THURSDAY, SEPTEMBER 30, 1886

OUR GUNS

WHEN a careful engineer sets about designing a structure, he first determines the strain which the several parts of it will have to withstand; he then selects his material and proportions it so that it will be able to carry the strains safely; in determining the margin which should be allowed he uses judgment based upon his own experience or that of others who have designed similar structures; and if the strains be difficult to determine, or if they be of the nature of severe and sudden shocks and complicated cross strains he increases his margin in proportion. Experience has shown, for example, that wrought iron in the form of a railway bridge may be worked safely at a load equal to one-fourth of that which will break it down, a cast-iron bridge to one-sixth. The builder of a steam-engine rarely loads those parts of his machinery which have to endure sudden and reverse strains to more than one-eighth or one-tenth of their ultimate strength. If his structure fail, the first thing the engineer does is to re-calculate his strains and the dimensions he has given to the various parts, and if these should prove correct he seeks for the cause of failure in unlooked-for defects in his material; and if failure in the same class of structure, of various sizes, recur repeatedly in the same place, he comes to the conclusion, either that he has under-estimated the strains, or that the margin of safety which he has allowed is not sufficient. An engineer accustomed to act in this manner must look with dismay upon the report of the Committee appointed to examine into the cause of the failure of the 12-inch gun of the *Collingwood*, and of other guns of similar construction It is possible, of course, that the Committee have been able to calculate the strains which tend to destroy the guns, and have satisfied themselves that sufficient metal has been provided for the purpose; but, if so, it is much to be regretted that they have not seen fit to make the results of their investigations public, because it would have been instructive to know how the stresses are arrived at and what margin of safety is considered sufficient for a gun. When Colonel Maitland read his paper on our new guns at the United Service Institution in the middle of 1884, the strains certainly were not known to the Ordnance Committee, because the pressure curves, purporting to represent one-fourth of the bursting pressure of the guns, and which were given on the official drawings, were incompetent to account even for the muzzle energy which the shot was supposed to possess, and consequently provided nothing whatever for other important work which has to be performed during the discharge. This fact was pointed out by the *Engineer* early in 1885, in an article commenting on one of the Howard Lectures which had just been delivered at the Society of Arts, and the weak point in our guns was actually indicated before any failures had taken place!

How much has the knowledge of the Ordnance Committee advanced in the meantime, and is four still considered a sufficient factor of safety? Any careful engineer who reflects on the strains a gun is subjected to would certainly class it among steam-engines, subject to the roughest work, and not to the class of railway bridges, which have well-defined and simple strains to stand. What takes place when a gun is discharged? First, there is the direct pressure of the gases, which is still very imperfectly known. Next, this pressure travels along the bore at a very rapid rate, producing a shearing strain between the material in advance of, and in rear of the base of the shot, and this strain is intensified at each point where a sudden change of thickness takes place. Thirdly, there is the reaction to the force producing rotation of the shot, which tends to twist the barrel. Fourthly, there are the strains produced by the momentary presence of a white-hot body pressing against the walls of the bore with a pressure ranging from 25 tons to 1 ton per square inch; and, lastly, there is the longitudinal strain representing the reaction between the pressure on the base of the bore and the inertia of the gun itself. What engineer would dream of counteracting such strains as these with a less margin than eight or ten, if he were perfectly unfettered, and if he were certain of the manner in which the strains were transmitted through the metal of the gun; but even on this point there are grave doubts, for it seems almost certain that the strains travel as pulses or vibrations, in a manner which sets at defiance all ordinary modes of calculation. No engineer would be surprised if guns, with a factor of safety of only four, burst frequently.

But it will, no doubt, be argued that guns of the strength suggested would be impracticable. We do not hold that opinion, because weight in a gun, even for naval purposes, is not objectionable, since the force of recoil diminishes with the increase of weight, and the metal appropriated to the gun would be saved in the carriage and structure of the ship; while, for land service, weight can be no objection whatever. The Committee make no allusion to a necessity of keeping down the weight of guns, hence it must be supposed that they would not sacrifice safety to this end, although it is well known that there is a kind of race among gun-makers to produce the greatest amount of shot-energy per ton of gun. It seems to us, therefore, most unfair that the whole blame of our failures is to rest on the Royal Gun Factory, that is, on the quality of material and on the manufacture, when it is certain that the faults are faults of design—a want of sufficient metal—a view which the Committee adopt by their acts, though not by their words; for they are adding largely to the weight of the guns in the very parts which experience and the investigations of outsiders have proved to be deficient in strength.

But although the Ordnance Committee are reticent as to the scientific views which they hold respecting the structure of guns, they have a way of taking the public into their confidence, through the instrumentality of a lecture delivered by one of their body, whose talents as an agreeable expounder of popular science are well known, and they show much wisdom in making these manifestations take the form of lectures instead of papers read before scientific Societies. The advantage of a lecture is that no awkward questions can be asked, and no fallacies or errors pointed out. Thus, when the *Thunderer's* gun burst, Sir Frederick Bramwell delivered a very pleasant lecture at the Royal Institution, and the other day the same gentleman selected the subject of our guns as the theme of his address to the Midland Institute at Birmingham, and, we have no doubt, charmed his audience with his manner and ready wit. But what is there in either of the lectures just mentioned that throws the slightest light on the real difficulties of the subject? Why was not Sir F. Bramwell moved to read a paper at the meeting of the British Association which took place a few weeks ago in the very building in which he addressed the Midland Institute? Had such a paper been read, a most interesting and animated discussion would have arisen, and the nation would have had the advantage of learning the opinions of men who have devoted their lives to working out the problems connected with the theory as well as the practice of the construction of ordnance. We are driven to the conclusion that the Ordnance Committee considered that such a course would have been dangerous to their self-respect; the deplorable ignorance which characterises all attempts at working out difficult questions by Committees would have been too glaringly exposed; it was much safer not to subject themselves to cross-examination.

If we are wrong, if the Committee have complete answers to the questions raised, it is open to them to convince the nation of the fact, because the sessions of the Institution of Civil Engineers will very soon commence, and some member of the Ordnance Committee should be deputed to read, not a popular lecture, but a serious paper that will demonstrate to a roomful of practical engineers that our guns have been constructed according to the rules which guide engineers in their ordinary work, and that yet they have failed. We have small hopes that the challenge will be accepted.

THE MATHEMATICAL AND PHYSICAL SCIENCES

Histoire des Sciences Mathématiques et Physiques. Par M. Maximilien Marie. Tomes I.-IX. (Paris: Gauthier-Villars, 1883-86.)

M. MARIE'S great work advances rapidly towards completion. The first three volumes appeared in 1883; the concluding three are in the press; we have now before us nine volumes, bringing down the narrative from the time of Thales to the time of Laplace. The undertaking is a vast one; and we are not surprised to hear that it has cost forty years of preparation. The learned world is to be congratulated that it has fallen into such able hands. M. Marie combines, in a rare degree, scientific with literary qualifications. A certain grace and poignancy of style set off his wide erudition and practical acquaintance with methods of teaching. He can be vivacious even over processes of integration. The accumulated mass of his materials nowhere hinders the lightness of his tread. Keen touches of sarcasm enliven his most abstruse expositions, and agreeably remind his readers that a sense of humour may subsist concurrently with a thorough mastery of the higher analysis.

He has accordingly produced a book which, with these merits and some corresponding defects, only a Frenchman could have written—one eminently interesting and original, at once lively and profound, instructive throughout if occasionally one-sided, frankly displaying the prepossessions of its author, and not unfrequently—as we shall presently show—his heedlessness of historical and biographical accuracy. Its characteristic merit consists in the lucid interpretations contained in it of the older methods of mathematical research. The works of ancient and mediæval geometers are analysed, not barely in the view of exhibiting the results attained by them, but with the further purpose, most completely realised, of rendering their various artifices and modes of working intelligible to the least skilled in the archæology of mathematics. M. Marie's is indeed in no sense a book for beginners. It presupposes a considerable acquaintance with the most recent developments of analysis. The reader thus provided may, however, follow with ease and pleasure the steps by which earlier inquirers advanced; he may enter into their conceptions, place himself at their precise point of view, and while marvelling at the ingenuity which carried them so far, study the limitations of thought which hindered them from proceeding any farther. He may learn how the singular deficiency in the idea of abstract number which hampered the workings of such luminous and powerful minds as those of Archimedes, Apollonius, and Euclid, was supplied from the far East; how Hindu algebra and arithmetic formed the complement of Greek geometry; how both were transmitted through Arabic channels to Italy, and together constituted the starting-point of modern discoveries. Nor is it less curious to watch the gradual emergence of ideas big with the progress of the future, such as those of negative and imaginary, or infinitely small quantities; how they presented themselves with hesitation, and were at first shunned and distrusted; how they grew bolder and insisted on recognition; how their tentative and partial treatment became widened and generalised until they finally developed the whole extent of their capabilities.

It is well known that Archimedes gave the first approximation to the value of π; but the occasion and significance of the step are often lost sight of. It marked, with the almost simultaneous attempt of Aristarchus of Samos to measure the relative distances from the earth of the sun and moon, the introduction of numerical calculation into theoretical researches (Marie, t. i. p. 59). The novel effort was prompted, in each case, by the interest of a special problem. Archimedes, naïvely enough, sought to prove that the idea of infinity had its root in enumerative impotence, and could be abolished by expanding the resources of arithmetic. He exemplified his contention by computing the number of grains of sand contained in a sphere with the interval from earth to sun for its radius. But a preliminary valuation of the ratio between the circumference of a circle and its diameter was indispensable; and the tract on the "Dimension of the Circle" was accordingly, in M. Marie's plausible view, written as a kind of preface to the "Arenarius." Incidentally to the calculations in the latter treatise, he perfected the Greek system of numeration, and foreshadowed the principle of logarithms.

M. Marie has ventured a kind of restoration of the "algebra" of Archimedes (t. i. p. 262). His remarks on this disputed subject are of great interest. He holds it impossible that his inventions should have been reached by the arduous path of his demonstrations; and ascribes to him, accordingly, the possession of a compendious method of reasoning founded on the transformation of ratios, conducting easily to results otherwise unattainable even by his transcendent genius. The secrecy observed regarding it may have been due, in part to the difficulty of setting it forth in the absence of a suitable algorithm, in part to the conventional prevalence of the synthetic mode of exposition. The art was doubtless held in common with Apollonius, and other geometers of the time; but was treated as a mere rude tool, not worth the labour of bringing to a higher perfection. Had this early analysis been independently cultivated, and set free from its servitude to geometry, the "method of exhaustions" must, by its aid, in our author's opinion, have given birth to the Calculus eighteen hundred years before Cavalieri thought of his "infinitesimals."

The work before us aims, above all, at developing, as a coherent whole, the logical sequence of ideas. It would be unfair to say that this aim has been missed; yet we cannot help thinking that it might have been more perfectly attained. A vivid light, it is true, is frequently thrown upon obscure passages of research, and the filiation and significance of discoveries are, here and there, brought out with uncommon sagacity. Nevertheless, there is something wanting which M. Marie could easily have supplied.

A somewhat fragmentary plan has governed the composition of the book. The twenty-four centuries embraced by it are divided into sixteen periods of very unequal length, treated each in a section apart, consisting of a prefatory sketch of the progress accomplished during its course, followed by a series of biographies of those who contributed towards it, arranged in strict chronological order. Chemists and mathematicians, astronomers and botanists, mechanicians and physiologists, are thus placed side by side, with no closer tie of connection than the successive occurrence of the years of their birth. We have no sooner done with Lagrange's Calculus of Functions than we are confronted with Watt's transformation of Newcomen's steam-engine; the convulsions of Galvani's memorable frog succeed; and we pass thence to Parmentier's triumphant growth of potatoes on the plain of the Sablons,—all subjects of great interest, and treated with singular charm. Yet their variety, if it form, in a certain sense, an attraction, demands a stronger bond of unity than is here afforded. The true historical element, in short, is deficient. Nor is the want satisfactorily supplied by M. Marie's sixteen prefaces. We should be sorry to lose them; but they do not suffice. The absence of a connected narrative is still sensibly felt. Even if its design had otherwise remained unchanged, the book might at least have been provided with a general introduction, delineating and characterising the course of events to be detailed, pointing out the confluence, at epochs of discovery, of various and distant streams of thought, and presenting, in one luminous view, the progress in exact knowledge made by our race from the first dawn of civilisation until now. Perhaps it may not even yet be too late to add to a most valuable work a supplementary volume which would go far towards rendering it complete.

In another direction, M. Marie has perhaps unduly extended the scope of his enterprise. To have treated adequately the history of *all* the sciences, natural as well as mathematical, would have demanded, not a dozen, but fifty volumes. Yet all are nominally included in his scheme, while, in point of fact, those branches of knowledge remote from his principal theme receive only the casual attention of some stray jottings, with biographical notices of their leading promoters.

His choice of representative names, too, is open to criticism. Among omissions, that of Adelard of Bath, the first translator of the "Elements of Euclid" from the Arabic, is very remarkable. He was one of the most effective popularisers of Arab science in the thirteenth century, and played no unimportant part in the revival of mathematical learning. Yet he is not only ignored by M. Marie, but his version of Euclid is handed over to Campanus of Novara, with whose commentary it was published at Venice in 1482, and who has in consequence frequently gained the credit of its execution.

Nor should the unfortunate Cecco d'Ascoli have been altogether forgotten. His "Acerba," if not all that it has been claimed to be, contains many striking intuitions of natural truths. M. Marie, however, takes little interest in the premonitory symptoms of discovery; and the rage for unearthing its obscure anticipations has possibly been carried a little too far. We miss, further, the name of Giambattista della Porta, the *effective* inventor of the camera obscura, whose "Magia Naturalis" was of European fame and influence. And William Cullen, the founder of rational chemistry in Great Britain, was at least as well worthy of notice as Kunckel, known in connection with the manufactures of ruby glass and of phosphorus, to whom just three times the space is allotted as to Black, the discoverer of "fixed air," and of "latent heat."

A crowd of superfluous names, on the other hand, might be cited. It seems ungracious to object to the presence of a sketch so interesting in itself as that of the career of Ambroise Paré; yet it is not easy to see what the treatment of gunshot wounds has to do with the history of mathematical or physical science. Equally outside the proper scope of its cognisance are Henkel's improvement of Dresden china, Bosc d'Antic's contributions to the art of glass-making, Perronnet's bridges, Trudaine's "superb" highways, and Vaucanson's automata. If these give a valid title to admittance, why exclude Hargraves, Arkwright, Smeaton, MacAdam, and a host besides? Why should the spinning-jenny be passed over in silence, when Loriot's ribbon-loom comes in for honourable mention? In truth, however, industrial and mechanical inventions belong elsewhere.

It remains that we should justify our hint that M. Marie's facts and dates occasionally stand in need of revision. A perusal of Prowe's "Life of Copernicus" would have obviated several inaccuracies in his brief account of the reformer of astronomy. The intention of Copernicus to embrace the ecclesiastical state was not superseded, even momentarily, by his journey to Italy; on the contrary, he received his appointment as Canon of Frauenburg in 1497, shortly after he had entered the University—not of Padua, as stated by M. Marie, but of Bologna. Nor was he ordained a priest at Cracow in 1501. He took minor orders on entering the Chapter, but never became a priest; and his sojourn in Italy was unbroken between 1501 and 1505. Moreover, his doctoral degree (not in medicine, but in canon law) was conferred at Ferrara, May 31, 1503. He sought no diploma at Padua, though he studied medicine there during the four years of his *second* stay in Italy. The assertion that at the age of twenty-seven he was summoned to profess mathematics at Rome is inexact. Uninvited, so far as is known, he repaired thither with his brother early in the year of jubilee (1500); and delivered unofficially some brilliantly-attended lectures during the ensuing winter. Finally, he settled down to his life's work at Frauenburg, not in 1510, but in 1512.

One rubs one's eyes in amazement to find Basil Montagu's discredited and superseded edition quoted as the best and completest of Bacon's Works. Is it really possible that the news of the late Mr. Spedding's labours has not yet reached Chantilly? It would appear not; for his name is unmentioned by M. Marie, who equally overlooks Mr. Fowler's instructive edition of the "Novum Organum."

In the date assigned for Robert Hooke's death, he copies an error of Poggendorff's, who states that he died at the age of eighty-seven, whereas he did not live to complete his sixty-eighth year. The substitution of 1722 for 1703 is of more than simply formal importance, since the publication of Newton's "Optics" and his acceptance of the Presidentship of the Royal Society, both depended upon the event thus post-dated. Newton loathed controversies, although drawn into many. But while Hooke lived, they could only be avoided by self-effacement; and this was accordingly the policy adopted, as far as possible, by his great rival.

M. Marie makes no secret of his aversion to the sour little professor of Gresham College; and it is too true that his character repels sympathy, while his achievements were not of the dazzling sort to blind men to his failings. Still, his claim to due recognition remains intact, although ignored by our author, who states openly that only Poggendorff's eulogies, by "forcing his hand," frustrated his intention of punishing his egotism with neglect. Yet Hooke, by Newton's express admission, discovered independently the law of inverse squares, and it is not too much to say that, but for his incitements, Newton would not have undertaken the investigation which led to his immortal discoveries.

M. Marie's grudge against Hooke does not seriously detract from the value of his work; but it is otherwise with his ill-will towards Newton. Not only is he avowedly the partisan of Leibnitz in the never-ending debate concerning the invention of the Calculus; but his dislike (not wholly unjustifiable) to Newton's conduct in the matter extends to all the processes of his mind. He compels himself, it is true, to utter a few obligatory words in acknowledgment of the greatness of his work; but its entire significance seems to escape him. His readers are only quite casually reminded that the discovery of the system of the world was of greater moment in the history of science than the solution of the problems of the brachystochrone, or of the centre of oscillation.

We are told by M. Marie that Newton represented his University in Parliament from 1688 until 1705, during which time he was assiduous in his attendance, but spoke once only, to request the usher to shut a window (t. v. p. 170). In fact he sat three (1689-90, and again 1703-5), not seventeen, years. At p. 162 of the same volume we meet the surprising statement that Halley predicted for 1682 the return of the comet observed by Kepler in 1607. It is almost superfluous to remark that its appearance in 1682 was as unlooked-for as that of any of its predecessors, that its periodical character was then first divined, and in 1759 verified.

The observation made by Wilcke in 1787 that the auroral corona forms in the magnetic zenith is attributed by our author to De Mairan in 1747. The eminent Academician, we can assure him, would have been the last to welcome a remark so subversive of the arguments by which he sought to efface the magnetic character of the phenomenon. Neither is it the case that Halley noticed the bisection of the auroral arch by the magnetic meridian. Obvious though the coincidence appear, it was first pointed out by Ussher in 1788.

We note further M. Marie's curious incredulity as to the authentic measurement, so far, of the parallax of any single star; his statements that the accepted value of the solar parallax is $8''\cdot5$ (t. ix. p. 43), that the mass of the moon is $\frac{1}{88}$ that of the earth, and that Herschel detected interstitial movements in resolvable nebulæ (t. ix. p. 145); finally, his ascription to Sir *William* Herschel of the translation of Lacroix' "Differential Calculus" executed by his son conjointly with Babbage and Peacock.

These and other similar blemishes, however, are very far outweighed by the merits of the work in which they occur. It is one of marked individuality; and individuality lends interest, if it sometimes begets defects. No student of the higher mathematics should leave it unread; its perusal cannot fail to afford pleasure, as well as to widen comprehension of modern methods by their comparison with those they have succeeded, and by the intelligent survey of their growth in the past.

CHEMISTRY OF WHEAT, FLOUR, AND BREAD

Chemistry of Wheat, Flour, and Bread, and Technology of Bread-making. By William Jago. (Brighton: Published by the Author, 1886.)

THIS bulky volume professes to treat of its subjects in an exhaustive manner. Wheat, flour, and bread-making are as important as they are universal; and if they are common-place, their study requires deep insight into chemical science. Mr. Jago's book will form a valuable addition to economic science. The composition of wheats from all parts of the world, the minute structure of the grain, the composition of milling products, and the processes of panary fermentation, fall properly within the limits of such a work, and are dealt with in an exceedingly painstaking manner. The various methods of bread-making, the chemistry of the art, and the effects of blending different descriptions of wheat so as to secure the best possible results, are amply and ably discussed. Modern milling and baking appliances are also described carefully and illustrated graphically. There is likewise enough of the author's own thought and research to redeem the work from the stigma of being mere compilation. The book is decidedly useful, and, making due allowance for the progressive state of our knowledge upon many of the topics dealt with, it will probably be received as a standard work. It brings within its ample limits a vast amount of information which has usually, and we think appropriately, been treated of by separate authorities. The book is, in fact, a sort of encyclopædia of bread-making, and this being the case, it is open to the faults of such works. The design or scope is too large, and the matter introduced to our notice is often too remotely relevant to the immediate wants of the reader. A definition of chemistry, a table of atomic weights, an explanation of chemical equations, atoms and molecules, are scarcely necessary in this connection. Similarly, we cannot approve of lessons upon polarisation of light, the uses of the microscope, and the *camera lucida* being introduced *in extenso* into a book specially treating of a technical subject like this. Such knowledge ought to be assumed as already possessed by the reader; and as well might the author have given instruction upon the origin and uses of decimal fractions, or led up to his subject by several preliminary volumes dealing with the whole "circle of the sciences." Certainly he lays himself open to the charge of instructing either too much or too little. He deals with abstract scientific problems lying at the root of chemistry, and with the vulgar processes of the cook and the baker; and treats with equal facility of the microscope and the flour-mill. We had rather leave the minuter criticism of this voluminous work to the many experts whom it concerns, and who will no doubt be ready to detect any errors into which the indefatigable and talented author may have fallen. If Mr. Jago is ever tempted to bring out a second edition, we may recommend the use of the pruning-knife, which, if judiciously and freely used, will leave a better proportioned but less bulky treatise in our hands.

JOHN WRIGHTSON

OUR BOOK SHELF

American Journal of Mathematics, vol. viii. No. 3. (Baltimore, July 1886.)

In her note on "the binomial equation $x^p - 1 = 0$," Miss Scott gives a somewhat simpler form of the equation for quartisection than that given by Prof. Cayley (*L.Math. S.Proc.*, vol. xi. pp. 11-14), and works out the equation for quinquisection on apparently different lines from Mr. F. S. Carey's solution (cf. Prof. Cayley, *L.Math.S.Proc.*, vol. xii. and vol. xvi.). Mr. F. N. Cole furnishes "A Contribution to the Theory of the General Equation of the Sixth Degree," which is interesting from the historical details which he gives. He acknowledges his great indebtedness to Klein, but there is a good deal of original work in the note. Mr. J. C. Fields gives a neat "proof of the elliptic-function addition-theorem." The *pièce de résistance* of the number is, however, the long-looked-for notes of "Lectures on the Theory of Reciprocants," by Prof. Sylvester. These are designed as "a practical introduction to an enlarged theory of algebraical forms, and, as such, are not constructed with the rigorous adhesion to logical order which might be properly expected in a systematic treatise. The object of the lecturer was to rouse an interest in the subject, and in pursuit of this end he has not hesitated to state many results, by way of anticipation, which might, with stricter regard to method, have followed at a later point in the course." The lectures, which are ten in number, have been reported by Mr. J. Hammond. The subject, which, it will be remembered, was fully brought before our readers by Prof. Sylvester's inaugural lecture, printed *in extenso* in NATURE (vol. xxxiii. pp. 222-231), has already attracted many of our younger mathematicians, so that there is hope of the Professor's closing aspiration of creating "such a school of mathematics as might go some way at least to revive the old scientific renown of Oxford" being soon an accomplished fact.

The Non-Euclidian Geometry Vindicated: a Reply to Mr. Skey. (*Transactions* of Wellington Philosophical Society.)

A PAPER entitled "On the Simplest Continuous Manifoldness of Two Dimensions and of Finite Extent," by Mr. F. W. Frankland, was read before the London Mathematical Society, December 14, 1876 (*Proc.*, vol. viii. pp. 57-64), and was subsequently published in our columns (vol. xv. p. 515) This same paper, or one of similar character with identical title, appears to have given rise to a paper by Mr. Skey, which took the form of notes on Mr. Frankland's paper, and was read before the Wellington Philosophical Society on June 26, 1880 (published in the thirteenth volume of the *Transactions* of the New Zealand Institute). The pamphlet before us is an abridgment of a series of letters in which Mr. Frankland convincingly replies *seriatim* to the main points raised by Mr. Skey, and maintains his former ground by additional arguments.

LETTERS TO THE EDITOR

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

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

The Sense of Smell

I KNOW a person who has never been conscious of any odour from a bed of mignonette, and I know another person who has never been conscious of any odour from a bean-field. Both of these persons have the sense of smell acute and discriminating as regards other odours.

I know persons who cannot discover a difference between certain odours which are very different to ordinary persons. Then there are persons who are sickened by certain odours which usually give pleasure. A considerable number of persons seem to be altogether destitute of the sense of smell; and on the other hand there are a few who have the sense very strongly developed.

I am at present investigating peculiarities of this sense, and I shall feel obliged to any one who will furnish me with illustrations or examples—whatever their character may be—with such fullness and precision as will enable me to use them in a scientific inquiry.

ARTHUR MITCHELL

34, Drummond Place, Edinburgh, September 24

Palæolithic Implements in Cambridgeshire

FEW Palæolithic implements have, I believe, been discovered in Cambridgeshire, although they are abundant in the gravels of the neighbouring counties of Suffolk and Essex. It may therefore interest your readers to learn that three implements have lately been found near Kennet, on the surface of a field not far from the high-road from Newmarket to Thetford. Two of the implements are kite-shaped. One, of lustrous black flint, is acutely pointed, with sharp cutting edges, and has a part of the original crust of the flint left on one of the faces, which is less convex than the other. It has lost a portion of the butt-end, but is otherwise perfect. The third is a sharp-rimmed ovate implement, the surface of which is stained a deep ochreous colour. Portions of the original crust remain on the faces and base.

Two other implements of the pointed type have been found on the surface near Kennet, but are not in my possession.

In the winter of 1884-85 several implements and flakes were found in a ballast-pit opened by the Great Eastern Railway Company by the side of the line, about a mile and a quarter from Kennet Station, in the county of Suffolk. A reference to the Geological Map of the Ordnance Survey, Sheet 51, N.E., will show that the gravel in which this pit was opened is an extension of that which underlies the site of the above *surface* discovery. The implements from the ballast-pit which have come into my possession are of the kite-shaped and ovate types. Some are water-worn and abraded, in others the angles and edges are as sharp as if made yesterday.

ARTHUR G. WRIGHT

Sign-Numbers in Use among the Masai

AMONG the numerous tribes of Central Africa the Masai are distinguished by their use of finger-signs to denote numbers. These notorious warriors rarely ever use language to indicate numbers without accompanying signs on the fingers, though very frequently the latter are employed without the former, especially in answering questions.

As by some inadvertency I omitted giving a list of these signs in "Through Masai-Land," and Mr. Johnston, in his book "Kilimanjaro," has followed suit, it may still be of some interest and value to anthropologists to learn what these are.

English	Masai	Sign
1	Nabo	First finger held out alone
2	Aré	First and second fingers held out and alternately moved backwards and forwards
3	Uni	Thumb and two first fingers placed tip to tip
4	Unghwani	First and second fingers laid on top of each other
5	Umiët	Thumb placed between first and second fingers
6	Ilé	Thumb scratched over nail of second finger
7	Nabishana	No finger indication
8	Usiët	Hand held open and vertical and moved up and down
9	Naŭdo	Thumb and first finger form a circle by joining the tips
10	Tomon	First finger drawn over the nail of the thumb
11	Tomoni-obwo	Same sign as in 10 accompanied by that for 1. The same rule for the succeeding numbers
20	Tikitum	The hand closed and opened rapidly
21	Tikitum-o-nabo	The same as 20, but followed by the sign for 1
30	Othman	First finger held out and shaken by a circular movement of the wrist
40	Artum	The hand held open and vertical and shaken or agitated by a circular movement of the wrist; not up and down as in 8
50	Unum	Thumb placed between first and second fingers, and hand agitated as in 40
60	Tomoni-ilé	Nail of thumb scratched on nail of third finger
70	Tomoni-nabishana	No finger indication
80	Tomoni-usiët	Same as in 8, but sign never employed alone
90	Tomoni-naŭdo	Same as in 9, but words always employed along with sign
100	Ipé	The partially closed hand opened once or twice
200	Ipé-aré	

JOSEPH THOMSON

A GLACIAL PERIOD IN AUSTRALIA

A GREAT many theories have been put forward to explain the extensive glaciation which repeatedly covered Europe and North America with enormous ice-streams. The ingenuity displayed by those who dealt with the subject was well worthy of the importance which attaches to the solution of the problem. However plausible some of the theories propounded may be, still it seems premature to approach such a question until all the available evidence bearing on the subject has been brought together. The southern hemisphere has, up till very recently, revealed only a few, and not very important facts, regarding glaciation, and it is evident that glacier traces in that hemisphere must be of great importance to explain the cause of glaciation; whether we may suppose it to be cosmic or terrestrial. I think, therefore, that my discoveries of glacier traces in Australia may be of sufficient general scientific interest to warrant my giving a short account of them in this journal.

Dr. von Haast, in his excellent work on the "Geology of Canterbury and Westland (New Zealand)" gave a detailed account of the traces of an extensive glaciation in the Middle Island of New Zealand, together with a map, showing that at one time the glaciers on the western slopes of the Southern Alps in many places reached down to the sea, and that those which descended from their eastern flanks covered a large portion of the lowlands extending between the mountains and the coast-line.

During my exploration of the central part of the Southern Alps I observed numerous old moraines and *roches moutonnées* in the area which, according to von Haast's map, had once been covered by glaciers. Particularly was I struck with the freshness of the striæ, the scratches and grooves in the steep and rocky precipices on the sides of Milford Sound, that jewel of the Southern Alps.[1] Capt. Hutton, who examined some of the other sounds has not discovered any glacial traces there.[2]

Even now the glaciers in New Zealand reach down to 700 feet on the west, and to 2000 feet on the east side, which shows that New Zealand must be subject to a very different climate to that in similar latitudes—44°—in the northern hemisphere. Like Patagonia, New Zealand is at the present day to a certain extent *in a Glacial period*. The much greater extent of the prehistoric glaciers shows, however, that it is now by no means *at the height* of its glaciation.

Although a Glacial period was shown to have existed in New Zealand, there have not up to now been any definite statements regarding this subject in the mainland of Australia. The Rev. Tenison-Woods[3] examined certain rocks in the Blue Mountains, an insignificant table-land to the west of Sydney, and came to the conclusion that these, which had been supposed to indicate ice-action, did not do so, and that in fact there was no evidence of a Glacial period in the Blue Mountains. Mr. Howitt[4] came to a similar negative result regarding certain gravels and conglomerates, which according to others indicated glacial action. Griffiths,[5] on the other hand, claims these and other conglomerates of Omeo and Gippsland as evidences of a Glacial period in Australia.

Prof. Tate[6] described some striated rocks found near Adelaide, and Stirling[7] has shown that there exist extensive traces of glacier action in certain valleys near Omeo.

I myself have,[8] in several papers, published some of

[1] R. von Lendenfeld, "Der Tasmangletscher und seine Umgebung," Ergänzungsheft No. 75 von *Petermann's geographischen Mittheilungen*. "The Time of the Glacial Period in New Zealand," *Proceedings* of the Linnean Society of N.S.W. for 1885.

[2] F. M. Hutton, *Proceedings* of the Linnean Society of N.S.W. for 1885.

[3] *Proceedings* of the Linnean Society of N.S.W., vol vii. p 382.

[4] *Quarterly Journal* of the Geological Society of London, vol. xxxv. p. 35.

[5] "Evidences of a Glacial Epoch in Victoria," *Proceedings* of the Royal Society of Victoria for 1884.

[6] Tate, Anniversary Address, *Transactions* of the Royal Society of South Australia, 1879-80.

[7] T. Stirling, "Notes on some Evidences of Glaciation in the Australian Alps," *Proceedings* of the Royal Society of Victoria for 1885.

[8] R. von Lendenfeld, "Official Report on the Exploration of Mount Kosciusco to the Government of New South Wales" (Sydney, 1885.) "The Glacial Period in Australia," *Proceedings* of the Linnean Society of New South Wales for 1885. "Report on an Exploration of Mount Bogong," *Proceedings* of the Royal Geographical Society of Victoria for 1886.

the results of my two explorations of the Australian Alps, and described numerous indubitable traces of prehistoric glaciers on Mount Kosciusko and on Mount Bogong.[1]

Mr. Brown, Government Geologist of South Australia, has furnished me with photographs of beautifully preserved striæ on rocks in the Mount Lofty group near Adelaide. Prof. Hutton[2] has taken objection to the conclusions arrived at by myself, and although he acknowledges a *Glacier* period in Australia, objects to its having been a *Glacial* period. If I now revise the facts stated by others, and compare them with my own observations, I hope I may be able to give some idea as to the time and extent of the Glacial period in Australia. Before entering on the subject, however, I must give an outline of the physiography of the Australian Alps.

The greater part of Australia is destitute of high mountains; only in the south-eastern corner we meet with greater elevations. Here a true Alpine chain is situated. These Australian Alps consist of numerous parallel chains extending from south to north, which are curved in such a way as to advance with their convex sides eastward. The Australian Alps are very old; only Palæozoic formations participate in the folding which runs parallel to the extent of the ridges. The predominant rocks on the surface are gneiss-granite and Silurian, which appears generally in the facies of brown slate. Devonian limestones and slates are found; they are, however, not common and discordant to the underlying Silurian. The stratified rocks are highly folded, and usually with a very large dip or vertical. The Silurian appears on the surface in elongated islands or bands (compare the Government geological map of New South Wales), which extend parallel to the strike and to the main ranges. The coast-line follows precisely the same direction as the mountain-ranges, and the contour lines on the steep submarine precipice which extends down from it also run in the same direction. It appears that these mountains have been formed by a process of folding consequent on a horizontal pressure acting from west to east, and moving the folds in that direction away from a centre of depression *situated in the interior of Australia*. The steep submarine precipice by which the land sinks abruptly to a very great depth appears *as a more recent fault*.

Volcanic action participated in the formation of the Australian Alps, particularly in the vicinity of Mount Bogong. The volcanoes which formed the Bogong basalt plains, &c., seem to have been active during the early part of the Devonian. It appears probable that the upheaval of the Australian Alps—the folding—took place between the Silurian and Devonian or in the ealy Devonian. Hardly any formations later than the Palæozoic take part in the formation of the Australian Alps, and those which, like the Miocene in the valleys, have been observed, show a perfectly undisturbed horizontal stratification.

It will be seen from this that the Australian Alps are *very old*, much older not only than the European Alps and Himalayas, but older also than the New Zealand Alps. The effects of erosion are consequently much more matured there than in the other mountain systems mentioned, and consequently the appearance of these mountains is of particular interest. Only here and there rocks crop out on the summits of the hills or form steep precipices on their sides. Generally speaking, the mountain forms are very tame, and round. Mountaineering in Australia can generally be performed on horseback. The basements of high massive elevations only are left. High and sharp ridges weathered into series of grotesque rock pinnacles, the characteristic of the much younger Alps of Europe, have long since disappeared, and extensive undulating table-lands now mark the localities where once high peaks have stood. These table-lands are well defined and surrounded by steeper inclines, by which they descend to the surrounding lowlands. The Kosciusco group, from which the highest mountains in Australia arise, is a remarkable example of such a table-land, extending over an area of 160 square miles, with an average height of 5600-6000 feet.

The highest mountain in Australia, Mount Townsend, which I discovered to be the culminating point of Australia, is 7351 feet high.[1] There are several other peaks in the Kosciusco group over 7000 feet, particularly Mueller's Peak, 7266 feet, which was, up to the time of my expedition, considered the highest. Two peaks, which I have named Abbott's Peak and Mount Clarke, are over 7100 feet high.

In other parts of the Australian Alps the height of 7000 feet is nowhere reached. Next in importance to the Kosciusco group is the Bogong range, the highest point of which was ascended by me this year; it is 6508 feet above sea-level. Other peaks on the basaltic plateau south of Mount Bogong attain a height of 6000-6400 feet.

The whole of the Australian Alps consists of several high table-lands divided by very broken and hilly country from each other. The lowest levels on the table-lands are usually higher than the hill-tops in the adjacent country. The valleys are cut deep into the land. The main Alpine valleys have in their upper and middle portions an elevation of about 2000 feet. Steep gorges and waterfalls occur only on the margins of the table-lands. Terraces in the valleys themselves are not met with.

The Australian Alps reach to the sea. The whole of the south-eastern coast of Australia is hilly. Towards the west and north-west they dive under the Tertiary plains through which the Murray River wends its way. In the north they terminate on the left bank of the Yass and right bank of the Shoalhaven River. The mountainous country extends beyond this line to the north-eastern corner of Australia with a similar direction of the chains; but in this locality the geological structure changes. Extensive Triassic and Carboniferous formations take the place of the gneiss-granite and Silurian of the Australian Alps South-west the Alps may be considered to terminate near Melbourne.

The Australian Alps, from the Murray plain to the sea, have a width of about 120 miles on an average, and they are, from Melbourne to the Yass River, nearly 400 miles long. They are situated between 35° and 39° S. lat. and 145° and 150° E. long. Their latitude accordingly corresponds to that of the Sierra Nevada.

The Australian Alps exercise a vast influence on the climate and rainfall, in such a manner that, whilst the greater part of Australia south of the zone of tropical rains, suffers exceedingly from want of rain, the south-east corner—the Alpine part of Australia—has sufficient rainfall for the development of the country. To this climatic influence of the Australian Alps the great superiority of the colonies of New South Wales and Victoria over all the other Australian colonies must be ascribed.

At Kiandra, situated to the north of the Kosciusco group, the only meteorological station at a high elevation, there is a very heavy rainfall; from there it rapidly decreases as we advance westwards towards the interior, and also to the east, although not so much. It increases again on the east coast.

The Eucalyptus forests of the lowlands extend up to a height of about 5800 feet. Higher elevations are destitute of trees. On the upper margin of tree-growth the forest consists of very low and stunted trees belonging to the species *Eucalyptus pauciflora* and *E. Gunnii*. This Alpine forest resembles "Krummholz" of our Alps.

[1] A detailed account of the results of my explorations will be published in the Ergänzungshefte zu *Petermann's Geografischen Mittheilungen*.

[2] Hutton, "The Supposed Glacial Period in Australia," *Proceedings* of the Linnean Society of New South Wales for 1885.

[1] I made the ascent of Mueller's Peak, the height of which was trigonometrically measured by the Victorian Geodetical Survey (Mr. Black), and Mount Townsend on the same day, and the height given above was calculated from the aneroid readings on *both peaks*.

It snows in the Australian Alps very much in the winter, and the prevailing westerly winter winds pile up masses of wind-blown snow just below the high ridges on their eastern or lee side. These masses of snow never disappear altogether in summer, and we find eternal snow in the Australian Alps at an elevation of 6500 feet.

The excessively clear and bright Australian atmosphere affords no obstacle to the nocturnal irradiation of the day's heat, and so it freezes very frequently there at night, even in midsummer, down to 5000 feet. I experienced severe frosts on Mount Kosciusco in January 1885—January corresponds to our July—whilst it was intensely hot in the adjacent lowlands.

From these statements it is evident that we have in the Australian Alps a formidable mountain-range, which, although not glaciated now, would bear glaciers if the climate were slightly colder and more humid.

It seemed particularly surprising, therefore, that the older authors on Australian glaciation had given a verdict without examining the Alps. If no glacier traces were found in the lowlands, they yet might be found in the Alps; and if glacier traces were found in the lowlands, how much more extensive must they be in the mountains. Up the mountains I accordingly went to look for them. I undertook two expeditions. In 1885 I visited the Kosciusco group and ascended Mueller's Peak and Mount Townsend, and this year I explored the Bogong range and ascended the highest mountain in Victoria, Mount Bogong.

The Governments and learned Societies of New South Wales and Victoria greatly assisted me in my work by pecuniary aid and in other ways, and I am glad here to find an opportunity of expressing my gratitude for the great—I might say splendid—liberality with which the Australians have aided me. On my second journey I was accompanied by Mr. James Stirling, District Surveyor of Omeo, whose well-known essays on Australian glaciation have closely connected his name with the subject I had in view.

I was favoured with fine weather on both occasions, and on both occasions travelled through country never previously explored by any one with practical mountaineering experience. North of Mount Bogong I travelled for three days through country hitherto unknown. I found glacier traces on both occasions in great abundance, and in a sufficient state of preservation to be recognised as such without the shadow of a doubt. On the sides of the valleys of the tributaries to the Snowy River, which drains the eastern slopes of the Kosciusco plateau, I found abundant *roches moutonnées* at levels over 5800 feet, and high above the bottoms of the valleys. Also in some parts of the table-land itself such were found. With a little Alpine experience it is not difficult to discriminate between such ice-worn rocks and the ordinary bosses of weathered granite. These rocks are particularly well-defined in the Wilkinson Valley, the upper part of which is situated between Mount Townsend and Mueller's Peak. The bottom of the upper part of this valley is a broad and flat plain 6260 feet above sea-level. The hill-sides which surround it are everywhere worn down by glacial action up to about 800 feet above the valley bottom. The upper limit of ice-action is clearly marked, as in many valleys of the European Alps, and the thickness of the prehistoric ice stream thereby clearly indicated.

On the southern slopes of Mount Bogong, and also on the spurs of the northern flank of the mountain, basaltic erratics were found, which rocks could hardly have been transported to that locality without ice-action. In the valley of Mountain Creek, to the north of Mount Bogong, we discovered a large and well-preserved terminal moraine at an elevation of about 2800 feet, and some traces of others further down the valley.

The large moraine was carefully studied by Mr. Stirling and myself. Rocks of various kinds are scattered irregularly in it. It extends from one side of the broad valley to the other, and is cut through near the centre by the Mountain Creek. On the steep slopes towards the stream its composition of rocks brought down by an ice-stream can be easily recognised.

These two expeditions to the Australian Alps convinced me that at one time these mountains were glaciated, and the discovery of the moraine in Mountain Creek Valley, together with Stirling's (*l.c.*) elaborate researches in the Livingstone Valley, prove that the ice-streams of the Glacial period must have descended to pretty low levels. Down to 2000 feet glacial traces have been found in various parts of the Alps, and also in the Lofty Mountains near Adelaide. It is assumed by C. Wilkinson and other leading Australian geologists that a pluviatile period existed in the Miocene period, and it is obvious that such a period would probably be isochronous with the glaciation at high levels.

It is difficult to say whether the Australian and New Zealand glaciation was simultaneous, but that also appears probable. The better preservation of striæ, &c., in New Zealand is doubtless due to the greater hardness and resisting power to meteorological influences, of the ice-worn rocks in New Zealand than in the Australian Alps, where rapidly weathering granite is prevalent.

Whether this glaciation of Australasia was simultaneous with the last glaciation of Europe, or whether it was in time situated between the last glaciation and the last but one of the northern hemisphere, is not easy to decide. It appears nearly certain that it was *not later* than the last European Glacial period, and, as far as my opinion of the appearance of the traces it left behind goes, it was *earlier*. It may be hoped that future researches will show in a decisive manner whether it was simultaneous or earlier. If we do not consider merely local circumstances of sufficient effect to produce such a great change of climate as to cause so extensive a glaciation, we may, by arriving at the decision of the time of the Glacial period in Australia, also ascertain whether Glacial periods in the southern and northern hemispheres are *simultaneous or alternating*, which would give a clue to the difficult problem before us.

The necessary researches will doubtless be carried on with vigour by Australian men of science; and we may hope that their sagacity and perseverance may lead to the solution of the question, What is the cause of Glacial periods? R. VON LENDENFELD

ROOTS[1]

IT is a fact which has become more and more evident to the practical cultivator that the results of his efforts manifest themselves on the whole in a sort of compromise between the plant and its environment: I mean that although he sees more or less distinctly what his plant should be—according to a certain standard, however—it is but rarely, if ever, that the plant cultivated perfectly fulfils in every respect what is demanded of it. Of late years this has of course forced itself more prominently before the observer, because the facts and phenomena constituting what is termed variation have been so much more definitely described, and the questions arising out of them so much more clearly formulated.

Two points can be asserted without fear of contradiction: first, the plant itself is a variable organism; and, secondly, its environment varies. Now within limits which are somewhat wide, when closely examined, the experience of man leads him to neglect the variations occurring around him, and so no one quarrels with the statement that two individual geraniums belong to the same variety, or two oak trees to the same species,

[1] See NATURE, vol. xxxi. p. 183. A lecture by H. Marshall Ward, M.A., F.L.S., Fellow of Christ's College, Cambridge; Professor of Botany in the School of Forestry, Royal Indian College, Cooper's Hill.

although an accurate description of each of the two geraniums or of the two oaks might require very different wording.

It has also become more and more evident that although we cannot ascribe all variations to their causes—very often, indeed, we cannot even suggest causes for them—there are nevertheless numerous deviations from the normal, so to speak, exhibited by plants which can be distinctly referred to certain deviations from the normal on the part of their environment.

To illustrate this we may take the case of two plants of that very common weed, the Shepherd's Purse, growing at different ends of the same small plot of ground: the soil is sandy, and so much alike all over as to be regarded as the same everywhere, nevertheless the plant at one end is large, more than a foot high, and luxuriant, with many leaves and flowers, and eventually produces numerous seeds, whereas that at the other end is small, less than 4 inches high, and bears but a few stunted leaves and three or four poor flowers and fruits. The cause of the difference is found to be the different supply of water in the two cases, and if any one doubts that this may be so, let him try the experiment of growing two or more specimens of this weed in pots: the pots to be new, filled with soil which has been thoroughly mixed, and all the pots exposed to the same conditions—*i.e.* practically the same—except that those of one series are watered sufficiently often, and those of the other only just sufficient to keep the plant actually living. The experiment is easy and conclusive with such a weed as the above. Now, it is just such experiments as that above described—some of them equally simple, others less so—that the physiologist devotes much of his attention to, and in just such a manner has been gathered together a nucleus of information around which more knowledge can be grouped.

I may make these points clearer by again quoting an illustration, and, not to confuse or mislead you by going too far afield, I will keep to the same line of investigation, partly because it is quite as simple and conclusive as any other of many that might be selected, and partly because it may be possible to set before you some facts which are interesting or even new to you.

It has been found that in some cases where two plants are growing in the same soil and under the same conditions as above, but where one plant receives less water than the other, that the dwarfed drier plant is more hairy than the larger and luxuriantly growing plant, which has been well watered. On looking more closely into this matter it turns out that the extra hairiness is (in some cases at any rate) simply due to the fact that the hairs are closer together, because the little cells on the outer parts of the plant which grow out into hairs do not increase so much in length and superficial extent as those on the well-watered plant, and thus the hairs stand thicker together on the same superficial area of the organ—of a leaf, for instance. In other cases, however, the hairs are really increased in numbers and length—the plant is absolutely more hairy. It will be noticed that details concerning growth and turgidity, and of the influence of various minerals, and so on, are not under consideration here. I am not asserting that all cases of hairiness in plants are to be ascribed to this cause; but it does occur, as stated, and the point is a curious one in view of the fact that very many plants which grow in sandy dry soils are conspicuously hairy, whereas allied species growing in or near water, or even only in moister situations, are devoid of conspicuous hairs, or even quite smooth.

The above peculiarity is not confined to leaves and stems, moreover, for experiments with roots have shown that the root-hairs, which are so important in collecting moisture, &c., from the soil, can be made to appear in enormous numbers when the root is kept in a soil which is very open and only slightly moist, whereas none or very few are developed on the same roots growing in water: this again is in accordance generally with the fact that the roots of land-plants growing in light soils develop innumerable root-hairs, whereas those of water-plants do not thus increase their surface and points of attachment. I cannot here go into all the interesting facts known about these hairs, but it will be sufficient if you bear in mind the main points just mentioned.

Let us now vary the experiments a little. It is obvious that we might suppose any number of differences in the amount of water given to the plants used in the experiments described above; but it would be found, as matter of fact, that however little be the quantity of water given to the soil in which the dwarfed plant is, compared with that put into the soil in which the luxuriant plant grows, the actual weight of water will nevertheless have to be considerable, taking the whole life of the plant into consideration—there will be more used than you probably know, moreover, because the soil itself will no doubt condense and absorb some from the atmosphere during the night. There is a minimum of water absolutely necessary, and if the plant does not obtain this it will die. Its death will be ushered in by drooping and withering of the leaves, stem, and roots, and this condition, in which the functions of the plant are interfered with beyond a certain point, passes into a condition of disease.

Now take another case. We might so arrange the experiment that we poured and continued to pour too much water into the soil. Here again it would be found that a condition of disease eventually sets in—*i.e.* a condition in which the functions of the plant are again interfered with beyond a certain point. The symptoms and progress of the disease will be very different in the latter case, however, from those in the former. It may also be mentioned that in neither experiment is death inevitable if the disturbing cause is removed soon enough—*i.e.* if sufficient water be added in the first case before the cells have ceased to be able to take it up, or if the previous conditions of the soil are restored soon enough in the case of the over-watered plant.

Here we come to a matter which is less simple than may appear at first sight. You will note that the problem in the latter case is to restore the previous conditions of the roots and soil soon enough; I put it thus, because the conditions of the roots and soil may soon be very profoundly altered by the over-watering.

To understand this, it is necessary to become a little more fully acquainted with the condition of affairs in what may be called the normal case, where the soil is light and open, and plenty of water but not too much is at the disposal of the roots. Such a soil will consist of innumerable fine particles, of different shapes, sizes, and composition. No doubt there will be grains of quartz, particles of broken up vegetable matter, and little rugged bits of stones containing various minerals; each of these tiny fragments will be covered with a thin layer of water, and you would probably be greatly surprised if I were to go into the proofs showing how extremely tenacious of its water-blanket each particle is. It may be enough for our present purpose if you accept the fact that it requires enormous force to deprive the particles of the last traces of their water-layers; they will give off some—or in some cases even a good deal—rather easily, and in fact when the layers become of a certain thickness no more water can attach itself to the particles, but it falls away and the soil remains saturated, as we say.

Now these particles of soil, each enveloped in its water-blanket, are not in close contact; there are spaces between them, and these interspaces influence the quantity of water which can be held back by the soil.

Let us suppose such a soil perfectly dry; the particles above referred to being irregular in shape and size, and only roughly in contact at various points, the interspaces will be filled with air. If water be then added in some quantity, each of the particles becomes clothed with a layer of water, and some of the air is driven out, though bubbles of air will still exist in the larger interspaces.

A third case is conceivable—so much water might be supposed to find its way in, that no air remained in the interspaces between the particles of soil. Now it is true that such a state of affairs is not readily brought about in a normal soil; but I may indicate how the result is occasionally attained to a great extent. Suppose that a layer of clay or other impenetrable subsoil lies beneath the soil in question; then if water oozes into the soil in larger quantities than can be got rid of in the time, it is possible for nearly all the air to be displaced. Of course the object of good drainage is to prevent this; and it is often overlooked that drainage from below has the effect of drawing in air as well as of running off superfluous water—air is driven into the spaces as the water leaves them.

In speaking of the "bubbles of air" entangled in the interspaces between the particles of soil, each with its water-blanket, I have overlooked some details as to what the bubbles really are. As a matter of fact they will not remain of the same composition as ordinary air, and may soon differ considerably; besides the vapour of water, they may contain gases in quite different proportions from those in the air outside. In the type case, however, there will be some oxygen present in the bubbles.

It is not intended here to go very fully into a description of the structure of the roots of land-plants; enough if you are reminded how the smaller ramifications of a root are found to be more numerous and thinner as we approach the periphery of the mass of earth which they traverse. From the youngest rootlets are produced the root-hairs, in enormous quantities, new ones arising forwards—*i.e.* near the tip of the rootlet—as the rootlet grows on, and those behind dying off after fulfilling their functions. These functions are chiefly to apply themselves in the closest manner to the surfaces of the particles of soil, and in this way to place the water which they contain in direct continuity with the water which clings with such enormous force to the surfaces of the particles. Hence this water can pass from the soil to the plant, and anything dissolved in the water can also pass into the root-hair and thus up into the plant.

I am not going to dwell on how the root-hairs themselves aid in dissolving mineral substances—corroding the surfaces of the particles of soil they cling to—nor shall I trouble you with the details of what substances will be dissolved in the water; for, of course, you will see that anything soluble will pass into solution and may be carried into the plant.

The chief point to be insisted on just now is that this water in the soil will contain among other substances oxygen dissolved in it from the air-bubbles referred to above, and that this dissolved oxygen will pass into the root-hairs in solution together with the minerals and any other substances. This oxygen, moreover, is absolutely indispensable for the life of the root-hairs; it can be easily shown that if the supply of oxygen is stopped, or even diminished to any considerable extent, the roots begin to die, because the root-hairs cease to act.

Let us look a little more closely into this point. Each root-hair is a tiny cell containing living protoplasm and certain other substances, all inclosed in a thin, elastic, porous membrane. Now it has been abundantly proved that if such a cell is deprived of oxygen, its protoplasm becomes dormant for a time, and slowly breaks up, as it were; subsequently it becomes decomposed into other and simpler materials. A sort of internal combustion and fermentation take place, and these processes result in the formation and liberation of bodies like carbon-dioxide, alcohol, acetic acid, and other acid matters—substances in the main not only incapable of supporting the life of the root-hairs but actually destructive of it.

Evidently, then, if we deprive all the root-hairs of oxygen, they will eventually die. Their death will entail that of the rootlets and roots to which they belong, and this for two obvious reasons—first, it is the root-hairs and the root-hairs alone which can absorb the necessary water and substances in solution from the soil to supply such a plant as we are concerned with; and, secondly, the noxious products resulting from their death accumulate in the soil and diffuse into the root, and so hasten similar decompositions in what were hitherto healthy cells.

It must not be supposed that these disastrous consequences of the deprivation of oxygen always follow immediately. Not only are the roots of some trees, for instance, able to withstand ill-treatment longer than others, but, obviously, the kind and degree of ill-treatment may affect the problem of how long the plant shall survive. The number of rootlets and root-hairs, and the spread of the roots and other factors, will obviously affect the matter.

Suppose the following case as an example. A young tree is growing and flourishing in an open, good soil, and, for some reason or other, more soil is heaped about the roots until the depth is increased considerably: the deeper situation has placed obstacles in the way of the roots obtaining oxygen so readily as before. Not only are the roots further from the atmosphere, but the water carried down has to percolate through more soil, and may part with much of its oxygen (or even all) on the way: of course the nature of the soil, the presence of organic matters, and other circumstances decide this. It is not at all difficult to conceive of such a case where the supply of oxygen to the roots is thus diminished so far that the activity of the root-hairs as a whole is simply lowered, but not destroyed,—a stage or two further and they might become dormant, and their protoplasm undergo intramolecular respiration for a time, and break up. It is clear that the diminished activity of the roots will affect the supply of water (and the substances dissolved in it) to the leaves: this will obviously react on the thickness of the annual rings, and this again on future supplies—since the water passes up the alburnum or young outer layers of woody tissue. Moreover, a diminution of supplies from the leaves means less substance and power for replacing the root-hairs, and so on. In this way it may require some time to kill the tree, and all kinds of complications may arise meanwhile. This case is probably by no means uncommon.

A more extreme case is where the soil becomes damp and clogged with excessive moisture: not only does no oxygen reach the roots, but noxious gases accumulate in solution in the soil, and will hurry matters by poisoning cells which might otherwise live a longer life of usefulness. It is extremely probable that such gases find their way into higher parts of the plant in the air-bubbles known to exist and to undergo alterations of pressure in the vessels of the wood: this being so, they would slowly retard the action of other living cells, and so affect the upper parts of the plant even more rapidly than would otherwise be the case. Damp soil may thus do injury according to its depth and nature; but it need not necessarily be deep to be injurious if much oxygen-consuming substance is present. I have seen excellent soil converted into damp, stinking, deadly stuff from the action and accumulation of the larvæ of cockchafers: these "grubs" may, it is true, accelerate the devastation caused by the consumption of oxygen and the accumulation of poisonous waste matters in the soil by directly cutting off portions of the roots themselves, but the accumulation of oxygen-consuming substance, and the cutting off of supplies to the root-hairs evidently plays a chief part in the destruction.

There is another matter with regard to damp soils that cannot be left out of account. I have already told you that roots which are developed in water or in very damp sandy soil—and which are perfectly healthy—have few or no root-hairs formed on their surfaces; whereas it may be readily shown that the roots of the same plant growing in a well-aërated open soil, which is scarcely moist to all appearance, will be densely covered with a close-set pile of hairs. Indeed it is by means of the millions of root-hairs on its rootlets that a sunflower or a bean, for instance, obtains the enormous quantities of water necessary for its needs from soil which, to our rough perception, seems to be dry.

I cannot here go into all the proofs that such a soil is by no means so dry as it looks; but will simply remind you of what was said above as to the enormous force with which the minute particles of rock, &c., which form "soil" retain their hold on the thin films of water which constitute what have been termed their water-blankets. This is certain, that a healthy well-rooted plant can take up water from a soil which is to all appearance air-dry; whereas a plant which has not yet had time to develop its root-hairs in sufficient numbers to take these firmly adherent water-films, from numerous particles of soil, would droop and wither.

Of course it must be borne in mind that we are speaking of land-plants such as we commonly meet with on ordinary dry land: in the case of plants which flourish in bogs or in water there are corresponding differences in the structures of their roots agreeing with the differences of environment. Even such plants need air at their roots, and an excellent illustration of this is is afforded by some willows. Our common osier and other willows grow, as you are aware, in low-lying damp and even boggy places, often flooded: now, it has been found that, if young willows are planted too deep in the soil, they very soon send out new roots—adventitious roots they are often called—close to the surface of the soil, and these roots soon do all the work. There is no doubt that this power enables these willows to live in places that would be fatal to them otherwise; and the same is true of some other plants.

Enough has now been said to show you how necessary it is that some care should be exercised in watering plants, or in exposing them to conditions different from those to which they are accustomed; and, it need scarcely be added, apparently mysterious diseases may sometimes be explained when it is shown that such precautions have been neglected. Any one can quote instances of plants which will grow in some soils and not in others, but no very satisfactory reason is afforded by simply saying that the one soil is suitable and the other not: however, all I have attempted to show you is that some soils are not suitable for some plants because the plants in question need more air at the roots than these particular soils can afford them under the circumstances.

Many plants flourish in an open soil with plenty of sand in it, but will not grow in a stiff wet soil. This is not necessarily because the heavier soil does not contain the right food-materials, but because its particles are so small, so closely packed, and so retentive of moisture, that the root-hairs do not obtain sufficient oxygen: moreover, the very damp state of the soil does not favour the development of the numerous root-hairs necessary, as we have seen. Nor is this all,—though I cannot here enter at length into this point,—root-hairs and roots cannot grow or act unless the temperature is favourable, and we have plenty of evidence to show that a close wet soil may be too cold for the roots at a time when an open drier soil (exposed to similar conditions as regards sunshine, &c.) would be of a temperature favourable to their growth. Many a pot-plant receives an extra over-dose of water because it is drooping from the roots being too cold to act properly. The opening up of stiffer soils by means of the spade or plough, or by the addition of other kinds of soil, such as sand, burnt lime, &c., or by means of drainage of various kinds, is thus to be regarded as a means of letting in air and therefore oxygen to the roots. "Sweetening the soil" is an expression one hears used by planters and others: this is often no doubt their way of expressing the fact that the air thus let in does so much to turn the noxious substances which have accumulated into other substances which the root-hairs of the plant can take up with profit. The exposure of certain soils to sharp winter frosts in part benefits the plants subsequently grown in it, because air can make its way into the cracks produced as the particles crumble: there are other advantages also due to the "weathering" of soils, of course, as also to the addition of lime, &c., but I am purposely abstaining from referring to points concerning the nutrition of plants as generally understood.

Let me shortly call your attention to a few other practical applications of the knowledge briefly summed up above. It is well known that a good deal of experience has been brought to bear on the question of what trees are the best to plant in or near large towns: there are very many facts to be considered. It is not sufficient to find a tree which will accommodate itself to the possibilities of the annual rainfall, or a diminished supply of sunlight throughout the year, and so on; nor is the problem solved when a tree is found that will put up with traces of acid gases in the atmosphere, and, as may follow, the accumulation of acids in the soil, and consequent alterations in its chemical composition. In many cases trees have been found to die as they grew older because the pavement or asphalt over their spreading root-system prevented proper aëration and a proper supply of aërated water to their root-hairs: imagine the effect of a few days' hot summer sunshine on roots just beneath the pavement of an exposed street! It is true the cover may prevent rapid evaporation, but it also shelters the soil from the well-aërated rain-drops; moreover, such sheltered roots will at certain seasons grow up to the surface of the soil and in contact with the lower surface of the pavement. Then there is the question of drainage. If the water which does find its way in slowly accumulates and becomes stagnant, the results are as disastrous or even more so; yet it is obviously a difficult matter so to arrange things that the accumulated surplus water of certain seasons shall pass away below, acting like a suction-pump and drawing in air after it, and still fulfil the other requirements hinted at above. I leave out the question of exhaustion of the soil—the dead leaves, &c., being carefully removed. Can we wonder that there are so few trees to choose from that will stand such treatment? The fact that there are some only accords with what has been already stated—that plants vary in their requirements and powers; and no one doubts that the variations have been influenced by variations in the environment.

We have now seen to a certain extent how variations of a particular kind may affect a plant. The plant responds to a certain extent—it is, as some people say, "plastic"—but if the limits are reached and slightly overstepped, the variations on the part of the plant become dangerous to its existence, and the plant becomes diseased and may die.

Not to dwell upon hypothetical matters, I will content myself with saying, in conclusion, suppose a variety of a given plant grows in damp places and has roots which form few or no root-hairs, and suppose an individual of that plant to become transferred to a more open soil: I have shown you reasons for regarding it as probable that the latter individual might produce more root-hairs and thus adapt itself to the altered conditions. If such a case happened, it is by no means improbable, but the contrary, that other circumstances co-operating or adverse would decide certain problems of importance to the existence of that particular individual.

But the main object of this lecture has been to show you how very complex the conditions may be which bring about a "diseased" condition of the roots. It is no uncommon event to see a tree flourish for years and then slowly die off from "something at the roots": examination shows that the soil still contains the necessary foods, the water-supply is constant and good, the tree is exposed to no obvious adverse influences, and yet with steps so slow that they are scarcely noticeable, the tree begins to die off before its time. In some cases this is probably because the root-hairs are not receiving their proper supply of atmospheric oxygen, and this may be due to very slight changes in the *structure* (not the chemical composition) of the soil: a very slight diminution in the activity of the root-hairs may cause a diminution in the supply of water to the leaves at seasons when they require much, and this means lessening their supply of food-materials. If the leaves are placed on short commons, they cannot form wood, and so the next season's supply of nutritive solutions may be cut short; moreover, fewer root-hairs will be formed. No doubt differences will appear in different years or seasons; but if the tendency on the whole is in the above direction, the life of the tree is already limited—it may drag on for years as an object, which can scarcely be termed a tree however, but its doom is sealed.

The difficulty of placing one's hand on an exactly illustrative case is due to the fact that other causes are usually at work after a short time. I have purposely avoided any reference to the changes brought about in the chemical nature of a soil by the addition or cutting off of air, &c.; and for the same reason—to keep your attention directed to the root-hairs as living cells exposed to the influence of a definite environment—I have left out of account some questions of food-supply. These matters do not invalidate anything said above, but they do profoundly affect the problems of the diseases of plants, and especially those diseases which start from the roots.

ON THE PROPOSAL TO ESTABLISH A PERMANENT COLONIAL MUSEUM IN LONDON

THE proposal to continue the present Colonial and Indian Exhibition at South Kensington having met with a good deal of support, it is worth while to examine it on its merits; quite apart from the popular accessories of music, illuminations, &c., the continued existence of which depends upon altogether different considerations.

The first point for examination is whether such a permanent exhibition or museum would materially and usefully supplement or form a real addition to the existing public institutions of London, for upon the determination of this question the decision ought largely to depend.

On a general review of the vast collection of objects exhibited in the present Exhibition, they are seen to be mainly included under the four following categories:—

(1) Natural history objects, or specimens of the animal, vegetable, and mineral kingdoms of Nature.

(2) The raw products derived from them, and their economic applications.

(3) Art of every description, with which may be included objects bearing upon archæology and ethnology.

(4) Manufactures of all kinds.

(1) With reference to natural history, it can scarcely be a public desideratum to attempt to form a new museum of this kind when there exists, within a few hundred yards of the Exhibition, the finest collection in the world in the great national Museum of Natural History. There the animals, plants, fossils, and minerals not only of the British colonies, but of the whole known world, are exhibited with a fullness and in a manner that there could not be a possibility of in any way approaching.

(2) Then, as regards the economic uses of the vegetable kingdom at least—such as food-products, drugs, timbers, &c.—the nation possesses in the Museum of Kew Gardens a probably unrivalled public collection admirably exhibited. Many years of energy and a very large expenditure of time and money would fail to make up again such a collection as this has now become.

(3) Objects of art both ancient and modern form a very striking and important portion of the Exhibition. It is probable, however, that the best part of those which are not on loan have been sold or otherwise disposed of, and thus are not available for future exhibition. But with the South Kensington Museum at our doors, the initiation of a new art collection cannot be needed; whilst as for objects illustrative of ethnology and archæological specimens, they are, it is needless to say, magnificently displayed in the galleries of the old British Museum in Bloomsbury.

(4) There remains only the commercial products and manufactures of the colonies and India, and, so far as I am aware, there exists at present no general public collection of such articles. Here then, it appears to me, we have a reasonable basis for the formation of a permanent museum. A public collection of trade samples is a real want in London.

It appears, then, from the above observations, that no necessity exists for a new *general* museum of colonial and Indian productions, inasmuch as the public is already amply provided with other museums which illustrate fully nearly all the objects and articles proposed to be exhibited in the new one.

There is also good reason to think that the multiplication of museums is undesirable as well as unnecessary. We are not without experience of this, and the history of the late India Museum is quite to the point. The vast collections brought together by the Honourable East India Company were quite similar in kind to those it is now proposed to form, and illustrated very thoroughly the productions of India. But the Museum never attracted public interest or proved of much practical utility; many departments were neglected, the specimens badly conserved, and not available for consultation or study, and at last its condition having become somewhat of an official scandal, it was, six or seven years ago, broken up and dispersed. It bears strongly on the remarks above made that the collections had to be distributed among the very museums which I have there enumerated. No doubt additions of much value thus accrued to them; but there was also an immense mass of duplicate and damaged material, some of which at least was destroyed. After this experience it seems scarcely credible that a proposal to form again another general *Indian* Museum in London will be seriously entertained, whatever may be the case as regards the colonies. But in the latter, as in the former, it is almost certain that from similar causes a few years would witness the same history and a similar termination.

It is then, I believe, in a permanent museum of trade samples and of the commercial products of our colonies that a really useful outcome of the present Exhibition is to be sought. The precise scope and character of such a museum would of course require careful consideration; but there is a great and increasing want of some central emporium of a public character where authentic samples, accurately determined and labelled, can be readily inspected and examined by those interested in commercial pursuits. The collection might well be arranged geographically, and should be accompanied by maps, trade statistics, and other aids to inquiry. Under able management such a museum would be capable of rendering great service to the commerce of the Empire, and be the means of bringing into trade the numerous neglected products of the world. I may add, parenthetically, that it would also relieve the staffs of our chief scientific establishments of a good deal of work, involving often much sacrifice of time, which now falls upon them, though outside the scope of their duties.

The situation of such a museum should, however, be readily accessible to business men, and would be prefer ably in or close to the City rather than in the West End of London. HENRY TRIMEN

NOTES

THE International Geodetic Conference will assemble in Berlin on October 20. Its principal business will be to deliberate on the best method of executing the resolutions arrived at at Rome and Washington in 1883 and 1884 respecting the actual measurement of a degree on the earth's surface, and likewise in reference to a scientific survey of the European continent. The adoption by all nations of Greenwich as the first meridian, in accordance with the decision taken at Washington, is to be strictly enforced in practice. The introduction of international normal time, on the other hand, has had to be postponed, owing to insuperable practical difficulties connected with ordinary business life. In order to promote the project of any international survey of the entire globe, it is proposed to establish a Central Geodetic Office in Berlin.

THE Association for the Improvement of Geometrical Teaching has revised its "Syllabus of Elementary Geometrical Conics," and is about to publish the same, with three figures lettered in accordance with the enunciations of the Syllabus. The work will be interleaved to allow of teachers and students supplying their own proofs, and will, it is hoped, appear early in November. Messrs. Swan Sonnenschein are the publishers.

THE Bombay Government has just issued a long resolution on the subject of technical education, which is one of special importance to India. The resolution lays down the outlines of the scheme which it favours under three heads—agriculture, art, and mechanical industries. It proposes that the College of Science at Poonah should be a central institution for the teaching of higher agriculture, and that local classes and schools should be established throughout the province under the supervision of district officers and of the Educational Department. The Jamsetjee Jeejeebhoy School of Art in Bombay is to be the centre of Government efforts for the purpose of art teaching, and a report is called for as to the propriety of obtaining additional teaching. The question whether a technological institute for mechanical industries should be established is discussed at some length, and the Government expresses the opinion that the time for doing so has not yet come. Meanwhile, it is suggested that the Committee of the Ripon Memorial Fund should form itself into an a sociation for promoting technical education in Bombay city, the Government promising to give it the utmost possible aid. The main dependence of other parts of the province must be upon the high schools for elementary science, and upon such institutions as may be started by means of local efforts. The resolution concludes by saying that the scheme is not academic, but that it is meant to enhance the well-being of the people at large by giving increased employment to labour and capital, and by cementing harmonious relations between them.

THE International Congress of Orientalists was opened at Vienna on the 27th inst., under the presidency of the Archduke Rénier. This is the seventh Congress of this body, the previous ones having been held at Paris in 1873, at London in 1874, St. Petersburg in 1876, Florence in 1878, Berlin in 1881, and Leyden in 1883. The Austrian Minister of Public Instruction welcomed the members, of whom there were about 300, in the name of the Government.

DR. SCHWEINFURTH has, in the interests of science, addressed to all Europeans, especially physicians, residing in Egypt, an inquiry as to whether, so far as they are aware, families of Northern origin settling in Egypt do, or do not, die out within three generations, or whether the race is capable of being perpetuated beyond that limit.

WE are requested to announce that the seventh annual Cryptogamic and Botanical Meeting of the Essex Field Club will be held on Friday and Saturday, October 15 and 16, in Epping Forest, the head-quarters for the day being at Buckhurst Hill. A large number of well-known botanists have promised to take part in the meeting, and the naming and arrangement of the specimens collected will be in the hands of Dr. Cooke, Rev. Canon Du Port, Dr. Wharton, Mr. Worthington Smith, and other fungologists. Botanists and others desirous of attending should communicate with the Hon. Secretary, Buckhurst Hill, Essex.

THE U.S. Hydrographic Office has received the following note:—"August 31, at 9.45 p.m., the steamer *City of Palatka*, Capt. Vögel, when a mile and a half north of Martin's industry lightship (off the coast, south of Charleston), in eight fathoms and a half of water, experienced a terrible rumbling sensation, lasting a minute and a half. There was quite a heavy sea from the south-east after leaving Charleston Bar at 5.30 p.m. When this rumbling sensation took place the wave-motion ceased. It was a perfect calm during the rumbling; after that the usual motion of the south-east swell took place. The wind at the time was south-west, light, weather cloudy, barometer 30·1, thermometer 80°. The sensation resembled a ship scraping a pebbly bottom, and the vibration of the ship was very great."

H.R.H. THE PRINCE OF WALES has decided that the Colonial and Indian Exhibition shall close on the evening of Wednesday, November 10.

WE hear that the first of the Grocers' "Medical Research Scholarships" has been awarded to Dr. Sims Woodhead, of Edinburgh. The value of the award is 250*l*.

"PHILIPS' Planisphere, showing the Principal Stars visible for every Hour in the Year" (London: George Philip and Son), is perhaps the best means yet devised of getting a preliminary acquaintance with the aspect of the sky. It consists of a movable disk representing the celestial sphere, and a fixed horizon corresponding to the latitude of London. On the edge of the disk are inscribed the signs of the zodiac, the months and days of the year; on the horizon, the hours of the day and night. By merely rotating the disk until any given day and hour are brought to coincide, the stars above the visible horizon of London at that time come into view. A continuance of the movement from east to west exhibits ten apparent revolutions of the stars. Each successive group on the chart rises and sets in its proper order, while its distance from the sun at any selected date can be estimated by following a line drawn from the celestial pole to the corresponding section of the disk. Its point of intersection with the ecliptic indicates the position of the sun. The same lines show the differences between solar and sidereal time throughout the year. A very little attention will enable the student to distinguish the circumpolar stars, to track the course of the Milky Way among the constellations, and to acquire some rough notion of the magnitudes of the principal stars. Quite a little stock of uranographical information, in short, is concentrated in this ingenious toy.

A CAREFUL revision of the hydrographic map of the Lake of Geneva has been lately made by M. Hornlimann, soundings being taken by the steel-wire method. It is shown that between Lutry, Ouchy, Evian, and La Tour-ronde the bottom of the lake is absolutely horizontal. For distances of 2 kilometres and more the differences of depth did not exceed 0·10 to 0·15 m. (being thus quite within the limits of observational error. The point of greatest depth was met with in the line which joins the mouth of the Flon, below Lausanne, and the church of Evian, 7 km. from the Swiss side and 5 from that of Savoy. This was 310 m. (say 1034 feet). The bottom of the lake is here about 219 feet above the sea.

THE earthquakes still continue in North America. Fresh shocks were felt at Charleston and other places in the south at five o'clock on the afternoons of the 27th and 28th inst. Shocks of earthquake were also at the same hour distinctly felt in Columbia, Augusta, and Savannah.

A SHARP shock of earthquake occurred at Constantinople at half-past four on the morning of the 26th inst., but no damage was done. At about a quarter to five on the same morning two sharp shocks in rapid succession were felt in Smyrna and the neighbourhood.

AN earthquake was felt at Aumale on the 22nd inst. at 11 a.m.; four shocks were reported.

THE White Island volcano, in the Bay of Plenty, off the North Island coast, New Zealand, is in active eruption, and sending forth a vast column of flame and smoke, rising to a height of 100 feet.

THE Ceylon branch of the Royal Asiatic Society has decided to print *in extenso* a translation of Prof. Virchow's monograph on the Veddas. An abridgment will appear in the forthcoming number of the Society's *Proceedings*.

FROM the Cambridge University Press the following new publications are announced:—"A History of the Theory of Elasticity and of the Strength of Materials, from Galilei to the Present Time," vol. i. "Galilei to Saint-Venant, 1639–1850," by the late I. Todhunter, D.Sc., F.R.S., edited and completed by Karl Pearson, M.A. "Lectures on the Physiology of Plants," by S. H. Vines, M.A., D.Sc., Fellow of Christ's College. "Travels in Northern Arabia in 1876 and 1877," by Charles M. Doughty, of Gonville and Caius College (with illustrations). "The Scientific Papers of the late Prof. J. Clerk Maxwell," edited by W. D. Niven, M.A.

MESSRS. CROSBY LOCKWOOD AND CO. announce the following books for the forthcoming season:—"Modern Engines and Boilers: Marine, Locomotive, and Stationary," by Walter S. Hutton, Civil and Mechanical Engineer (with upwards of 300 illustrations). "The Works' Manager's Hand-book of Modern Rules, Tables, and Data, for Civil and Mechanical Engineers, &c.," by Walter S. Hutton (third edition). "The Portable Engine, in Theory and Practice," by W. D. Wansbrough (with numerous illustrations). "Expansion of Structures by Heat," by John Keily, C.E., late Indian Public Works and Victorian Railway Departments. "Safe Railway Working," by Clement E. Stretton, C.E. "Drainage of Lands, Towns, and Buildings," a practical treatise, being an abridgment of the works of the late G. D. Dempsey, C.E., with extensive additions by D. Kinnear Clark, M.Inst.C.E. "Trusses of Wood and Iron: Practical Applications of Science in determining the Stresses, Breaking Weights, Safe Loads, Scantlings, and details of Construction," by William Griffiths. "Shoring and its Application," a manual for students, by George H. Blagrove (with numerous illustrations).

H. K. LEWIS has in preparation "An Introduction to Practical Bacteriology," by Edgar M. Crookshank, M.B. Lond., F.R.M.S., Demonstrator of Physiology, King's College, London (2nd edition); also, by the same author, "Photographs of Bacteria: an Investigation into the Value of Photography for delineating Preparations of Bacteria" (illustrated with 50 permanent autotypes and numerous wood engravings).

THE following publications are announced by Messrs. W. and R. Chambers:—"Natural History: its Rise and Progress in Britain, as developed in the Life and Labours of Leading Naturalists," by Prof. H. Alleyne Nicholson (Aberdeen). This will form vol. i. of a series called "Chambers's British Science Biographies," of which series the second volume, by Prof. Lapworth (Birmingham), will cover the field of British Geology. Other new books by the same publishers are: "Recent Travel and Adventure," with illustrations; and "Lessons in Elementary Dynamics," by H. G. Madan, M.A., Science Master in Eton College.

THE grease of sheep's wool, a substance hardly utilised hitherto, may now find use, according to a process lately brought before the French National Society of Agriculture by M. Rohart. He finds that, brought to its point of fusion, it very readily absorbs certain sulphur-compounds; thus it will fix as much as 100 times its volume of sulphuretted hydrogen; and so treated it becomes saponifiable in the cold state. M. Rohart presented some excellent soap made from the grease. The operation required takes less than an hour, whereas soaps with a base of soda generally take 6 to 8 hours in their production. Moreover, the saponification can be obtained completely without caustic alkalies, and simply with alkaline carbonates; a new scientific fact, applicable to all fatty matters when sulphurised. Thus a great economy is possible. This sulphurised soap is recommended by M. Rohart, *inter alia*, for use in vine-cultivation.

IN a recent thesis on the modification of plants by climate, Mr. Crozier, of Michigan University, considers it established "that as plants move from the locality of their largest development towards their northern limit of growth, they become dwarfed in habit, are rendered more fruitful, and all parts become more highly coloured, Their comparative leaf surface is often increased. their form modified, and their composition changed. Their period of growth is also shortened, and they are enabled to develop at a lower temperature."

THE successful cultivation, since 1884, of the Ramie or China grass plant (*Boehmeria nivea*) on the Champ-de-l'Air at Lausanne (altitude 520 m.), by Prof. Schnetzler, is an interesting fact in botany. This shrub, a native of China and Sumatra, has been grown in the south of the United States and of France for thirty years. Recently it has been introduced into Algeria. There is of course a striking difference in the conditions of temperature between Lausanne and the places in Asia where Ramie is grown. While the latitude of the latter is from 15° to 35°, that of Lausanne is 46° 31′. The mean temperature at Lausanne is 9°·5 C. Last winter the plants underwent long periods of great cold; in one case, *e.g.*, the thermometer being below zero for 124 hours, with a minimum on the ground of – 12°·5 C.

THE question of telephony *v.* telegraphy has been recently discussed by a well-known German electrician, Dr. Wietlisbach. The chief hindrance to the use of the telephone for long distances is, he points out, of a financial, not of a technical, nature. A telephone-line 2000 km. long costs considerably over a million marks. It is still possible to speak very well this distance; but even supposing the line were in constant use day and night, the receipts must be 5 marks (say shillings) a minute to make it pay. In telephone work, however, the line is in use only a few hours daily; hence a short conversation would cost at least 50 marks (2*l.* 10*s.*). That is, of course, too dear for ordinary traffic. The telegraph works, with almost the same speed, more than ten times more cheaply. Thus the question as to rivalry between telephone and telegraph finds its settlement. The telephone, up to about 500 km. distance (say 310 miles), will more and more displace the telegraph, and find an extension which the telegraph would never reach. But for greater distances the telegraph must keep the upper hand. Thus telephone and telegraph are really not rivals, but fitted to supplement each other.

WE have received the report for the past year of the School of Mines, Ballaarat, an institution which its Council believe is in a fair way of becoming the leading School of Science in the colony of Victoria. The increasing number of the students who avail themselves of the constantly extending opportunities for instruction offered by the School renders additional teaching power a necessity, and this requires, first of all, an increased income. It is to be hoped that the Council have been successful in its request for double the present annual subsidy from the Government. A School of Mines is perhaps the most immediately useful and paying one a young community can have. A new and enlarged museum has been added to the School, and Mr. Oddie, the Vice-President, has undertaken at his own expense to erect and equip an astronomical observatory. Two rooms, each 16 feet by 18, were erected when the report was drafted, and in one of these a 12½-inch Newtonian reflector has been placed in position. The second room is to be utilised for spectrum analysis, solar physics, testing specula, &c. A system of meteorological observations with the latest instruments, in connection with the Melbourne Observatory, has also been introduced. A recent task of the School authorities, in which many of our readers may be presumed to be interested, is the collection of rocks and minerals representing the geology of Western Victoria in the Colonial and Indian Exhibition. At the close of the Exhibition it will be presented to the Museum of the Geological Survey of Great Britain. The reports of the individual professors show progress in almost every direction—in the number of students, of subjects taught, and of average attendances of each student. We observe that the benefits of the School are largely extended by means of a concession from the Government railways permitting students to travel over long distances at exceedingly low fares. This is one of those concessions which cost so little, yet are worth so much, and which are more common in the United States or the colonies than they are in England.

IN a very interesting paper contributed to the *Bulletin* of the Essex Institute of Salem, Mr. A. McFarland Davis writes on some of the games of the Indian tribes of North America. Several of these are described at considerable length, mostly from the early Jesuit records. Lacrosse is the first and most important of these; it was, as it is now, purely a game of skill, but it was a contest of grave importance, not a mere pastime, and was domesticated over a wide extent of territory. Another very widely-spread game was "platter," which was played with dice, and was wholly a game of chance; the third was a game of chance and skill combined, and in some of its forms was exceedingly complicated. It was called "straws," because a bundle of straws was divided, the game turning on the odd or even numbers in the heaps. It resembles the celebrated Chinese game of *fantan*, which forms one of the principal sources of revenue of one European colony in the East. Sundry other games not so widely spread as these are also described by Mr. Davis. The extraordinary importance attached to these games, the strange and solemn ceremonies with which they were frequently initiated, give them an interest in the eyes of anthropologists beyond that of mere curiosity.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mrs. Faulkner; two Golden Eagles (*Aquila chrysaetos*) from the Isle of Mull, Argyllshire, Scotland, presented by His Grace the Duke of Argyll, K.G., F.Z.S.; a Stock Dove (*Columba œnas*), British, presented by Mr. Charles Whymper, F.Z.S.; an Anaconda (*Eunectes murinus*) from South America, deposited; a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, purchased; a Maned Goose (*Bernicla jubata*) from Australia, received in exchange; a Spotted Hyæna (*Hyæna crocuta*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

STELLAR PHOTOMETRY.—Mr. Chandler, of Cambridge, U.S., presented an interesting and important paper to the Section of Mathematics and Astronomy of the American Association at the recent meeting, the title being "A Comparative Estimate of Methods and Results in Stellar Photometry." According to the account of the paper given in *Science* (vol. viii., No. 187), Mr. Chandler took for his text the general statement that instrumental photometry had thus far proved a failure; that is, it had not developed a more uniform scale than Argelander's, nor had the accuracy of individual determinations been increased, but they were, on the contrary, far more uncertain than the old differential naked-eye estimates. In support of his views Mr. Chandler showed that, for stars of Argelander's scale between magnitudes 2 and 6, the photometric catalogues of Seidel, Peirce, Wolf, Pickering, and Pritchard differed among themselves as much in their measures of what Argelander called a difference of one magnitude, as they did in their measures of his successive magnitudes. Their average values of the logarithm of the light-ratio for one of Argelander's magnitudes between 2 and 6, ranged between ·30 and ·38, about ·35 for the mean of all the above-mentioned catalogues. Between magnitudes 6 and 9 of Argelander's scale, the catalogues of Rosén and Ceraski averaged about ·35 for the light-ratio, while Pickering's late results with the meridian photometer gave (between magnitudes 6 and 8·5) ·48 instead of ·35 for this ratio. Coming to accidental errors, Mr. Chandler showed that, from a discussion of the naked-eye estimates of Gould, Sawyer, and himself, the probable error of a single estimate was a little over ± ·06 of a magnitude when the stars were at considerable distances from each other, and about ± ·05 of a magnitude when near; while the probable error of a single measure in the "Harvard Photometry" was ± ·17 of a magnitude, and in the "Uranometria Oxoniensis" about ± ·10 of a magnitude. The large residuals in the "Harvard Photometry" appear to arise, according to Mr. Chandler, from the wrong identification of stars in many cases, one instance being cited where no bright star exists in or near the place given in the observing-list, on account of a misprint in the *Durchmusterung*, and yet some neighbouring star was observed on several nights for it. The author, in conclusion, pointed out that we must obtain better results from photometers if we ever expect to use their results for the detection or measurement of variable stars, since several variables have been detected, and their periods and light-curves well determined by eye-estimates, whose whole range of variation is no greater than the whole range of error in the photometric observations upon a single star with the meridian photometer.

A NEW OBSERVATORY IN LA PLATA.—In the *Bulletin Astronomique*, tome iii. Août 1886, M. Mouchez gives an account of a new Observatory which is being built in the town of La Plata. The Observatory appears to have a remarkably good instrumental equipment, including a telescope of 0·80m. aperture, an "équatorial coudé" of 0·43m. aperture, a meridian instrument of 0·22m. aperture, an apparatus for celestial photography of the same dimensions as that of MM. Henry at the Paris Observatory, a Thollon spectroscope with objective of 0·25m. aperture, besides a collection of geodetical instruments. The new Observatory is under the direction of M. Beuf, lately an officer in the French Navy, and his first efforts are to be directed towards the carrying out of a geodetic survey of the vast territory of the province, including the measurement of an extensive meridian arc in the plains of Chaco and Patagonia. The measurement of this arc will supply a want which has been long felt by geodesists, and will give new and valuable data for an increase in our knowledge of the terrestrial spheroid. He trusts that M. Beuf will be successful in this arduous and important undertaking, and also that he will have sufficient energy, and be supplied with a sufficient staff of observers, to work to advantage the numerous and powerful instruments which the Observatory possesses.

HELIOMETRIC OBSERVATIONS OF THE PLEIADES.—In the note on this subject, printed in last week's "Astronomical Column," the words "since 1860" should read "since 1840," the latter being the date of Bessel's determinations resulting from his observations with the Königsberg heliometer made during the years 1829-41.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 OCTOBER 3-9

(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 October 3

Sun rises, 6h. 6m.; souths, 11h. 49m. 1·7s.; sets, 17h. 32m.; decl. on meridian, 4° 2′ S.: Sidereal Time at Sunset, 18h. 21m.

Moon (at First Quarter October 4) rises, 12h. 26m.; souths, 16h. 54m.; sets, 21h. 20m.; decl. on meridian, 18° 28′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	6 26	12 5	17 44	4 52 S.
Venus	4 34	10 55	17 16	3 23 N.
Mars	10 44	14 55	19 6	20 45 S.
Jupiter	6 30	12 8	17 46	4 54 S.
Saturn	22 43*	6 46	14 48	21 24 N.

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

Occultations of Stars by the Moon (visible at Greenwich)

Oct.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
6	B.A.C. 7097	6	22 33	23 42	142 312
8	e^2 Aquarii	5½	22 29	near approach	39 —

Oct.	h.	
9	17	Jupiter in conjunction with the Sun.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′			h. m.
Algol	3 0·8	40 31 N.	Oct.	4,	21 20 *m*
λ Tauri	3 54·4	12 10 N.	,,	6,	4 9 *m*
ζ Geminorum	6 57·4	20 44 N.	,,	4,	2 0 *M*
δ Libræ	14 54·9	8 4 S.	,,	5,	1 19 *m*
U Coronæ	15 13·6	32 4 N.	,,	5,	21 10 *m*
S Scorpii	16 10·9	22 37 S.	,,	9,	*M*
U Ophiuchi	17 10·8	1 20 N.	,,	3,	2 6 *m*
			and at intervals of		20 8
U Sagittarii	18 25·2	19 12 S.	Oct.	5,	0 0 *m*
			,,	8,	0 0 *M*
β Lyræ	18 45·9	33 14 N.	,,	5,	3 0 *m*
R Aquilæ	19 0·9	8 4 N.	,,	6,	*M*
δ Cephei	22 24·9	57 50 N.	,,	9,	0 0 *M*

M signifies maximum; *m* minimum.

Meteor Showers

The coming week is usually a somewhat less fruitful one for meteors than the one just past. The *Arietids*, October 7, R.A. 31°, Decl. 9° N., form the principal shower; a radiant in Musca, R.A. 46°, Decl. 26° N., and another near Polaris, R.A. 133°, Decl. 79° N., are also active at this time.

METEORITES, METEORS, AND SHOOTING-STARS[1]

YOU are kindly giving to me an hour to-night in which I may speak to you. I do not have enough confidence in myself to justify me in speaking to such an audience as this upon one of those broad subjects that belong equally to all Sections of the Association. The progress, the encouragements, and the difficulties in each field are best known to the workers in the field, and I should do you little good by trying to sum up and recount them. Let me rather err, then, if at all, by going to the opposite extreme.

Two years ago your distinguished President instructed and delighted us all by speaking of the pending problems of astronomy, what they are, and what hopes we have of solving them. To one subject in this one science, a subject so subordinate that he very properly gave it only brief notice, I ask your attention. I propose to state some propositions which we may believe to be probably true about the meteorites, the meteors, and the shooting-stars.

In trying to interest you in this subject, so remote from the studies of most of you, I rely upon your sense of the unity of all science, and at the same time upon the strong hold which these weird bodies have ever had upon the imaginations of men. In ancient times temples were built over the meteorite images that fell down from Jupiter, and divine worship was paid them; and in these later days a meteorite stone that fell last year in India became the object of daily anointings and other ceremonial worship. In the fearful imagery of the Apocalypse, the terrors are deepened by there falling "from heaven a great star burning as a torch," and by the stars of heaven falling "unto the earth as a fig tree casteth her unripe figs when she is shaken of a great wind." The "great red dragon having seven heads and ten horns, and upon his head seven diadems," is presented in the form of a huge fire-ball. "His tail draweth the third part of the stars of heaven, and did cast them to the earth." Records of these feared visitors, under the name of flying dragons, are found all through the pages of the monkish chroniclers of the Middle Ages. The Chinese appointed officers to record the passage of meteors and comets, for they were thought to have somewhat to say to the weal or woe of rulers and people.

By gaining in these later days a sure place in science, these bodies have lost their terrors; but so much of our knowledge about them is fragmentary, and there is still so much that is mysterious, that men have loved to speculate about their origin, their functions, and their relations to other bodies in the solar system. It has been easy, and quite common too, to make these bodies the cause of all kinds of things for which other causes could not be found.

They came from the moon; they came from the earth's volcanoes; they came from the sun; they came from Jupiter and the other planets; they came from some destroyed planet; they came from comets; they came from the nebulous mass from which the solar system has grown; they came from the fixed stars; they came from the depths of space.

They supply the sun with his radiant energy; they give the moon her accelerated motion; they break in pieces heavenly bodies; they threw up the mountains on the moon; they made large gifts to our geological strata; they cause the auroras; they give regular and irregular changes to our weather.

A comparative geology has been built up from the relations of the earth's rocks to the meteorites; a large list of new animal forms have been named from their concretions; and the possible origin of life in our planet has been credited to them.

They are satellites of the earth; they travel in streams, and in groups, and in isolated orbits about the sun; they travel in groups and singly through stellar spaces; it is they that reflect the zodiacal light; they constitute the tails of comets; the solar corona is due to them; the long coronal rays are meteor streams seen edgewise.

Nearly all of these ideas have been urged by men deservedly of the highest repute for good personal work in adding to human knowledge. In presence of this host of speculations it will not, I hope, be a useless waste of your time to inquire what we may reasonably believe to be probably true. And if I shall have no new hypotheses to give you, I offer as my excuse that nearly all possible ones have been already put forth This Association exists, it is true, for the advancement of science, but science may be advanced by rejecting bad hypotheses as well as by framing good ones.

I begin with a few propositions about which there is now practical unanimity among men of science. Such propositions need only be stated. The numbers that are to be given express quantities that are open to revision and moderate changes.

(1) The luminous meteor tracks are in the upper part of the earth's atmosphere. Few, if any, appear at a height greater than one hundred miles, and few are seen below a height of thirty miles from the earth's surface, except in rare cases where stones and irons fall to the ground. All these meteor tracks are caused by bodies which come into the air from without.

(2) The velocities of the meteors in the air are comparable with that of the earth in its orbit about the sun. It is not easy to determine the exact values of those velocities, yet they may be roughly stated as from fifty to two hundred and fifty times the velocity of sound in the air, or of a cannon-ball.

(3) It is a necessary consequence of these velocities that the meteors move about the sun and not about the earth as the controlling body.

(4) There are four comets related to four periodic star-showers that come on the dates April 20, August 10, November 14, and November 27. The meteoroids which have given us any one of these star-showers constitute a group, each individual of which

[1] Address to the American Association for the Advancement of Science, at Buffalo, August 18, 1886, by Prof. H. A. Newton, of New Haven, the retiring President of the Association.

moves in a path which is like that of the corresponding comet. The bodies are, however, now too far from one another to influence appreciably each other's motions.

(5) The ordinary shooting-stars in their appearance and phenomena do not differ essentially from the individuals in star-showers.

(6) The meteorites of different falls differ from one another in their chemical composition, in their mineral forms, and in their tenacity. Yet through all these differences they have peculiar common properties which distinguish them entirely from all terrestrial rocks.

(7) The most delicate researches have failed to detect any trace of organic life in meteorites.

These propositions have practically universal acceptance among scientific men. We go on to consider others which have been received with hesitation, or in some cases have been denied.

With a great degree of confidence, we may believe that shooting-stars are solid bodies. As we see them they are discrete bodies, separated even in prolific star-showers by large distances one from another. We see them penetrate the air many miles, that is, many hundred times their own diameters at the very least. They are sometimes seen to break in two. They are sometimes seen to glance in the air. There is good reason to believe that they glance before they become visible.

Now these are not the phenomena which may be reasonably expected from a mass of gas. In the first place a spherical mass of matter at the earth's distance from the sun, under no constraint, and having no expansive or cohesive power of its own, must exceed in density air at one-sixth of a millimetre pressure (a density often obtained in the ordinary air-pump), or else the sun by his unequal attraction for its parts will scatter it. Can we conceive that a small mass of gas with no external restraint to resist its elastic form, can maintain so great a density?

But suppose that such a mass does exist, and that its largest and smallest dimensions are not greatly unequal; and suppose further that it impinges upon the air with a planetary velocity; could we possibly have as the visible result a shooting-star? When a solid meteorite comes into the air with a like velocity, its surface is burned or melted away. Iron masses and many of the stones have had burned into them those wonderful pittings or cupules which are well imitated, as M. Daubrée has shown, by the erosion of the interior of steel cannon by the continuous use of powder under high pressure. They are imitated also by the action of dynamite upon masses of steel near which the dynamite explodes. Such tremendous resistance that mass of gas would have to meet! The first effect would be to flatten the mass, for it is elastic; the next to scatter it, for there is no cohesion. We ought to see a flash instead of a long burning streak of light. The mass that causes the shooting-star can hardly be conceived of except as a solid body.

Again, we may reasonably believe that the bodies that cause the shooting-stars, the large fire-balls, and the stone-producing meteor, all belong to one class. They differ in kind of material, in density, in size. But from the faintest shooting-star to the largest stone-meteor, we pass by such small gradations that no clear dividing lines can separate them into classes. See wherein they are alike:—

(1) Each appears as a ball of fire traversing the apparent heavens, just as a single solid but glowing or burning mass would do.

(2) Each is seen in the same part of the atmosphere, and moves through its upper portion. The stones come to the ground, it is true, but the luminous portion of their paths generally ends high up in the air.

(3) Each has a velocity which implies an orbit about the sun.

(4) The members of each class have apparent motions which imply common relations to the horizon, to the ecliptic, and to the line of the earth's motion.

(5) A cloudy train is sometimes left along the track, both of the stone-meteor, and of the shooting-star.

(6) They have like varieties of colours, though in small meteors they are naturally less intense and are not so variously combined as in large ones.

In short, if the bodies that produce the various kinds of fire-balls had just the differences in size and material which we find in meteorites, all the differences in the appearances would be explained; while, on the other hand, a part of the likenesses that characterise the flights point to something common in the astronomical relations of the bodies that produce them.

This likeness of the several grades of luminous meteors has not been admitted by all scientific men. Especially it was not accepted by your late President, Prof. J. Lawrence Smith, who by his studies added so much to our knowledge of the meteorites. The only objection, however, so far as I know, that has been urged against the relationship of the meteorites and the star-shower meteors, and the only objection which I have been able to conceive of that has apparent force is the fact that no meteorites have been secured that are known to have come from the star-showers. This objection is plausible, and has been urged, both by mineralogists and astronomers, as a perfect reply to the argument for a common nature to all the meteors.

But what is its real strength? There have been in the last hundred years five or six star-showers of considerable intensity. The objection assumes that if the bodies then seen were like other meteors we should have reason to expect that among so many hundreds of millions of individual flights a large number of stones would have come to the ground and have been picked up.

Let us see how many such stones we ought to expect. A reasonable estimate of the total number of meteors in all of these five or six star-showers combined makes it about equal to the number of ordinary meteors which come into the air in six or eight months. Inasmuch as we can only estimate the numbers seen in some of the showers, let us suppose that the total number for all the star-showers was equal to one year's supply of ordinary meteors. Now the average annual number of stone-meteors of known date from which we have secured specimens has, during this hundred years, been about two and a half.

Let us assume, then, that the luminous meteors are all of like origin and astronomical nature; and further assume that the proportion of large ones, and of those fitted to come entirely through the air without destruction, is the same among the star-shower meteors as among the other meteors. With these two assumptions, a hundred years of experience would then lead us to expect two, or perhaps three, stone-falls from which we secure specimens during all the half-dozen star-showers put together. To ask for more than two or three is to demand of star-shower meteors more than other meteors give us. The failure to get these two or three may have resulted from chance, or from some peculiarity in the nature of the rocks of Biela's and Tempel's comets. It is very slender ground upon which to rest a denial of the common nature of objects that are so similar in appearance and behaviour as the large and small meteors.

It may be assumed, then, as reasonable that the shooting-stars and the stone-meteors, together with all the intermediate forms of fire-balls, are like phenomena. What we know about the one may with due caution be used to teach facts about the other. From the mineral and physical nature of the different meteorites, we may reason to the shooting-stars, and from facts established about the shooting-stars we may infer something about the origin and history of the meteorites. Thus it is reasonable to suppose that the shooting-stars are made up of such matter and such varieties of matter as are found in meteorites. On the other hand, since star-showers are surely related to comets, it is reasonable to look for some relation of the meteorites to the astronomical bodies and systems of which the comets form a part.

This common nature of the stone-meteor and the shooting-stars enables us to get some idea, indefinite but yet of great value, about the masses of the shooting-stars. Few meteoric stones weigh more than 100 lbs. The most productive stone-falls have furnished only a few hundred pounds each, though the irons are larger. Allowing for fragments not found, and for portions scattered in the air, such meteors may be regarded as weighing a ton, or it may be several tons, on entering the air. The explosion of such a meteor is heard a hundred miles around, shaking the air and the houses over the whole region like an earthquake. The size and brilliancy of the flame of the ordinary shooting-star is so much less than that of the stone-meteor that it is reasonable to regard the ordinary meteoroid as weighing pounds, or even ounces, rather than tons.

Determinations of mass have been made by measuring the light and computing the energy needed to produce the light. These are to be regarded as lower limits of size, because a large part of the energy of the meteors is changed into heat and motion of the air. The smaller meteors visible to the naked eye may be thought of without serious error as being of the size of gravel stones, allowing, however, not a little latitude to the meaning of the indefinite word "gravel."

These facts about the masses of shooting-stars have important consequences. The meteors, in the first place, are not the fuel of the sun. We can measure and compute within certain limits of error the radiant energy emitted by the sun. The meteoroids large enough to give shooting-stars visible to the naked eye are scattered very irregularly through the space which the earth traverses; but in the mean each is distant two or three hundred miles from its near neighbours. If these meteoroids supply the sun's radiant energy, a simple computation shows that the average shooting-star ought to have a mass enormously greater than is obtained from the most prolific stone-fall.

Moreover, if these meteoroids are the source of the solar heat, their direct effect upon the earth's heat by their impact upon our atmosphere ought also to be very great: whereas the November star-showers, in some of which a month's supply of meteoroids was received in a few hours, do not appear to have been followed by noticeable increase of heat in the air.

Again, the meteoroids do not cause the acceleration of the moon's mean motion. In various ways the meteors do shorten the month as measured by the day. By falling on the earth and on the moon they increase the masses of both, and so make the moon move faster. They check the moon's motion, and so, bringing it nearer to the earth, shorten the month. They load the earth with matter which has no momentum of rotation, and so lengthen the day. The amount of matter that must fall upon the earth in order to produce in all these ways the observed acceleration of the moon's motion, has been computed by Prof. Oppolzer. But his result would require for each meteoroid an enormous mass, one far too great to be accepted as possible.

Again, the supposed power of such small bodies,—bodies so scattered as these are, even in the densest streams,—to break up the comets or other heavenly bodies, and also their power, by intercepting the sun's rays, to affect our weather, must, in absence of direct proof to the contrary, be regarded as insignificant. So, too, their effect in producing geologic changes by adding to the earth's strata has, without doubt, been very much over-estimated. During a million of years, at the present rate of, say, 15,000,000 of meteors per day, there comes into the air about one shooting-star or meteor for each square foot of the earth's surface.

To assume a sufficient abundance of meteors in ages past to accomplish any of these purposes is, to say the least, to reason from hypothetical and not from known causes. The same may be said of the suggestion that the mountains of the moon are due to the impact of meteorites. Enormously large meteoroids in ages past must be arbitrarily assumed, and, in addition, a very peculiar plastic condition of the lunar substance, in order that the impact of a meteoroid can make in the moon depressions ten, or fifty, or a hundred miles in diameter, surrounded by abrupt mountain walls two, and three, and four miles high, and yet the mountain walls not sink down again.

The known visible meteors are not large enough nor numerous enough to do the various kinds of work which I have named. May we not assume that an enormous number of exceedingly small meteoroids are floating in space, are falling into the sun, are coming into our air, are swept up by the moon? May we not assume that some of these various results, which cannot be due to meteoroids large enough for us to see as they enter the air, may be due to this finer impalpable cosmic dust? Yes, we may make such an assumption. There exist, no doubt, multitudes of these minute particles travelling in space. But science asks not only for a true cause, but a sufficient cause. There must be enough of this matter to do the work assigned to it. At present we have no evidence that the total existing quantity of such fine material is very large. It is to be hoped that through the collection and examination of meteoric dust we may soon learn something about the amount which our earth receives. Until that shall be learned, we can reason only in general terms. So much matter coming into our atmosphere as these several hypotheses require would, without doubt, make its presence known to us in the appearance of our sunset skies and in a far greater deposit of meteoric dust than has ever yet been proven.

A meteoroid origin has been assigned to the light of the solar corona. It is not unreasonable to suppose that the amount of the meteoroid matter should increase toward the sun, and that the illumination of such matter would be much greater near the solar surface. But it is difficult to explain upon such an hypothesis the radial structure, the rifts, and the shape of the curved lines, that are marked features of the corona. These seem to be inconsistent with any conceivable arrangement of meteoroids in the vicinity of the sun. If the meteoroids are arranged at random, there should be a uniform shading away of light as we go from the sun. If the meteoroids are in streams along cometary orbits, all lines bounding the light and shade in the coronal light should evidently be projections of conic sections of which the sun's centre is the focus. There are curved lines in abundance in the coronal light, but, as figured by observers and in the photographs, they seem to be entirely unlike such projections of conic sections. Only by a violent treatment of the observations can the curves be made to represent such projections. They look as though they were due to forces at the sun's surface rather than at his centre. If those complicated lines have any meteoroid origin (which seems very unlikely), they suggest the phenomena of comets' tails rather than meteoroid streams or sporadic meteors. The hypothesis that the long rays of light which sometimes have been seen to extend several degrees from the sun at the time of the solar eclipse are meteor streams seen edgewise, seems possibly true, but not at all probable.

The observed life of the meteor is only a second, or at most a few seconds, except when a large one sends down stones to remain with us. What can we learn about its history and origin?

Near the beginning of this century, when small meteors were looked on as some form of electricity, the meteorites were very generally regarded as having been thrown out from the lunar volcanoes. But as the conviction gained place that the meteorites moved not about the earth, but about the sun, it was seen that the lunar volcanoes must have been very active to have sent out such an enormous number of stones as are needed, in order that we should so frequently encounter them. When it was further considered that there is no proof that lunar volcanoes are now active, and that when they were active they were more likely to have been open seas of lava, not well fitted to shoot out such masses, the idea of the lunar origin of the meteorites gradually lost ground.

But the unity of meteorites with shooting-stars, if true, increases a hundredfold the difficulty, and would require that the comets have the same origin with the meteorites. No one claims that the comets came from the moon.

That the meteorites came from the earth's volcanoes is still held by some men of science, particularly by the distinguished Astronomer-Royal for Ireland. The difficulties of the hypothesis are, however, exceedingly great. In the first place, the meteorites are not like terrestrial rocks. Some minerals in them are like minerals in the rocks. Some irons are like the Greenland terrestrial irons. But no rock in the earth has yet been found that would be mistaken for a meteorite of any one of the two or three hundred known stone-falls. The meteorites resemble the deep terrestrial rocks in some particulars, it is true, but the two are also thoroughly unlike.

The terrestrial volcanoes must also have been wonderfully active to have sent out such a multitude of meteorites as will explain the number of stone-falls which we know, and which we have good reason to believe, have occurred. The volcanoes must also have been wonderfully potent. The meteorites come to us with planetary velocities. In traversing the thin upper air they are burned and broken by the resisting medium. Long before they have gone through the tenth part of the atmosphere the meteorites usually are arrested and fall to the ground. If these bodies were sent out from the earth's volcanoes, they left the upper air with the same velocity with which they now return to it. What energy must have been given to the meteorite before it left the volcano, to make it traverse the whole of our atmosphere and go away from the earth with a planetary velocity. Is it reasonable to believe that volcanoes were ever so potent, or that the meteorites would have survived such a journey?

No one claims that the meteors of the star-showers, or their accompanying comet, came from the earth's volcanoes. To ascribe a terrestrial origin to meteorites is, then, to deny the relationship of the shooting-star and the stone-meteor. Every reason for their likeness is an argument against the terrestrial origin of the stones. To suppose that the meteors came from any planets that have atmospheres involves difficulties not unlike to, and equally serious with, those involved in the theory of a terrestrial origin.

The solar origin of meteorites has been seriously urged, and deserves a serious answer. The first difficulty which this hypothesis meets is that solid bodies should come from the hot sun.

Besides this, they must have passed without destruction through an atmosphere of immense thickness. Then there is a geometric difficulty. The meteorite shot out from the sun would travel, under the law of gravitation, nearly in a straight line out and back again into the sun. If in its course it enters the earth's atmosphere, its relative motion—that which we see—should be in a line parallel to the ecliptic, except as slightly modified by the earth's attraction. A large number of these meteors, that is, most if not all well-observed fire-balls, have certainly not travelled in such paths. These did not come from the sun.

It has been a favourite hypothesis that the meteorites came from some planet broken in pieces by an internal catastrophe. There is much which mineralogists can say in favour of such a view. The studies of M. Stanislas Meunier and others into the structure of meteorites have brought out many facts which make this hypothesis plausible. It requires, however, that the stone-meteor be not regarded as of the same nature as the star-shower meteor, for no one now seriously claims that the comets are fragments of a broken planet. The hypothesis of the existence of such a planet is itself arbitrary; and it is not easy to understand how any mass that has become collected by the action of gravity and of other known forces should, by internal forces, be broken in pieces, and these pieces sent asunder. The disruption of such a planet by internal forces, after it has by cooling lost largely its original energy, would be specially difficult to explain.

We cannot, then, look to the moon, nor to the earth, nor to the sun, nor to any of the large planets, nor to a broken planet, as the first home of the meteoroids, without seeing serious if not insuperable objections. But since some of them were in time past certainly connected with comets, and since we can draw no line separating shooting-stars from stone-meteors, it is most natural to assume that all of them are of a cometary origin. Are there any insuperable objections that have been urged against the hypothesis that all of the meteoroids are of like nature with the comets, that they are in fact fragments of comets, or it may be sometimes minute comets themselves? If such objections exist, they ought evidently to come mainly from the mineralogists, and from what they find in the internal structure of the meteorites. Astronomy has not as yet furnished any objections. It seems strange that comets break in pieces, but astronomers admit it, for it is an observed fact. It is strange that groups of these small bodies should run before and follow after comets along their paths, but astronomers admit it as fact in the case of at least four comets. Astronomically there would seem to be no more difficulty in giving such origin to the sporadic meteor, and to the large fire-ball, and to the stone-meteor, than there is in giving it to the meteor of the star-shower. If, then, the cometic origin of meteorites is inadmissible, the objections must come mainly from the nature and structure of the meteoric stones and irons. Can the comet in its life and history furnish the varied conditions and forces necessary to the manufacture or growth of these peculiar structures?

It is not necessary, in order to answer this question, to solve the thousand puzzling problems that can be raised about the origin and the behaviour of comets. Comets exist in our system, and have their own peculiar development, whatever be our theories about them. It will be enough for my present purpose to assume as probably true the usual hypothesis that they were first condensed from nebulous matter; that that matter may have been either the outer portions of the original solar nebula, or matter entirely independent of our system and scattered through space. In either case, the comet is generally supposed, and probably must be supposed, to have become aggregated far away from the sun. This aggregation was not into one large body, to be afterwards broken up by disruption or by solar action. The varieties of location of the cometic orbits seem inexplicable upon any such hypothesis. Separate centres of condensation are to be supposed, but they are not *a priori* unreasonable. This is the rule rather than the exception everywhere in Nature.

Assume, then, such a separate original condensation of the comet in the cold of space, and that the comet had a very small mass compared with the mass of the planets. Add to this the comet's subsequent known history, as we are seeing it in the heavens. Have we therein known forces and changes and conditions of such intensity and variety as the internal structure of the meteorites calls for? What that structure is, and, to some extent, what conditions must have existed at the time and place of its first formation, and during its subsequent transformations, mineralogists rather than astronomers must tell us. For a long time it was accepted without hesitation that these bodies required great heat for their first consolidation. Their resemblance to the earth's volcanic rocks was insisted on by mineralogists. Prof. J. Lawrence Smith, in 1855, asserted without reserve that "they have all been subject to a more or less prolonged igneous action corresponding to that of terrestrial volcanoes." Director Haidinger, in 1861, said, "With our present knowledge of natural laws, these characteristically crystalline formations could not possibly have come into existence except under the action of high temperature combined with powerful pressure." The likeness of these stones to the deeper igneous rocks of the earth, as shown by the experiments of M. Daubrée, strengthened this conviction. Mr. Sorby, in 1877, said, "It appears to me that the conditions under which meteorites were formed must have been such that the temperature was high enough to fuse stony masses into glass; that the particles could exist independently one of the other in an incandescent atmosphere subject to violent mechanical disturbances; that the force of gravitation was great enough to collect these fine particles together into solid masses, and that these were in such a situation that they could be metamorphosed, further broken up into fragments, and again collected together."

Now, if meteorites could come into being only in a heated place, then the body in which they were formed ought, it would seem, to have been a large one. But the comets, on the contrary, appear to have become aggregated in small masses. The idea that heat was essential to the production of these minerals was at first a natural one. All other known rock-formations are the result of processes that involve water or fire or metamorphism. All agree that the meteorites could not have been formed in the presence of water or free oxygen. What conclusion was more reasonable than that heat was present in the form of volcanic or of metamorphic action?

The more recent investigations of the meteorites and kindred stones, especially the discussions of the Greenland native irons and the rocks in which they were embedded, are leading mineralogists, if I am not mistaken, to modify their views. Great heat at the first consolidation of the meteoric matter is not considered so essential. In a late paper M. Daubrée says:—"It is extremely remarkable that, in spite of their great tendency to a perfectly distinct crystallisation, the silicate combinations which make up the meteorites are there only in the condition of very small crystals, all jumbled together as if they had not passed through fusion. If we may look about us for something analogous, we should say that, instead of calling to mind the long needles of ice which liquid water forms as it freezes, the fine-grained texture of meteorites resembles rather that of hoar-frost, and that of snow, which is due, as is known, to the immediate passage of the atmospheric vapour of water into the solid state." So Dr. Reusch, from the examination of the Scandinavian meteorites, concludes that "there is no need to assume volcanic and other processes taking place upon a large heavenly body formerly existing, but which has since gone to pieces."

The meteorites resemble the lavas and slags on the earth. These are formed in the absence of water, and with a limited supply of oxygen, and heat is present in the process. But is heat necessary? Some crystallisations do take place in the cold; some are direct changes from gaseous to solid forms. We cannot in the laboratory reproduce all the conditions of crystallisation in the cold of space. We cannot easily determine whether the mere absence of oxygen will not account fully for the slag-like character of the meteorite minerals. Wherever crystallisation can take place at all, if there is present silicon and magnesium and iron and nickel, with a limited supply of oxygen, there silicates ought to be expected in abundance, and the iron and nickel in their metallic form. Except for the heat, the process should be analogous to that of the reduction of iron in the Bessemer cupola, where the limited supply of oxygen combines with the carbon and leaves the iron free. The smallness of the comets, should not, then, be an objection to considering the meteoric stones and irons as pieces of comets. There is no necessity of assuming that they were parts of a large mass, in order to provide an intensely heated birth-place.

But although great heat was not needed at the first formation, there are many facts about these stones which imply that violent forces have in some way acted during the meteorites' history. The brecciated appearance of many specimens, the fact that the fragments in a breccia are themselves a finer breccia, the fractures, infiltrations, and apparent faultings seen in microscopic sections and by the naked eye—these all imply the action of force. M. Daubrée supposes that the union of oxygen and silicon furnishes sufficient heat for making these minerals. If this is possible, those transformations may have taken place in their first home. Dr. Reusch argues that the repeated heating and cooling of the comet, as it comes down to the sun and goes back again into the cold, is enough to account for all the peculiarities of structure of the meteorites. These two modes of action do not, however, exclude each other. Suppose, then, a mass containing silicon, magnesium, iron, nickel, a limited supply of oxygen, and small quantities of other elements, all in their primordial or nebulous state (whatever that may be), segregated somewhere in the cold of space. As the materials consolidate or crystallise, the oxygen is appropriated by the silicon and magnesium, and the iron and nickel are deposited in metallic form. Possibly the heat developed may, before it is radiated into space, modify and transform the substance. The final result is a rocky mass (or possibly several adjacent masses), which sooner or later is no doubt cooled down throughout to the temperature of space. This mass, in its travels, comes near to the sun. Powerful action is there exerted upon it. It is heated. How intense is that heat upon a cold rock, unprotected apparently by its thin atmosphere, it is not possible to say. We know that the sun's action is strong enough to develop that immense train, the comet's tail, that sometimes spans our heavens. It is broken in pieces. We have seen the portions go off from the sun, to come back, probably, as separate comets. Solid fragments are scattered from it to travel in their own independent orbits. What is the condition of the burnt and crackled surface of a cometic mass or fragment as it goes out from the sun again into the cold? What changes may not that surface undergo before it comes back again, to pass anew through the fiery ordeal? We have here forces that we know are acting. They are intense, and act under varied conditions. The stones subject to those forces can have a history full of all the scenes and actions required for the growth of such strange bodies as have come down to us. Some of our meteors, those of the star-showers, have certainly had that history. What good reason is there for saying that all of them may not have had the like birth-place and life?

The pieces which come into our air in any recurring star-shower belong to a group whose shape is only partly known. It is thin, for we traverse it in a short time. It is not a uniform ring, for it is not annual, except possibly the August sprinkle. How the sun's unequal attraction for the parts of a group acts as a dispersive force to draw it out into a stream, those most beautiful and most fruitful discussions of Signor Schiaparelli have shown. The groups that we meet are certainly in the shape of thin streams.

It has been assumed that the cometic fragments go continuously away from the parent mass, so as to form, in due time, a ring-like stream of varying density, but stretched along the entire elliptic orbit of the comet. The epochs of the Leonid star-showers in November, which have been coming at intervals of thirty-three years since the year 902, have led us to believe that this departure of the fragments from Tempel's comet (1866, I.) and the formation of the ring was a very slow process. The meteors which we met near 1866 were therefore thought to have left the comet many thousands of years ago. The extension of the group was presumed to go on in the future until, perhaps tens of thousands of years hence, the earth was to meet the stream every year. Whatever may be the case with Tempel's comet and its meteors, this slow development is not found to be true for the fragments of Biela's comet. It is quite certain that the meteors of the splendid displays of 1872 and 1885 left the immediate vicinity of that comet later than 1840, although at the time of those showers they had become separated two hundred millions of miles from the computed place of the comet. The process, then, has been an exceedingly rapid one, requiring, if continued at the same rate, only a small part of a millennium for the completion of an entire ring, if a ring is to be a future form of the group.

It may be thought reasonable in view of this fact about Biela's comet, established by the star-showers of 1872 and 1885, to revise our conception of the process of di integration of Tempel's comet also. The more brilliant of the star showers from this comet have always occurred very near the end of the thirty-three year period. Instead of there being a slow process which is ultimately to produce a ring along the orbit of the comet, it certainly seems more reasonable to suppose that the compact lines of meteors which we met in 1866, 1867, and 1868 left the comet at a recent date. A thousand years ago this shower occurred in the middle of October. By the precession of the equinoxes and the action of the planets, the shower has moved to the middle of November. One half of this motion is due to the precession, the other half to the perturbing action of the planets. Did the planets act upon the comet before the meteoroids left it, or upon the meteoroid stream? Until one has reduced the forces to numerical values, he may not give to this question a positive answer. But I strongly suspect that computations of the forces will show that the perturbations of Jupiter and Saturn upon that group of meteoroids hundreds of millions of miles in length,—perturbations strong enough to change the node of the orbit 15° along the ecliptic,—would not leave the group such a compact train as we found it in 1866. If this result is at all possible, it is because the total action is scattered over so many centuries. But it seems more probable that the fragments are parting more rapidly from the comet than we have assumed, and that, long before the complete ring is formed, the groups become so scattered that we do not recognise them, or else are turned away so as not to cross the earth's orbit.

Comets, by their strange behaviour and wondrous trains, have given to timid and superstitious men more apprehensions than have any other heavenly bodies. They have been the occasion of an immense amount of vague, and wild, and valueless speculation by men who knew a very little science. They have furnished a hundred as yet unanswered problems which have puzzled the wisest. A world without water, with a strange and variable envelope which takes the place of an atmosphere, a world that travels repeatedly out into the cold and back to the sun, and slowly goes to pieces in the repeated process, has conditions so strange to our experience, and so impossible to reproduce by experiment, that our physics cannot as yet explain it. But we may confidently look forward to the answer of many of these problems in the future. Of those strange bodies, the comets, we shall have far greater means of study than of any other bodies in the heavens. The comets alone give us specimens to handle and analyse. Comets may be studied, like the planets, by the use of the telescope, the polariscope, and the spectroscope. The utmost refinements of physical astronomy may be applied to both. But the cometary worlds will be also compelled, through these meteorite fragments,—with their included gases and peculiar minerals,—to give up some additional secrets of their own life, and of the physics of space, to the blowpipe, the microscope, the test-tube, and the crucible.

THE BRITISH ASSOCIATION

SECTION D—BIOLOGY

Initiation of a Discussion upon the Value of the "Type-system" in the Teaching of Botany, by Prof. Bayley Balfour.—The speaker remarked that within the last fifteen years there had been a complete revolution in the method of teaching botany and zoology. The old method was practical teaching based on classification. In fact, in the olden times it was taught by means of object-lessons, which were sporadically chosen. In that method the real significance of plant life was completely overlooked, and also the position of the plants in Nature and their relationship to the animal kingdom. The result was that they had naturalists bred who had a wide range of knowledge of plant forms, and able to recognise and name a great number of plants, but of the life-history and sequence of events they were in the dark. The knowledge was a wide but superficial one. The new system was the natural outcome of the progress of the science, and as more knowledge of the minuter forms were obtained, it became necessary to select individual forms to be made types for special study. Thus by degrees a system of teaching was introduced which consisted in the selection of a few characteristic forms, and those were thoroughly studied in their structural and physiological relationship. Thus accurate knowledge of a few types was obtained, and the work now, instead of being in the field, was transferred to the laboratory. That new method was greatly used at the present time, and promised to be more widely introduced by the publication of new text-books running along the lines of that teaching. The old system he did not think produced good results, but he thought that teaching from types, combined with a certain amount of old teaching, would be effective.

In the discussion which followed, Prof. Bower said that in the elementary schools it would be well to give first the classification of the higher plants, and then, if the students succeeded in that part, they might pass to the more strict laboratory learning. — Prof. Hartog condemned the use of the type-system with children under sixteen, and, referring to the college instruction, lamented that the study of botany should have to be regulated by the requirements of the medical students.—Dr. Trimen thought the type-system was apt to give the students a false impression of the vegetable kingdom. They were apt to think that those types covered the whole matter to be studied. It would be well if the system could be extended. As to the question of medical students, they certainly did not require a complete course of technical botany. The teaching of botany in some of the London schools was a mere farce.—Prof. Marshall Ward remarked that the type-system has done good service to education, and pointed out how necessary it is to obtain exact knowledge from the study of actual objects, and how valuable is the training due to their careful investigation. The types should be real, and not imaginary or badly-selected ones.—Dr. Shaw observed that it would be a great mistake to drop biology out of the curriculum of the medical student.—Prof. Hillhouse pointed out that the type-system gave the student the advantage of commencing with simplicity and working up to complexity. The system, to be successful, must be carefully arranged and the selection of types judicious.

Remarks on "Physiological Selection, an Additional Suggestion on the Origin of Species," by G. J. Romanes, F.R.S., by Henry Seebohm.—This was a criticism of the above paper, and was followed by a short discussion, the general conclusion arrived at being to the effect that the paper referred to does not contribute anything essentially new to the theory of Charles Darwin. In criticising this theory, Mr. Seebohm pointed out that its author not only demanded an impossible number of coincidences, but coincidences of such a character that, once granted, the additional coincidence of fertility *inter se* but sterility outside the family was almost, if not quite, an unnecessary incumbrance to it.

On the Morphology of the Mammalian Coracoid, by Prof. G. B. Howes.—The author seeks to show that the importance of a third centre of ossification of the mammalian coracoid has escaped attention; he claims that it is the representative of the true coracoid bar of the lower vertebrata, the coracoid process being held to answer to the epicoracoid plate of the monotreme. He further upholds the view that the mammalian shoulder-girdle has been derived from a primarily expanded sheet-like form.

Some Experiments upon the Acquisition of an Unpleasant Taste as a Means of Protecting Insects from their Enemies, by E. B. Poulton.—This paper dealt with experiments upon the acquisition of an unpleasant taste as a means of protecting insects from their enemies. The author remarked that Darwin thinking of the use of colour in animals, and deciding that it was of use in courtship, came across the bright colours of caterpillars, which were sexless. He directed Wallace's attention to the subject, and he ventured a prediction that the bright colours would be associated with an unpleasant taste or smell, so that lizards, &c., refused to eat them. Experiments proved that this was correct, but, on thinking the subject over, it seemed to the writer that some limitations were required. If an insect was distasteful to a lizard, the former would either be starved or would have to put up with an unpleasant taste. It might probably acquire a relish for what hitherto was disagreeable, and then the distasteful organisms being brilliant and conspicuous would be easily caught and exterminated. Mr. Poulton therefore determined to experiment upon them, believing that it would be found that protection by a disagreeable taste was not so complete as was supposed. He obtained lizards from Italy, but found that that was the case. They often refused an insect at first, and took it afterwards unless they were fed on other things which they liked better. It was found that the small lizards refused a large moth, such as the privet hawk, although entirely harmless and undoubtedly palatable. The larger lizards disposed of it at once, and the former were evidently afraid of it, from its size bearing some comparison to their own. Further, the brilliant black and red moth, the cinnabar, was eaten by the tree-frog, and a second specimen was eaten directly afterwards. It was quite clear that the frog did not dislike the taste, but the moths disagreed with the frog, and they were afterwards found floating in the aquarium. The moth of the buff tip, which was protected by resembling a piece of broken rotten wood, was evidently disliked by the lizards, although they ate it in the end. In some cases disagreeable insects were eaten with a relish by those particular animals, such as the larvæ of the common Crœsus found on birch. The protection was therefore less perfect than was supposed to be the case.

On the Germination of the Spores of "Phytophthora infestans," by Prof. Marshall Ward.—One of the objects of this communication was to bring before the meeting copies of some careful drawings of all the stages of germination. These were obtained by actually watching the development, escape, and germination of the zoospores from the "conidia," following all the phases in *one* individual. The curious effects of light and of abnormal conditions upon the development of the zoospores were also pointed out, and the author showed diagrams of other forms of germination obtained by interfering with the conditions. In the short discussion which followed Prof. Marshall Ward referred to some points in the development and escape of the zoospores of the Saprolegnia.

On the Flora of Ceylon, especially as affected by Climate, by Henry Trimen, M.B., F.L.S.—Attention was first called to the fact that the Island of Ceylon was practically known to Europeans only by its south-west part, being about one-fifth of the whole area, but including the chief European centres, the planting districts of the hills, and the railway system. The remainder of the country is thickly covered with jungle, thinly inhabited, and rarely visited by Europeans, save Government officials and sportsmen. This difference was shown to be due to climate, especially to rainfall. The distribution of the rain, so far as is shown by annual amount, was exhibited by a map, in which the great advantage to the south-west of the lofty forest-clad escarpment of the central mountain-mass of over 7000 feet was exhibited. The south-west monsoon wind commencing at the end of May deposits an immense quantity of rain here, especially in the neighbourhood of Adam's Peak. In the rest of the island this wind becomes dry, and the country is parched and arid until the arrival of the north-east monsoon, which commences in October. This wind brings rain to the whole island, and is the only rain which the dry districts get; in many places it all falls in a few weeks, when the country is completely under water, though parched with drought for the rest of the year. This is very different to the well-known south-west of Ceylon, where, save in February or March, a fortnight's drought is a very rare event. In some parts over 200 inches falls in the year. In these respects Ceylon is an epitome or continuation of the Southern Indian peninsula. The peculiarities of the flora were then gone through in some detail, taking first the low country of the wet districts up to 3000 feet—in which the number of introduced tropical plants was commented upon; then of the lower hills, the principal home of the planting enterprise and tea and coffee estates; and next of the higher or true mountain districts above 5000 feet. In the low country the forest has been much destroyed by the indolent and improvident native mode of cultivation called "chena," and but little virgin forest remains in this portion of Ceylon. From 3000 to 5000 feet the agent of destruction has been European planting, and the forest has almost wholly disappeared. Above 5000 feet, land is no longer sold by Government. Attention was specially called to the concentration of endemic species in this wet district—over 800, or nearly 30 per cent. of the whole flora—and to the strongly Malayan, as distinguished from Peninsular Indian, type of these and of the whole flora. There are no Alpine plants in the Ceylon hills; dense forest covers their summits, but a number of temperate genera are represented. This flora is entirely Indian in type, with no *genus* represented which is not also found in the Nilghiris, but the number of endemic *species* is very remarkable, only about 200 being common to both mountain-ranges. A few remarks were then made upon the naturally open grass lands, called "patanas," in the hills, and their peculiar vegetation. The flora of the great dry tracts of Ceylon was then considered. It is completely distinct from that already considered, being mainly the same as that of the Carnatic or Coromandel coast of India, with no Malayan admixture, and very few endemic species. The whole country is covered with forest, apparently primæval; but in reality much of it is secondary, and not more than 800 or 1000 years old, as is reported by native tradition, and evidenced by the vast remains of temples, tanks, and ancient buildings now overgrown with trees. Most of the timbers of importance in trade are obtained in these districts, and, owing to a very faulty forest conservancy, there is now but little first-class timber remaining, save in very remote places. The botanical characters of this forest, which is everywhere evergreen, were given; and the paper concluded with a few remarks on the coast flora, which is very uniform throughout the tropical belt of the world.

On "Humboldtia laurifolia" as a Myrmecophilous Plant, by Prof. Bower.—It had been found that there were considerable numbers of plants in tropical countries which were pre-eminently associated with ants. The Italian botanist Picari propounded a general view with regard to the subject that the association was mutually advantageous to the ants and to the plants. He found that the plants gave shelter to the ants, and in certain cases supplied them with food. No one would deny the statement that the relation was advantageous to the ants themselves, but the converse case was not so clear. In some cases it had been found that the ants served to protect the plants, and drove off other insects. Picari also pointed out that in certain cases the plants derived nutriment from the excreta of the ants, but whether that was the case was a view open to considerable discussion. He (Prof. Bower) had come to the conclusion that the ants derived all the benefit, and that there was no advantage to the plants. Not only were the ants provided with a capital lodging, but it might be fairly assumed that from the glands of the plants the insects derived food as well.

On the Artificial Production of a Gilded Appearance in Chrysalises, by E. B. Poulton.—The author remarked that some years ago Mr. T. W. Wood brought before the notice of the Entomological Society of London some proofs that certain chrysalises imitated the colour of the surfaces upon which they threw off their caterpillar skin. The intimation was received with some amount of credulity by leading entomologists, but evidently without sufficient reason. For some years the writer had been working upon the colour of caterpillars in relation to the colour of their surroundings, and he had shown that the colour could be modified in one generation by the alterations of their surroundings. It seemed certain that through some sensory surface, possibly the eye, caterpillars were affected by their external relations, and a corresponding effect was produced in colour. Mr. Wood's experiment was but a special case of some general method of production. He explained the results by supposing that the moist surface of a fresh chrysalis was photographically sensitive to the colour of surrounding surfaces. That appeared to be merely a metaphor, and was unsupported by proof. It was more probable that the colour was produced by the effect upon the caterpillar before it turned to the chrysalis. Experiments were therefore made by the writer to put the fact itself beyond dispute. That was done first by the use of caterpillars of the peacock butterfly and the common tortoiseshell butterfly. It was found that by allowing them to turn to chrysalises upon a white or a black screen very different results were produced. Those upon white paper were often brilliantly golden, although the chrysalises of the tortoiseshell were not quite so golden. Gilded specimens were sometimes found, but their appearances seemed to be produced as a disease. While that was the case of chrysalises found in the fields, the specimens experimented with by the writer were perfectly healthy, and produced healthy butterflies. He then saw that, although a white paper produced a golden appearance, a gilded surface would produce the same effect to a greater extent. That bore in a most important manner on the use of the metallic tints of many of the exposed chrysalises of butterflies: which were thus seen to have harmonised with some metallic surroundings. The next point was to ascertain the period during which the caterpillar was sensitive to the colour of the surrounding surfaces, and the nature of the surface which was affected. The former end was achieved by carefully watching the caterpillars between the time at which they ceased feeding and that at which they turned to chrysalises. It was found that they were sensitive for many hours, even more than a day, before the change took place. The other object was attained by placing the larvæ suspended downwards for ten or twelve hours before the change took place in a tube, of which the upper part was golden and the lower black, the two being separated by a perforated disk. The caterpillar's head was turned round so that it could not see through the aperture, and the result showed that the chrysalises were the colour of the chamber in which the head was placed. Hence it seemed that the sensory surface must have existed upon that area. The full results, however, had not yet been obtained.

The Nervous System of Sponges, by Dr. R. von Lendenfeld.—The author gives an account of his discoveries on this subject up to date. Sensitive and ganglia cells have been observed by him in a good number of sponges. Their locality varies, their shape is constant. They are mesodermal, and appear to preside over the movements of the membranes and pore-sieves, and so regulate the water current. The great difference between sponges and higher cœlenterates is, that in the former the most important organs are mesodermal, whilst in the latter they are ecto- or ento-dermal. He divides the type Cœlenterata accordingly into *Cœlenterata Mesodermalia*, or sponges, and *Cœlenterata Epithelaria* or Cnidaria, as Lulitypes.

The Function of Nettle-Cells, by Dr. R. von Lendenfeld.—The author gives a detailed account of the structure of the nettle-cells, or cnidoblasts, and discusses some biological facts regarding their function. He comes to the conclusion that the nettle-cells are exploded by direct reflex action when the cnidocil is touched, but that the animal can counteract this reflex action by a centrifugally acting nervous irritation in a similar manner as reflex actions are controlled by higher nervous centres in man.

Note on the Floral Symmetry of the Genus Cypripedium, by Dr. Maxwell T. Masters, F.R.S.—In this note the author adverts to so much of the normal structure of Orchids in general, and of Cypripedium in particular, as is necessary for the elucidation of his subject, and proceeds to describe a case of regular peloria in *Cypripedium caudatum*, which shows a reversion to the typical form of Orchids, and goes to prove that the so-called genus Uropedium is only a pelorian form of Cypripedium. The construction of the andrœcium in these plants is then alluded to, and illustrations given of all intermediate stages from monandry to hexandry. The frequently observed tendencies to a dimerous condition, and to the development of the inner row of stamens, are alluded to, and the significance of these changes pointed out. The morphological changes consequent upon hybridisation, and the inferences to be derived from them, are passed under review. The paper concludes with a general summary of the teratological changes observed in the tribe Cypripedieæ.

Notes on Australian Cœlenterates, by Dr. von Lendenfeld.—The author describes the extraordinary mode of development of *Phyllorhiza punctata*, a rhizostomous Medusa discovered by him in Port Jackson. The Ephyra has eight, the next stage twenty-four, the next sixteen, and the adult again eight marginal bodies. If the umbrella margin is injured and newly formed, marginal bodies appear between *all* the newly-formed flaps. Further, the migrations of *Crambessa masaica* at the breeding time are described. This and other species of that genus of rhizostomous Medusæ migrate far up the rivers, like the salmon, to deposit their young. A remarkable change in the colour of *C. masaica*, which has taken place in Port Jackson since the observations of Huxley about fifty years ago, is described. A new variety, which is brown, seems to have been produced or to have immigrated and superseded the *blue* form, which was observed by Huxley and others in that locality. In Port Philip the blue variety is still found. The author has found in examining the lower freshwater animals that the freshwater Hydroids and Sponges, as also the freshwater Rhizopoda of Australia, are very similar to the European, whilst the *marine* species of these groups differ very much in the two localities. He concludes that these freshwater forms are very old and conservative, and may be supposed to be the unchanged offspring of old ancestral forms, as such possessing particular systematic importance.

Bugio; the Biological Relations of an Atlantic Rock, by Michael C. Grabham, M.D., F.G.S., F.R.C.P.—Region almost unknown, but interesting as being typical of flora distribution, and of variation in isolation. Author proposed to illustrate present knowledge by reference to prominent forms, animal and vegetable, existing at Bugio, the most unknown of the Dezerta islets.

Dezerta.—Physical characters: Foundation on a narrow ledge; dimensions never much greater; no evidence of ancient contact; not survivals of an ancient continent, but islands in a Miocene sea, deriving their first colonists from Miocene Europe.

Description of Bugio.—Difficulty of access; central volcanic dyke; large proportion of tufas; no sections of old river-beds or surface obliterations; summit showed deep clay-beds and surface deposits of calcareous sand and earth.

Flora.—How related to Madeira; arbitrary distribution; absence of easily wafted forms; *Senecio incrassatus*, Madeiran and Canarian varieties; *Echium fastuosum*, maritime form of; hybrid with *E. simplex*, remarkable perpetuation of perennial growth, and other changes; several instances of fitful distribution; *Chrysanthemum dæmatomma*, a distinct and only species; remarks on cognate Madeira forms; *Monizia edulis*, Dezertan, Salvagic, and Madeiran examples; Miocene origin of.

Fauna.—Rabbit, identical with that of Porto Santo, described by Darwin as having acquired specific characters in shortened length, and colour of skin. *Sea-birds* breeding at Bugio: *Sternus hirundo, Thalassidroma bulwerii*, and many others. *Procellaria anglorum* dominant to the exclusion of *P. major* and *P. obscura*. Influence of birds in migration of plants and mollusks. *Testacea*: Distribution and affinities—*Helix crystallina*, affinities of; *H. erubescens*, distribution of; *H. punctulata*, modification of; *H. Leonina*, area of, and relations; *H. vulgata*, dwarfed example of; *H. polymorpha*, distinct races of connections of *H. tiarella, H. coronula*, and *H. grabhami*. *Coleopterous Deucalion*, isolated species, now related to a Salvagic form.

Summary.—Showing the difficulties attending the determination of the origin and migration of species to be equally great in the component rocks of a group of islands as in the archipelago itself. Agency of man, chiefly in extinction and destruction, illustrated by introduction of opposing or contaminating forms; ravages of Eupatoria and *Phylloxera vastatrix* in Madeira; surviving vigour of Miocene plants. Author's paper only meant to be indicative of those branches and details which might singly occupy the attention of the Section.

The Multiplication and Vitality of certain Micro-organisms, Pathogenic and otherwise, by Percy F. Frankland, Ph.D., B.Sc., F.C.S., F.T.C., Assoc.Roy.Sch.Mines.—In this paper the author records a number of experiments which he has carried out on the multiplication of the micro-organisms present in natural waters, and also on the vitality of certain pathogenic organisms when purposely introduced into similar media. These phenomena have been studied by aid of the method of gelatine-plate cultivation, originally devised by Koch. The first part of the paper treats of the influence of storage in sterilised vessels, upon the number of micro-organisms present in the unfiltered water of the Rivers Thames and Lea, in the waters of these rivers after sand filtration by the companies supplying the metropolis, and in deep-well water obtained from the chalk. Of these three different kinds of water, at the time of collection the unfiltered river-waters are the richest in micro-organisms, containing, as they do, several thousand microbes, capable of being revealed by plate-cultivation, in 1 cubic centimetre of water, whilst the filtered river-waters have this number generally reduced by about 95 per cent., and the number present in the deep-well water rarely exceeds ten per cubic centimetre. On storage in sterilised vessels at 20° C., however, a great change in the relationship of these numbers soon takes place, for whilst the number of organisms in the crude river-water undergoes but little change, or even suffers diminution, that in the filtered river-water exhibits very rapid multiplication, and this increase is even still more marked in the case of the deep-well water. The author suggests that the differences in the rate of multiplication exhibited by these three kinds of water is dependent upon the number of different varieties of micro-organisms which they contain. Thus in the unfiltered river-waters the organisms belong to a number of different kinds; the filtered river-waters exhibit fewer varieties; whilst in the deep-well water the number of varieties is still more limited, the gelatine-plates having generally the appearance of almost pure cultivations. The microbes in the deep-well water will thus be less hampered in their multiplication by hostile competitors than those in the filtered river-waters, and these again less than those in the crude river-waters, in which an equilibrium must have already been established between the various competitors. When the waters were exposed to a temperature of 35° C., the multiplication was in all cases very much more rapid, but both at 20° C., as well as at 35° C., the multiplication was, on prolonged storage, followed by reduction. The pathogenic forms which have been studied by the author are: (1) Koch's "*Comma*" *spirillum* of Asiatic cholera, (2) Finkler-Prior's "*Comma*" *spirillum* of European cholera, and (3) the *Bacillus pyocyaneus*, which produces the greenish-blue colouring-matter frequently present in abscesses. The vitality of these organisms has been studied by introducing minute quantities of their cultivations into sterilised distilled water, deep-well water, filtered Thames water, and London sewage. In these media they present some very striking differences. Thus the *Bacillus pyocyaneus* was found to flourish in all; even in distilled water it was present in largely multipled numbers after fifty-three days. Koch's "*Comma*" *spirillum*, on the other hand, when introduced into deep-well water was no longer demonstrable after the ninth day, whilst in sewage it was still found in enormously multiplied numbers after twenty-nine days. Finkler-Prior's "*Comma*" *spirillum*, although showing such far greater vital activity than Koch's in gelatine cultures, possesses far less vitality than the latter when introduced into water. Thus in the above-mentioned media it was in no case demonstrable after the first day.

SCIENTIFIC SERIALS

American Journal of Science, September.—A post-Tertiary elevation of the Sierra Nevada, shown by the river-beds, by Joseph Le Conte. In further elucidation of his already published speculations regarding an upheaval of the Sierra Nevada towards the close of the Tertiary epoch, the author here brings forward much additional evidence, also correlating this movement with a contemporaneous elevation in other parts of the western half of the continent. He endeavours to show that the upward movement, which seems to have affected all high latitude regions at that time, but which was oscillatory and therefore temporary on the eastern side of North America and in Europe, on the Pacific slope was permanent, and has largely determined the orographic structure of that region.—The strain effect of sudden cooling, as exhibited by glass and by steel (second paper), by C. Barus and V. Strouhal. In their first communication the authors compared the strains experienced by glass and steel on sudden cooling, by aid of the density variations observed when the bodies carrying strain were annealed, *as a whole*. Here they seek to confirm their earlier inference relative to the temper-strain of glass. They also investigated the density-relations of consecutive similar shells of the Prince Rupert drop, and the optical character of the successive cores. In general it is shown that the optical effect of the temper-strain in glass may be regarded as the analogue of the electrical effect of the temper-strain in steel. In a further communication a more specific inquiry will be made into the causes of hardness itself, with a view to throwing some light on the mysterious transformations of carbon.—Devonian Lamellibranchiata and species-making, by Henry S. Williams. In connection with the publication of Prof. James Hall's monograph on Devonian Lamellibranchs, completing vol. v. part 1 of the "Palæontology of New York," it is pointed out that fossil species, and even genera, are unduly multiplied on totally inadequate data. Species and genera cannot be regarded as established so long as the author himself is unable to distribute the typical specimens, twice alike, without reference to the original labels.—Note on the composition of certain "Pliocene sandstones" from Montana and Idaho, by George P. Merrill. While lately classifying the rocks collected in Montana and Idaho by Dr. A. C. Peale in 1871, the author's attention was called to some fragments labelled as "Pliocene" sandstones. A glance, however, showed that they strongly resembled compacted volcanic dust and sand, and a microscopic examination made it evident that the stones consisted very largely of minute flakes of pumiceous glass sufficiently compacted to be readily broken out into hard specimens, but extremely friable. The specimens are fully described and some speculations offered as to their probable origin. It is added that in Kansas and Nebraska these dusts are collected and sold as "diamond polishing powder," or used in the preparation of the so-called "geyserite" scouring-soap.—Contributions to mineralogy, by W. Earl Hidden, with crystallographic notes by A. Des Cloizeaux. The paper deals with the ipodumene, black tourmaline, xenotime, and twin crystals of monazite from North Carolina; a remarkable crystal of herderite found in 1884 near Stoneham, Maine; a twin crystal of molybdenite from Renfrew, Canada; and the phenacite from Florissant, El Paso County, Colorado.—Turquois from New Mexico, by F. W. Clarke and J. S. Diller. A full analysis and microscopic study is given of some specimens from the turquois mines of Los Cerillos, New Mexico, about 22 miles south-west of Santa Fé. The turquois-bearing rock appears to be eruptive, and probably of Tertiary age, while the small size of the veins and their limited distribution show that the turquois is of local origin, possibly the result of alteration of some other mineral.—On the electrical resistance of soft carbon under pressure, by T. C. Mendenhall. In reply to Prof. Sylvanus P. Thompson's objections, the author describes some fresh experiments fully confirming his views regarding the change in the resistance of carbon due to change of pressure. In the form of compressed lamp-black the electrical resistance of carbon varies greatly with the pressure to which it is subjected, and the variation is mainly due to a real change in the resistance of the carbon itself.—Comparison of maps of the ultra-violet spectrum, by Edward C. Pickering. Prof. Rowland's recently published photograph of the solar spectrum is compared with Draper's map of the ultra-violet portion of the spectrum prepared in 1873, with which it is shown to agree very closely. The mean difference for the seventy-six lines compared was 0·012, corresponding to about 1/800 inch upon the Draper map. It may therefore be assumed that the probable error of a wave-length derived from this map will not exceed 1/100 unit if the correction here given be first applied.—On two hitherto undescribed meteoric stones, by Edward S. Dana and Samuel L. Penfield. One of these meteorites was found, in 1869, between Salt Lake City and Echo, Utah; the other, in 1846, near Cape Girardeau, South-West Missouri. Olivine is the most prominent constituent of the former, while the latter is a light gray chondrite.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, September 20.—M. Fizeau in the chair.—Kinematic analysis of human motion, by M. Marey. In the figure accompanying this paper are represented the successive attitudes of the lower right limb while describing a complete step. This action is shown to be divided into two periods, a rest and a rise, which are again subdivided into four unequal phases, of which the last three belong to the period of rise. The simultaneous movements of ankle, knee, and hip are explained, and it is pointed out that, whatever be the velocity of the pace, the form of the various trajectories here described is maintained in their salient features. But, the more rapid the motion, the more is the tendency of the centre of gravity to approach a straight line parallel with the surface of the ground. —"Modern Kinetics and the Dynamism of the Future," by M. G. A. Hirn. This is the title of a new work, which the author presents to the Academy with some remarks explaining its general purpose. After replying to the various objections raised against his general principles, he deals with the arguments which, as he maintains, render henceforth indefensible the kinetic theory of the gases, referring to molecular movements most of the properties of these bodies. Three arguments are advanced of such a nature that he believes future physicists will wonder how this kinetic theory could ever have been accepted for a single moment. Even were it correct, it would not follow that light, radiant heat, electricity, magnetic attraction and repulsion, and gravitation were due to movements of ponderable matter, far less that thought itself was nothing more than a molecular movement. But the reverse is not true, so that with the collapse of the kinetic theory of the gases fall the kinetic theories in general, which claim to explain all possible phenomena of the universe by invisible movements of matter. The doctrine here substituted for kinetic force, he thinks, explains quite as easily, and much more rationally, the universal phenomena of the physical world. He does not, however, hope at once to convince all minds of what they should have long ago been themselves convinced. Interpretations formulated *a priori*, and apart from experience and observation, have unfortunately more vitality than truths gained to science by the patient study of Nature.—Observations of Winnecke's comet made at the Observatory of Nice (Gautier equatorial), by MM. Perrotin and Charlois. The results of these observations, which extend over the four days from August 27 to September 1, are embodied in tables showing the positions of the stars 25339 Lalande, 25588 Lalande, 4989 Schjellerup, 5004 Schjellerup, and the apparent positions of the comet.—On the transformation of algebraic surfaces in themselves, by M. Emile Picard. A proposition analogous to that of Schwarz is thus formulated: Algebraic surfaces capable of being transformed in themselves by a bi-rational substitution, including two arbitrary parameters, are of the genus zero, or one.—On a class of differential non-linear equations, by M. Roger Liouville.—Historical note on a series whose general term is of the form $A_n (x - a_1) (x - a_2) \ldots (x - a_n)$, by M. G. Eneström.—Researches on the structure of the nerve-centres in the Arachnidæ, by M. G. Saint-Remy. Having in a previous communication dealt with the structure of the brain of the scorpion, the author here extends his observations to the spider family, and more particularly to *Tegenaria domestica*, *Epeira diadema*, and *Phalangium opilio*. In these groups he shows that the brain offers the same plan of organisation as that of the Scorpionidæ.—Fresh researches on the configuration and extent of the Carmaux Coal-measures, by MM. Alfred Caraven-Cachin and Grand. In this basin, which extends for nearly six miles from Rozières to Saint-Quentin, there are in some districts three successive coal-deposits with a joint thickness of over 31 metres underlying Tertiary formations 156 metres thick. They appear to have been deposited horizontally, always in shallow water, the land subsiding sometimes slowly, sometimes intermittently, during the whole period of their formation.—Note on the affinities of the Oolitic floras in the West of France and in England, by M. L. Crié. In this paper the author communicates the first results of his studies of the Oolitic floras of these regions. The conifers are represented at Mamers (Sarthe) and at Scarborough (Yorkshire) by traces of Brachyphyllum, which present a remarkable identity. Certain imprints at Scarborough also show a strong resemblance, in the disposition of the foliage, and especially in the veinous system, to *Otozamites marginatus*, Sap., which is so characteristic of the Mamers flora. About the middle of the Oolitic period this group must have covered certain upheaved tracts in the Venetian Alps, in the neighbourhood of Mamers, and at Scarborough.—The waterspout of September 14 at Marseilles, by M. Barthelet.

BOOKS AND PAMPHLETS RECEIVED

"How Readest Thou? or the First Two Chapters of Genesis," by E. Dingle (Partridge and Co.).—"The Chalk and Flint Formation," by W. B. Galloway (Low and Co.).—"Life-History of Plants," by Prof. D. M'Alpine (Sonnenschein).—"Tobacco: a Farmer's Plant," by P. M. Taylor (Stanford).—"Therapeutics founded upon Organopathy and Antipraxy," by W. Sharp, M.D. (Bell and Sons).—"Report of the Iowa Weather Service, January to April 1883," by Dr. G. Hinrichs.—"Scientific Romances: No. v. Casting out the Self," by C. H. Hinton (Sonnenschein).—"Lessons in Elementary Dynamics," by H. G. Madan (Chambers).—"Studies in Ancient History," N.E., by J. F. McLellan (Macmillan).—"Manual of the New Zealand Coleoptera," parts 3 and 4, by Capt. T. Brown (Didsbury, Wellington).—"School of Forest Engineers in Spain," by Dr. J. C. Brown (Oliver and Boyd).—"Hand-book of Mineralogy," by J. C. Foyr (Van Nostrand, N.Y.).—"Monographs U.S. Survey," vol. ix. (Washington).—"Hommage à M. Chevreul, à l'Occasion de son Centenaire" (Alcan, Paris).—"The Handy Natural History," by J. G. Wood (Religious Tract Society).—"General Report on the Operations of the Survey of India Department," 1884-85, by Col. G. De Prée (Calcutta).—"Notes on the Bones of a Species of Sphenodon," by W. Colenso.—"The Economical Aspects of Agricultural Chemistry," by H. W. Wiley (Wilson, Camb., Mass.)—"Report on the Decapod Crustacea of the *Albatross* Dredgings off the Coast of the United States," by S. I. Smith (Washington).—"Metodo per Misurare la Dilatazione Termica dei Corpi Solidi: Memoria di F. Artimini (Firenze).—"The Cause of Electricity, with Remarks on Chemical Equivalents," by G. T. Carruthers (Benares).

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