividly before us an important point in political economy—the accretion of wealth in the hands of corporations. It was computed that just before the Revolution more than half the soil of France was owned by the Church. Looking at this array of figures, and remembering that since the Church could never alienate its property all surplus income must be regarded as at compound interest, we can only wonder that it was the half and not the whole.

The first table for facilitating the computation of logarithms was one given by Long (Phil. Trans., 1724) of the decimal powers of 10 to nine figures. Thus, to find the number the logarithm of which is

$$\cdot 30103 = 10^{\cdot 3} \times 10^{\cdot 001} \times 10^{\cdot 00003} = 1\cdot 99526231 \times 1\cdot 00230523 \times 1\cdot 00006908 = 1\cdot 99999997, \text{ or } 2.$$

This method is cumbrous, but it is perhaps one of the most simple for explaining the calculation of logarithms to beginners.

A much more convenient method has been well worked out by M. Namur, but, unfortunately, only his twelve-figure table seems to be still in print. The table contains the logarithms of numbers from 433300 to 434300 to twelve figures, and the numbers corresponding to logarithms from 637780 to 638860. By the aid of certain factors which are tabulated with their complementary logarithms, any number or logarithm can be reduced between these limits.

Thus, to find log π—

314 159 265 359 × 1·3
94 247 779 607 7

408 407 044 956 7 × 1·063
24 504 422 698 0
1 225 221 134 9

434 136 688 799 6

log from table 637 625 800 474 Δ = 1·000364
206 4
41 3
2 4

637 626 489 524
973 466 735 477 } complementary logs of
886 056 647 693 } 1·3 and 1·063

497 149 872 694 = log π.

The last method I shall mention is generally known by the name of Weddle; it was probably used by Briggs, and published by Flower in 1771. It consists in multiplying the given number by a series of factors of the form $1 \pm \frac{x}{10^n}$ until it is reduced to one. The complement of the sum of the logarithms of the factors is the required logarithm. The logarithms of the factors are easily calculated by the first series; they have been tabulated to about thirty places.

Thus to find log 3550·26:—

355026 × 2	30103
	11394 3
710052 × 1·3	3342 4
2130156	130 1
	3 9
9230676 × 1·08	
738454	44973 7
	55026 complement.
9969130 × 1·003	
29907	
9999037 × 1·00009	

Hence log 3550·26 = 3·55026, or we have a number which is expressed by the same figures as its logarithm.

It is the present fashion, while depreciating our own country men, to extol all Germans in matters connected with education, and especially to award them the palm for patient plodding. It will be some time before a German rivals Prof. Adams, and even then there is a height beyond. Of all monuments of calculation the value of π, or the number of times the circumference is longer than the diameter of a circle, is most astounding. Archimedes found it to be $\frac{22}{7}$, Wolf calculated it to 16 places, Van Ceulen to 35, Machin to 100, Beerens de Haan to 250, Richter to 500. But in 1853 Mr. Shanks threw all these results into the shade, and excited the admiration even of De Morgan by calculating π to 530 places, "throwing aside as an unnoticed chip the 219th power of 9"! Two printers' errors were pointed out by Mr. John Morgan, which Mr. Shanks corrected from his manuscript, and in 1873 gave a new result to 707 places. Hence the value of π is known to within $\frac{1}{3 \times 10^{707}}$, an exactness which is useless from the inability of the human mind to comprehend the figures which express it.

Clerk Maxwell proposed, possibly in irony, to take the wave-length of a certain light as the universal unit of length. Choosing for this purpose about the middle of the violet, a mile would be expressed by $60000 \times 63360 = 3\cdot 8 \times 10^9$ units nearly. Suppose that Sirius, the brightest star in our firmament, has an annual parallax of $\frac{1}{5}''$, a quantity perceptible, but barely measurable, by our best telescopes, the distance of the sun from Sirius is about $5 \times 206,265 \times 92,300,000$ miles, or $3\cdot 5 \times 10^{23}$ units. Assume again that Kant's fanciful conjecture is correct, and that the sun revolves round Sirius in a circle the length of which is expressed by $7 \times 10^{23} \times \pi$ units. Make the still greater assumption that all our measures are correct, and our arithmetic as it ought to be, so that the only possible error would be in the evaluation of π. The greatest possible error according to Mr. Shanks's determination would be $\frac{7 \times 10^{23}}{3 \times 10^{707}}$ or $\frac{1}{4\cdot 3 \times 10^{683}}$ of a wave-length of violet light. Whatever metaphysicians may say, I think we have here reached, if not surpassed, the limits of the human understanding.

SYDNEY LUPTON.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, January 2.—M. Janssen, President, in the chair.—On an objection made to the employment of electro-magnetic regulators in a system of synchronous time-pieces, by M. A. Cornu. This is a reply to M. Wolf's recent communication, in which several objections were urged against the apparatus in question. It is shown (1) that such a regulator does not necessarily tend to stop the system to which it is applied; (2) that in any case the stoppage may be prevented without complication or expense; and (3) that in a public time-distributing service the stoppage should not only not be prevented, but efforts should be made to bring it about whenever the synchronizing system gets out of order. The paper was followed by some further remarks on the part of M. Wolf, who reiterated his objections, and treated M. Cornu's third point as somewhat paradoxical.—Remarks on Père Dechrevens's letter regarding the artificial reproduction of whirlwinds, by M. H. Faye. The author complains that, like other partisans of the prevailing ideas on the subject of tornadoes, typhoons, and cyclones, M. Dechevrens endeavours to suit the facts to the exploded theory of an ascending motion in the artificial reproduction of these aërial phenomena.—On the meteorite which fell at Phû-Long, Cochin China, on September 22, 1887, by M. Daubrée. In supplement to M. Delauney's communication of December 19, the author adds that this meteorite was an oligosiderite of somewhat ordinary type, closely resembling those of Tabor (Bohemia), July 3, 1753; Weston (Connecticut), December 14, 1807; Limerick, September 10, 1813; and Ohaba (Transylvania), October 10, 1817.—Remarks in connection with the presentation of the "Annuaire du Bureau des Longitudes" for 1888, the "Connaissance des Temps" and the "Extrait de la Connaissance des Temps" for 1889, by M. Faye. Amongst the fresh matter added to the "Annuaire" this year are papers by M. Janssen on the age of the stars, by Admiral Mouchez on the progress of stellar photography, and by M. d'Abbadie on his recent expedition to the East in order to determine the elements of terrestrial magnetism in Egypt, Palestine, and Syria.—Observations of Olbers' comet made at the Observatory of Nice (Gautier's 0·38 m. equatorial), by M. Charlois. These observations are for December 25, 26, and 27, after the comet was discovered on December 23, when the nucleus was of the tenth magnitude, surrounded by a bright nebulosity, and with tail from 20′ to 25′ in length.—On the total eclipse of the sun observed on August 19, 1887, at Petrovsk, Government of Jaroslav, by M. G. M. Stanoiewitch. Owing to the extremely unfavourable atmospheric conditions the observer was unable to carry out any important part of his programme. A chief result of his observations was the conclusion that the gloom prevailing during eclipses is all the deeper the less clouded is the sky and the flatter the ground, especially on the horizon. The sky being on this occasion almost completely overcast, he was able to read the title of a pamphlet printed on a red cover at a distance of 2 metres.—On the variations of temperature of gases and vapours which preserve the same quantity of heat under different tensions, by M. Ch. Antoine. A simple means is proposed for avoiding the laborious calculations required to determine the values Θ and Θ^1 in the formula $y = 25\sqrt[3]{\Theta - \Theta^1}$ deduced from V. Regnault's experiments on atmospheric air.—On the energy needed to create a magnetic field and to magnetize iron, by M. Aimé Witz. The researches here described serve to verify Lamont's statement that the effect produced by a magnetic field on a magnet is greater when the force acts to diminish than it is when the force acts to increase the magnetizing power.—On the rapidity of transformation of metaphosphoric acid, by M. Paul Sabatier. Solutions of metaphosphoric acid are transformed spontaneously with greater or less rapidity. Berzelius and Thomsen suppose that there is at first production of pyrophosphoric acid, which is afterwards changed to orthophosphoric acid. Others, with Graham, think that there is immediate formation of tribasic orthophosphoric acid, and the author's researches tend to show that this is normally the case. It is also established that the rapidity of transformation is at each instant proportional to the mass of transformable substance present in the system.—On an alloy of titanium, silicium, and aluminium, by M. Lucien Lévy. Wöhler indicated two alloys of these metals without giving their composition. The author here determines a similar alloy differing in some of its properties from those of Wöhler. He has also determined its composition, as apparently a mixture of two isomorphous bodies crystallized together with formula $TiAl_4$ and $SiAl_4$. The same preparation with zinc or magnesium substituted for aluminium yielded no results.—On some derivatives of cinchonine, by MM. E. Jungfleisch and E. Léger. The authors were able some time ago to announce that the sulphate of cinchonine being heated to 120° C. for forty-eight hours with a mixture in equal parts of sulphuric acid and water, the alkaloid changes to diverse bases, of which they have isolated the six most abundant. Here they explain the process by which they have succeeded in separating the alkalies.—On the presence of diaphragms in the aëriferous ducts of roots, by M. C. Sauvageau. The transverse diaphragms intersecting the aëriferous ducts of vascular plants have hitherto been supposed to be confined to the middle region of the bark of their various members. But the author has now determined their presence also in the root of at least one such aquatic plant, the *Hydrocharis morsus-ranæ*.

Berlin.

Physiological Society, December 16, 1887.—Prof. du Bois Reymond, President, in the chair.—Herr Meyer, from Hamburg, discussed the nature of ventriloquism, and combated the opinion, so widely spread among physiologists, that it consists in speaking while inspiring, and without the cavity of the mouth acting in any way as a resonator; on the contrary, ventriloquists speak while expiring, and do move their mouths. An extended series of laryngoscopic observations on the speaker, who has practised ventriloquism for many years, has shown that in ventriloquizing the vocal opening of the larynx is shortened as it is when producing the falsetto, and that the soft palate is pressed back and that the uvula becomes invisible. Everybody who naturally possesses a high voice can easily learn to ventriloquize. One most important factor in the deception of the listeners is the contrast between the loud, full and metallic tone in which the question is asked and the answer which immediately follows in a high and gentle falsetto. Sibilants and the high I should be as far as possible avoided. The speaker then gave a series of extremely successful examples of ventriloquism, which did not presuppose any particular training, and showed that it is never accompanied by any special action of the abdominal muscles. Prof. Gad has made some experiments on Herr Meyer, and by graphically recording the variations in pressure of the air, has shown that the curve obtained when a certain sentence is spoken in the ordinary way is in all respects identical with the one which is described when the same sentence is spoken ventriloquially. In the latter case the volume of air expired was considerably less than during normal speech; in one particular case it amounted to only 900 c.c., whereas during normal speech the volume expired was 1300 c.c. Dr. Benda expressed his idea that when ventriloquizing the Eustachian tubes are open and the cavity of the tympanum, together with the tympanic membrane, are set into simultaneous vibration. He had not been able to detect any resonance of the tympanic membrane in Herr Meyer; but he believes that this explanation of the curiously veiled tones emitted is not thereby invalidated, since they closely resemble the tones produced by speaking while yawning, in which case the Eustachian tubes are certainly open and the tympanic cavity acts as a resonator.—Dr. Benda gave a further account of his researches on the development of spermatozoa, and referred to several works which have been recently published and do not agree with the results obtained by himself. For his own part he could only confirm his earlier opinions by his later researches. In Marsupials he finds some resemblance to that which holds good in Sauropsida. In general it may be said that the very varying relationships observed in Mammalia between the parent-cell and the spermatozoa-cells which are connected with this may be looked at from one common point of view; it is only necessary to adopt for animals the differentiation of the cells of pollen-grains, observed by botanists, into vegetative or nutritive, and into generative, from which the spermatozoa then arise. These vegetative and generative cells can be made out both in the functioning and not yet active testes of embryos, the cells having extremely varying relations each to the other.

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