THURSDAY, AUGUST 27, 1885

THE LIFE OF FRANK BUCKLAND

Life of Frank Buckland. By his Brother-in-Law, George Bompas. (London: Smith, Elder, and Co., 1885.)

FEW Englishmen were unacquainted with the central figure of this admirably written memoir. His ubiquity as a lecturer and inspector, the happy self-forgetfulness and adaptability of manner which associated him with royal princes as readily as with seaside fishermen, and the strong personality by which he permanently impressed all who came in contact with him, made him beyond all other men of his time the representative and the preacher of the subject to which he devoted all the energies of his life. That subject was natural history, a term not without meaning even in the present day of minute and subdivided scientific work, but conterminous with science half a century ago, when comparative anatomy was hardly known, when the microscope was costly and imperfect, when the provinces of nature had not been mapped nor its workers differentiated.

Frank Buckland was born a naturalist, into a home crammed with animals, living, preserved, fossil; his mother a woman of rare intellectual accomplishment and scientific taste, his father the first geologist of the age. At three years old he could "go through all the natural history books in the Radcliffe Library"; at four we find him lispingly explaining to a Devonshire parson who had brought with pride to Dr. Buckland "some very curious fossils," that they were the vertebræ of an Ichthyosaurus; at five he is rapturous over the teleology of the "tongue-bone" in the skeleton of a whale; and in the archæology of Worcester Cathedral can find only one object of interest—the figure of a lady who had been starved by a disease in the throat.

At twelve he went to Winchester, not the least barbarous school of that barbarous scholastic time. He was "launched," and "tin-gloved," and "toe-fit-tied," and "tunded," and "clowed," and "watched out" at cricket, and "kicked in" at foot-ball, living for two or three years the wretched life of a college junior amid a mob of boys not overlooked by any master and influenced by the bad traditions of a savage past. He used to say that it had done him good, had cured him of "bumptiousness" and arrogance, but he cherished painful memories of individual tyrants and of special acts of tyranny, and was wont when a senior boy to criticise with a bitterness alien from the ordinary conservatism of schoolboys the coarseness of a system which turned a gentleman's son, bred in the refinement of a cultured home, into an abject domestic serf.

Buckland's fagging days over, he was happy, for he could follow his bent undisturbed, and the pages which describe his later Winchester life are amongst the most amusing in the biography. Fond of school work he was not; he was, in fact, looked upon as a "thick," and his compulsory fagging experiences had given him a dislike for games. But he wired trout and eels in the clear Itchen streams, dug out mice on "Hills," chased badgers on Twyford Down, skinned and dissected cats, moles, and bats, articulated skeletons, baked squirrel pies, and cooked mice in batter. A buzzard, an owl, and a racoon tenanted his lockers in "Moab," a viper lived in his "scob" amongst his books, his hedgehogs kept open a perpetual fosse at the base of the college wall, and a regiment of tame jackdaws looked up to him as their patron. On "Saints' days" he attended the Winchester Hospital, bringing back gruesome fragments of humanity in his pocket-handkerchief, talked medical language, treated confiding boys professionally. Applying for admission to the sick house on behalf of a patient who had partaken too generously of "husked gooseberry fool," he informed the surprised second master that the invalid had a "stricture of the colon;" he was wont to offer sixpence to any junior who would allow himself to be bled; and he treated surgically a football-wounded shin with such results that the leg when shown eventually to a doctor was pronounced to be in imminent danger of amputation.

The Winchester life found fuller development at Oxford. No one who knew Frank Buckland there will forget those merry breakfasts in the corner of Fell's Buildings; Frank in the blue pea-jacket and the German student's cap, blowing blasts out of a tremendous wooden cow-horn; the various pets who made it difficult to speak or move: the marmots, and the dove, and the monkey, and the chamæleon, and the snakes, and the guinea-pigs, and the after-breakfast visits to the eagle or the jackal or the bear or the pariah dog in the little yard outside. His Long Vacations were spent in Germany, whence he brought back little besides collections of red slugs and green frogs; in 1848 he entered at St. George's Hospital, and in 1854 was gazetted Assistant-Surgeon to the second Life Guards.

The next eight years were very pleasant ones. His father's position as Dean of Westminster threw open to him all the best society in London: we read of parties at Miss Burdett-Coutts's, at the Duke of Wellington's, at Chief Baron Pollock's; microscopic evenings at Dr. Carpenter's; walks around the Abbey with Prince Albert; conversations with Sir B. Brodie, Mr. Gladstone, Whewell, Whately, Prof. Owen, Sedgwick, Bunsen, Ruskin. He was beginning to feel his strength and strike out his line in life; in these years he wrote his first magazine article, delivered his first lecture, published his first book. In 1865 he resigned his commission, married, took the house in Albany Street which he has made historic, started *Land and Water*, devoted himself to fish culture, became Inspector of Fisheries, and worked in his vocation till 1880, when he died at the age of fifty-four, worn out by excessive overwork and by the exposure to wet and cold in all seasons which his professional duties, as he interpreted them, involved.

His power as a lecturer was unrivalled. He could keep an audience in ecstasies of laughing enjoyment for two hours at a stretch. He had inherited his father's remarkable felicity of illustration; his own keen delight in his subject was contagious, his comedy incessant and irresistible. Never was a memory more stored with interesting facts. He was all eyes; noted everything, remembered everything, used everything. Through London streets, as he surveyed them from his favourite seat on the knifeboard of an omnibus, on the walls of exhibitions, on sea-coast, river-shore, and hill-side, in the belfry at Ross, by Dean Gainsford's grave—phenomena which others overlooked or passed as trivial were by him pounced upon and analysed and made to bear fruit in discovery and correlation and historical association and practical scientific use. Of human prodigies in every department he was the recognised Proxenus and patron. Miss Swann the giantess and her husband Captain Bates the giant, and the Two-headed Nightingale, and the Siamese Twins, and the New Zealand Chiefs, and Fatima, and Zariffa, and Julia Pastrana the hairy woman, and Benedetti the sword swallower, and the Wild Man of the Woods, and the man who could sing two notes at once, and the man who could drink a bottle of milk under water,—all looked up to him as a father, or sat as guests at his table. He came by degrees to be accepted as an *Arbiter monstrorum;* as the necessary referee whenever any strange revelation or any novel puzzle presented itself in the world of nature. If a whale ran on shore at Gravesend, or a dolphin at Herne Bay; if an unusual sturgeon or tunny was consigned to a London fishmonger; if the lawyers at Nisi Prius were at issue whether a hole in a ship's bottom could have been made by the beak of a swordfish, or the Gloucester Magistrates hesitated over the identity of elvers with young eels; if a sick porpoise arrived at the Zoological Gardens in a condition requiring brandy and water to be exhibited internally and caustic applied without; if the Chief Rabbi felt searchings of heart as to whether oysters might for edible purposes be inserted in the Mosaic catalogue of things that creep; if a sea-lioness were ill in the Aquarium, or a plague of frogs occurred at Windsor; if search were required for John Hunter's coffin in St. Martin's Church, or the skeleton of William Rufus had to be exhumed in Winchester Cathedral,—it was inevitable that Frank Buckland should be telegraphed for first of all. And the influence he exerted was often highly beneficial. To his interference we owe the close time for seals and the Bill for the preservation of marine birds. A description in *Land and Water* of a neglected Museum at Canterbury shamed the Curator into setting it to rights; his good-humoured criticism, from a naturalist's point of view, of the pictures in the Royal Academy, taught the artists beneficially that an eye as keen as Ruskin's was noting their performances in a region beyond Ruskin's reach.

His home in Albany Street was one of the sights of London; but to enter it presupposed iron nerves and a stomach like those of Horace's reapers. Iron nerves—for, introduced at once to some five-and-twenty poor relations, exempt from shyness and deeply interested in your dress and person, to Jacko, and the Hag, and the Nigger, and Jenny, and Tiny, and the parrot and the jaguar, and the laughing jackass, and Jemmy the suricate, and Dick the bear, and Arslan the Turkish wolf-dog, you felt, like Jaques in the play, as if another flood were toward, and the animals were parading for admission. Dura ilia—for the genius of experiment, supreme in all departments of the house, was nowhere so active as at the dinner-table. We read of panther chops, rhinoceros pie, bison steaks, kangaroo ham, horse's tongue, elephant's trunk; of whale boiled with charcoal to refine the flavour; of tripang and lump-fish; of stewed whelks and land-snails, roasted hedgehog, potted ostrich. We notice in the diary such entries as "seedy from lump-fish;" "very poorly indeed, effects of horse;" and we sympathise with a departing guest who notes—"tripe for dinner—don't like crocodile for breakfast."

He was the Samson of science; the "Sunny One" amongst *savants*, as was Manoah's son amongst judges; roars of genial laughter accompany the heroism and the feats of both. But the comic recollections which surround him ought not to mask the serious admiration which is his due;—first, as a public teacher, circulating popular science, generating field clubs and microscopical societies, preparing a public to appreciate and to support the more purely scientific labourer; secondly, as a material benefactor, raising in fifteen years the commercial value of English and Scottish salmon to the extent of 100,000*l.* per annum; thirdly, as having in a manner rare, if not unique, passed behind the veil which hangs between us and the animal creation. He understood their gestures and expressions as we interpret those of one another, and they understood him in their turn; the creatures at the Gardens, the beasts at Jamrach's, the pets at home, seemed to know him in a human fashion; his dying words—"God is so good to the little fishes that I do not think He will let their inspector suffer shipwreck at the last"—show his identity of feeling with them; no one could talk to him long without a strangely new and reverential sense of brotherhood with these existences who were to him so entirely fraternal as people of his Father's pasture and sheep of his Father's hand. Science has had very many greater sons; none more simple, modest, blameless; none more genial, more humane, or more beloved.

W. TUCKWELL

COMPENSATION OF COMPASSES

Practical Guide for Compensation of Compasses without Bearings. By Lieut. Collet, French Navy, Tutor in the Polytechnique School of France. Translated by W. Bottomley. With a Preface by Sir W. Thomson, F.R.S., &c. (Portsmouth: Griffin and Co., 1885.)

THIS work appears in its English garb under the auspices of Sir W. Thomson.

In the published instructions for the adjustment of his patent compass, Sir William Thomson gives short directions for the use of the deflector, an instrument to facilitate correcting that compass by magnets and soft iron when neither bearings of sun nor terrestrial objects can be obtained. With this deflector a fog is not the unwelcome visitor it generally is, for with the fog there is often a smooth sea, a condition favourable to a successful use of this delicate instrument.

As an invention of Sir W. Thomson it is certain that the inquirer into the use of the deflector will at once be disposed to look for an instrument theoretically correct in conception and of great refinement in construction. The useful work of fully describing the practical applications and several uses of this instrument has, however, been left to an able writer on subjects connected with the compass in iron ships—Lieut. Collett, of the French Navy—and the book now under review is the result.

It may be remarked that Sir W. Thomson, in the preface, fully recognises it as a complete and able exponent of the uses of this deflector, which is an important point to those desirous of using the instrument.

In four chapters of his Practical Guide M. Collet has given, in detail, practical rules for correcting the errors of the compass without bearings, illustrated by numerous examples, and including instructions for the graduation of the deflector, or measuring the magnetic force for each division of its scale. Collected in a tabular form, the results of this graduation will be found of great use to observers, and of the five advantages arising from it enumerated by the author, not least is that which gives an approximate value of the coefficients of deviation. This would prove useful when the observer, wishing to leave the magnets undisturbed, required only to know if any change of deviation had taken place.

Another advantage of this graduation is that it forms an additional method of measuring the diminution of the mean directive force of the compass on board ship as compared with that on land, or the term λ of the text-book. λ is a necessary element in the exact correction of the heeling error—a part of the correction to which the author devotes a chapter, as it rightly comes under the denomination of a compensation requiring no bearings.

Lieut. Collet, in his introductory chapter and elsewhere, strongly urges that the deflector, concerning the uses of which he has written so fully, should in the immediate future become the chief instrument used in the compensation of compasses, on account of the rapidity and sufficiency of precision with] which it may be made, and that it be adopted for frequent if not daily use on board ships at sea. Before remarking on this proposal it may be as well to inquire into the present customs with regard to the standard or navigating compass at the time of its first compensation and subsequent changes of deviation.

In the Royal Navy the adjustment of compasses is invariably made by bearings, and the instances are rare when the adjustments of the standard compass alone, including the final swinging of the ship, occupy more than an hour or two with results absolutely correct. Subsequently to this one adjustment the compensating magnets are not moved during their three or four years' period of service, but the deviations of the compass are carefully observed on all occasions when bearings can be taken—in other words, from day to day—and noted for guidance when bearings cannot be taken. In the Mercantile Marine a large number of ships are fitted with Sir W. Thomson's standard compass with the accompanying magnets adjustable at pleasure. This compass is often compensated by experts in the use of the deflector and the magnets left in a given position.

Now, what is the almost universal practice of the commanders of these vessels subsequent to this adjustment by means of the deflector? They observe the deviation frequently by day and night when possible, note the results in a compass journal for present and future guidance, and object most strongly to any alteration of the magnets.

In the paragraph headed "Weather" it will be seen that a moderately smooth sea is required when using the deflector, and in another place it will be seen that it is no certain guide to navigation unless observations are made on all the cardinal points. The question therefore arises, Are the necessary conditions for using this instrument often available in the North Atlantic and "roaring forties," when bearings are at times unobtainable for some days?

The result of the foregoing consideration is to show that there is long custom of very practical men—and possibly prejudice—to overcome before Lieut. Collet's future of frequent use of compensation without bearings becomes general.

The nautical world has had the deflector as invented by Sir W. Thomson before it for some years; it now has an excellent practical guide to its use in the book under review, and it remains to be seen how far that world will avail itself of the invention.

It may probably suggest itself to some minds that the book would lose none of its value by being shortened somewhat in detail; indeed, the shorter the better, if combined with accuracy for the practical navigator, and should a new edition be required the translator who has done his part well, and knows the deflector thoroughly, will perhaps try his hand at the work of condensation.

THE FORBES MEMORIAL VOLUME

In Memoriam. The collected Scientific Papers of the late William Alexander Forbes, M.A., Fellow of St. John's College, Cambridge, Lecturer on Comparative Anatomy at Charing Cross Hospital, Prosector to the Zoological Society of London. Edited by F. E. Beddard, M.A., Prosector to the Zoological Society of London. With a preface by P. L. Sclater, M.A., Ph.D., F.R.S., Secretary to the Zoological Society of London. (London: R. H. Porter, 1885.)

THE death of Alfred Henry Garrod at the early age of thirty-three was a great misfortune to the cause of zoology in this country. But that his distinguished successor, William Alexander Forbes, a man full of vigour and in the best of health, should have suddenly succumbed to the influence of a pernicious climate at the age of twenty-eight, was perhaps a still more severe blow, and one that will long be felt by the naturalists of the present day. We do not seek to compare Forbes with Garrod, but it must be recollected that Forbes was a man of undoubtedly strong physique, for whom there was every prospect of a long and successful career. There can be not the slightest doubt that, had he not lost his life from the accidental force of circumstances, Forbes would have left a considerable mark on the progress of science. As regards natural history at least, if not in some other matters, Forbes was a universal genius. Of the whole zoological series he had an enormous knowledge, ranging from one end of the animal kingdom to the other. Possessed of a most retentive memory and of an abundant stock of energy, he was unremittingly at work on his favourite subject, and never forgot what he had acquired either by reading or by experience. Not only was he thoroughly up in zoological literature, but he was also an accurate observer and a diligent collector in the field, where nothing came amiss to him. Mammals, birds, butterflies, and beetles were perhaps the groups which he knew best; but Forbes had, as already stated, an excellent general knowledge of the whole animal series. Whatever novel object might be shown to him he was very rarely at a loss for its correct name, nor for where to refer to for information about it.

It can thus be well understood, even by those who never had the good fortune to know Forbes, that the loss of such a man was keenly felt by his numerous friends and fellow-workers. Soon after his death, in 1883, it was resolved, at a meeting of the Zoological Club, that some sort of memorial of him should be carried out. After due consideration of the question it was unanimously determined by the Committee to whom the subject was referred that the best scheme would be the republication of Forbes's numerous papers in a connected form. This had been the course adopted in the case of Garrod, who had preceded Forbes in the Prosectorship of the Zoological Society of London. It was found that Forbes's contributions to science would make a volume of about the same size as the scientific papers of Garrod, and would not, it was believed, be of inferior interest.

The memorial volume, prepared and issued under these circumstances, contains sixty-seven papers published by Forbes in different periodicals from 1875 to 1882. The original illustrations have been in every case reproduced, and to increase the usefulness of the reprint, exact references to the paging of the original articles are added in the margin. At the end of the volume is given Forbes' last journal, reprinted from the *Ibis* for 1883, and containing a most interesting account of his observations during his fatal expedition up the Niger. Forbes died at Shonga, one of the stations of the United African Company on that malarious river, on January 14, 1883. Up to two days before his death the entries in the journal are in his own writing. The fatal termination of his illness, recorded by another hand, concludes the volume.

OUR BOOK SHELF

Elementary Algebra for Schools. By H. S. Hall, B.A., and S. R. Knight, B.A. (Macmillan, 1885.)

THIS is, in our opinion, the best *elementary* Algebra for school use. It is the combined work of two teachers who have had considerable experience of actual school teaching, aided by the advice of such men as the present Head of Clifton College, and so successfully grapples with difficulties which our present text-books in use, from their authors lacking such experience, ignore or slightly touch upon. Up to the point to which the subject is carried in this volume, it is treated with sufficient completeness for ordinary school purposes: the last four chapters present a somewhat concise account of ratio, proportion, and the progressions, which, however, covers enough ground for the ordinary examinations which schoolboys have to encounter. The authors propose to treat these parts in fuller detail in a *Higher Algebra*, which they are preparing. We do not propose to examine the book at any length, but confidently recommend it to mathematical teachers, who, we feel sure, will find it the best book of its kind for teaching purposes. Many subjects of interest are also treated of, and a vast collection of (3500) examples will furnish ample exercise for the boys, and save the teacher the trouble of concocting illustrations of the best methods. Answers are furnished at the end, so that those teachers who do not care that their pupils should have them handy, may have them sewn up.

Key to the Elements of Euclid. By J. S. Mackay, M.A. (W. and R. Chambers, 1885.)

THIS is a most valuable pendant to the edition of the "Elements" which we recently had occasion to notice so favourably. It is a book of nearly the same size as the "Elements" and yet contains, in consequence of the general omission of diagrams, solutions of the very large collection of admirable deductions which Mr. Mackay collected for the student in that work. De Morgan's words, quoted in the short preface, furnish ample ground for the omission of figures: "I am satisfied, from sufficient trial, that when proper description of the diagram is given in the text, the person who draws his own diagram from the text will arrive at the author's meaning in half the time which is employed by another to whom the successive appearance of the parts is prevented by his seeing the whole from the beginning."

The Essentials of Histology. By E. A. Schäfer. (London: Longmans, Green and Co., 1885.)

THIS will prove a useful book for students. It is arranged in forty-two lessons and appendix. Each lesson commences with a short statement of methods for the microscopic examination of the tissue described in the lesson. All simple tissues and organs are thus passed in review, and their most essential characters are succinctly described and illustrated. It is to be regretted that Prof. Schäfer has deviated from the customary plan of giving some kind of reference both for the text and the illustrations. The latter are mostly taken from Prof. Schäfer's portion of Quain's Anatomy, and their original source, although mentioned in Quain's, is here omitted.

An index at the end of the book would be a desirable addition.

E. KLEIN

An Atlas of Practical Elementary Biology. By G. B. Howes. (London: Macmillan, 1885.)

THE anatomical drawings of Mr. Howes have foı some years been well known in all laboratories where animal morphology is taught. In his "Atlas of Elementary Biology" he has now published a very complete series of figures illustrating the chief of those animal and vegetable types which are generally given to students in their first session. The need for such a work as this is well known to every one who has any experience of biological teaching; and the name of its author is a sufficient guarantee of the careful accuracy and artistic excellence of the drawings it contains. The low price at which a student's text-book must necessarily be sold has precluded the use of colour, which might in a few cases have given some additional clearness to the figures; but all that could be done with black and white has been done, and every figure is evidently a faithful copy of an actual dissection, such as a student may reasonably hope to repeat for himself.

In the case of every animal chosen, a series of drawings showing the gross anatomy of the adult is followed by a few illustrations of the minute structure of its tissues, and of its main developmental features.

The drawings of adult anatomy are throughout excellent; the others, though the size of the work has somewhat restricted their number, will probably suffice for most of the needs of commencing students. It is however to be regretted that there is no figure showing the minute structure of the gill in Anodon, and also that Mr. Howes has not been able to accept Spencer's statement as to the conversion of the frog's blastopore into the permanent anus.

The botanical portion of the Atlas contains an admirable series of figures, showing the structure of the plants described in Huxley and Martin's well-known text-book, and completes a work which cannot fail to be of the greatest service both to teachers and to students of biology.

W. F. R. W.

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.*]

Radiant Light and Heat

I AM sure that all students must be grateful to Prof. Balfour Stewart for his exposition in last week's NATURE (p. 322) of the errors and absurdities into which recent scientific men had fallen, and out of which they are now groping their way. But if it be not trespassing too much on his good nature, may I ask him one or two questions in order to further educe his views on points which he cannot but have given much thought to, though they are points which, without further explanation, some of us are liable to misunderstand. We have some of us had the "advantage of being wrong first," combined with the further advantage of thinking ourselves right, but I for one will now gladly admit that I was wrong, if I may thereby hope to join "the generation which is right."

The following are the five points I wish to receive help in understanding:—

(*a*) "It is absurd to suppose that particles of air are shot . . . with a constant velocity of 1100 feet a second."

I am disposed to agree; but am unable to see clearly how far this absurdity destroys the validity of the so-called "kinetic theory of gases," and of the mode in which sound is considered to be conveyed by such a medium, if indeed it is still so considered at all.

(*b*) "Can it be thought that hot bodies emit myriads of very small particles, which pass through space with the enormous velocity of 187,000 miles per second? Or again, is it likely that this velocity should be precisely the same for all bodies and for all temperatures?"

I should say it was highly *un*likely, in fact, that the idea is ludicrously absurd. This is a triumphant refutation of the corpuscular theory, but I am rather troubled by the thought that the argument seems equally to refute the wave-theory, if for "particles" in the above sentence, we substitute the word "waves." I know it is only my stupidity which causes me to feel this difficulty.

Again, it sometimes seems to me that the undulatory theory itself requires a good deal of "propping up;" and that several phenomena—for instance, "aberration"—explain themselves more easily and simply on the corpuscular.

(*c*) In speaking of the "transmutation of *visible energy* into heat," we are surely justified in calling heat "*invisible* energy" in contradistinction to the other; but, suppose the blow is so intense as to make a flash, are we to consider that flash as part of the invisible energy which has been "created," or are we to consider it a portion of the visible energy which has escaped destruction? The notion of a certain quantity of visible energy disappearing from the universe at one place, and an equivalent quantity of invisible energy being simultaneously created at another, is so beautifully simple and satisfying that I am sure the process can be made quite clear to any mind of common intelligence with a little more trouble.

(*d*) "This train of thought enables us . . . to assert that there is a definite mechanical relation between the amount of heat which leaves a hot body as it cools, and the radiant energy which accompanies the act of cooling."

I fear I am too stupid to understand this sentence. As I read it, it sounds like the following:—"There is a definite mechanical relation between the number of people which leave a train as it empties, and the number of people who get out of it and go away during the act of emptying." And the paragraph seems to go on thus:—"If, for instance, ten people get out of a train, and all of them enter an omnibus so as to be entirely absorbed by it, then, while the train has become ten people emptier, the omnibus has gained an equal number and has become ten people fuller."

I know that this is absurd, but I am unable to seize the point properly, and therefore venture to put my difficulty in this plain and outrageous way.

(*e* "Radiant heat is physically similar to radiant light, the only difference being that its wave-length is greater, and its refrangibility less, than those of light."

May I ask if it is known *how much* greater "the wave-length of radiant heat" is than "those of light"? The modern distinction between them is evidently so simple and numerical that it must be possible to definitely draw the line and to specify the exact wave-length which characterises each, or at any rate which partitions the one from the other.

Similarly it would be a help to us students to have the refrangibility of radiant heat specified and distinguished from those of light, too.

There are one or two other matters concerning which I should have been glad of further information; but I will not now trespass further upon your space or upon the good nature of the professor.

A STUDENT

IN reply to the remarks of a student I may state as follows:—

(*a*) In the kinetic theory of gases the pressure of a gas is regarded as being due to a bombardment by the molecules of the gas, and the velocity of sound in any gas can by this theory be shown to be definitely related to the velocity with which these molecules move about.

(*b*) It is no doubt true that the demonstration of "aberration" on the corpuscular theory of light is of a simpler nature than its demonstration on the undulatory theory, but I have yet to learn that the geometrical simplicity of a demonstration is always a characteristic of truth. The question is rather, Can "aberration" be shown to be a legitimate consequence of the theory of undulations quite apart from the mathematical difficulty or easiness of demonstration? If the demonstration is *valid* its *easiness* can wait.

(*c*) While admitting that our nomenclature regarding energy is of a temporary nature, I have hitherto confined the term "invisible energy" to that kind of energy the motions constituting which are on so small a scale and so rapid that they cannot by any means be rendered visible. No doubt we see a red-hot body, but we do not and cannot see the motions of the individual molecules of the hot body.

(*d*) The train of thought referred to was that which concluded that the particles of a hot body (like those of a sounding body) are in a state of vibration and (in both cases) communicate their energy of vibration to a medium which surrounds them. It is thus a question regarding energy, therefore a mechanical question, and we are thus entitled to assert that there is a definite mechanical relation of equivalence in energy between the amount of absorbed heat which leaves a hot body as it cools and the radiant energy which accompanies the act of cooling.

We have now so clear and definite a conception regarding energy that "A Student's" simile of a train and an omnibus represents the truth, and it may perhaps look a trifle ridiculous to assert such an obvious equivalence. But my remarks were partly historical, and to the physical student of a past generation the equivalence would not be equally clear. The meaning is that the radiant heat and light given out by a body when cooling, measured in any way you like and used up in any way you like, will always be mechanically equivalent to the amount of ordinary heat which the body has lost.

(*e*) Your correspondent asks how much greater the wave-length of radiant heat is than that of light. Let me refer him to a diagram which was given in a recent number of NATURE in illustration of a lecture by Prof. Langley, and which will likewise be reproduced in the course of this present series of articles.

BALFOUR STEWART

Pulsation in the Veins

THE writer of a very long and exhaustive article on "The Heart," occupying forty-one pages in Rees's "Cyclopædia," quotes, among other authorities, Bichat, who says "that the blood, when it has arrived at the veins, is no longer influenced by the heart's action; *consequently these vessels have no pulsation*" . . . "that the blood's return in the veins is involved in an obscurity;" and he propounds as a "contrast" "the fact of general pulsation in the arteries, the absence of this in the veins." The writer of the article states that "many authors, particularly Haller, considering that this [the venous] system has no agent of propulsion, have ascribed to the veins some peculiar structure" of which the evidence is insufficient; also "that there is no analogy to the course of the blood in the arteries where the action of the heart produces the whole effect," and adds: "There is much obscurity on the subject, as well as in the course of the blood in the general veins; and every judicious mind cannot fail to observe that there is a great vacuum to be filled up."

The object of this paper is to contribute to the solution of this mystery—first by proving that there *is* pulsation in the veins, and that therefore the heart's action *is* directly concerned in the return of the venous blood, and secondly, by suggesting the mode in which it is exerted.

I had observed that, on very close and careful inspection, there was in the veins (in those at the back of the hand, for instance) a visible though exceedingly delicate pulsatory undulation; but so minute that I have generally (but not always) failed to show it to others. It therefore became desirable to devise some means by which the fact might be made more manifest.

In the first instance I requested a lady, who was unable to perceive any pulsatory movement in the veins on the back of her hand, to feel and silently count her own pulse while I counted aloud the beats as indicated by what I could perceive in those veins. She was surprised to find that my counting corresponded exactly with hers, but observed that the beats of her pulse came intermediately and *alternately* with my counting. This I had not thought of or suggested. But it is, in fact, what would be the necessary result of the heart's action, and corresponds with its *alternate* contraction and dilatation.

In order to make more evident this venal pulsation the following experiment was successfully tried:—A small piece of silvered glass (about ⅝ inch by ¼ inch) was made to adhere to the surface of a swollen vein on the hand in such manner that *one* edge of the glass rested on the central ridge of the vein, while the other was in contact with the surface clear of the visible vein by the side of it. This, applied in the sunshine, of course afforded a spot of light, the movement of which reflected from the mirror, would indicate the slightest tilting of its plane by the undulating action of the vein, and the result was beautifully conclusive. The light spot vibrated in accordance with the pulse, and its vibrations were in the direction which corresponded with the tilting which should be the result of the position of the mirror in relation to the vein. Then a second mirror was applied on the *opposite* side of the vein, and the vibrations of the light spot took an *opposite* direction, which was to be expected, as the result of a tilting anticlinal to the first. A mirror placed on the knuckle (where are no veins of sensible importance) showed no sensible vibration. Thus the objection which I anticipated, that the vibration of the light spot might be caused by the general response in the limbs to the ordinary arterial pulsation, is answered by the evidence that a varied position of the mirror in respect of an individual vein was productive of a correspondingly varied motion of the light spot.

If, then, it is proved that, notwithstanding all previous authorities (to which I have had access), pulsation in the veins does exist, pulsation corresponding in rhythm with that of the arterial system, it becomes a corollary that the heart's action *does* extend to the motion of the blood in the veins, and an evident solution of the mystery of the return of the blood from the extremities appears to result. The expulsive effect of the heart's contraction is familiar, but the effect of its expansion, much in consequence of the venous pulsation having been unseen and denied, has been, as far as I know, ignored. Every one knows how an indiarubber ball syringe is filled by its expansion after compression. Apply this analogy to the expansion of the heart, and the return of the venous blood, the valves in the veins cooperating, would be equally certain. But this involves the existence of a corresponding venal pulsation, the supposed absence of which supported the theory that the direct action of the heart was limited to the arterial system.

I add some directions for the successful trial of the mirror experiment. The pulsatory motion is very small, and the action of neighbouring veins seems to cause parts of the surface to be neutral in respect of the displacement of the plane of the mirrors. It is, therefore, desirable to search experimentally for the best place for them; that is, where the resulting displacement becomes most evident; also the use of some sort of vigorous movement of the body or limbs, such as would cause a general exaggeration of the heart's action, naturally causes the vibrations of the light spot to become more conspicuous. The hand should be supported in the most steady manner, otherwise the pulsatory vibration becomes mixed up with an indefinite movement of the light spot due to general unsteadiness. The mirrors should not be more than ⅝ inch square or thereabouts. The silvering is liable to be detached from the glass by adhesion to the skin, if the glutinous substance is applied directly to the back of the mirror; to prevent this its back and edges should be covered with thin gummed paper (such as the margin of a sheet of postage stamps affords). This protects the silvering, so that the mirrors may be used repeatedly, and their position changed as often as may be required; whereas without this precaution they may be spoilt on the first application. Any sticky glutinous substance which does not dry readily (such as indiarubber dissolved in mineral naphtha) is convenient, because by its use the mirror may be with the least trouble shifted from one spot to another, in the search for a place where the venous pulsation is most visibly effective; and this will not always be found exactly where from the appearance of the veins it might be expected to occur.

J. HIPPISLEY

Stoneaston Park, Bath, August 10

The Fauna of the Sea Shore

IN the recent correspondence in NATURE on the "fauna of the sea shore," an ambiguity has arisen in the use of the term "littoral."

I, following Prof. Moseley, on whose lecture I was commenting, used the word in the extended sense of describing areas and faunas that were neither "deep-sea" (in the modern acceptation of the term) nor "pelagic." Mr. Hughes, on the other hand, has employed it in its common acceptation as descriptive of the shore area between tide marks.

The portion of the sea-bottom disturbed by waves has at present no term told off to describe it. It is not necessarily "littoral" in any sense, as that word will not cover the case of sand-banks far from the coast; such, for instance, as the banks of Newfoundland, where, according to both zoological and nautical evidence the waves act strongly on the sea-bottom. Some such term as "undal zone" might be used to describe those marine areas where the waves can sensibly affect the fauna.

The downward limit of this undal zone has not, I believe, been hitherto defined. In the case of oscillating waves (the ordinary ocean waves) 50 fathoms seems to approach the practical limit of disturbance, but, according to the evidence of marine charts, the waves appear to make themselves felt at greater depths.

In the late Mr. R. A. C. Godwin-Austen's map of the English Channel (*Q. J. G. S.*, vol. vi. p. 96), the following deposits are indicated, viz.:—

40 to 50 fathoms, fine granite shingle with fragments of *Haliotis tuberculata*.
50 to 60 fathoms fine granite shingle with fragments of *Patella vulgata*.
70 to 80 fathoms, coarse sand and gravel, with decayed *Patella vulgata*.
90 to 100 fathoms, coarse sand, fine gravel, *Cardium edule*, *Turbo littoralis*, and *Patella vulgata*.
Outside 100 fathoms, very fine shell sand, *Pecten varius*, *Cardium edule*, *Patella vulgata*, and *Turbo littoreus*.

Referring to one of these collections of shells (in upwards of 90 fathoms) between Ushant and the Little Sole Bank, the distinguished author remarks:—"Taking the two phenomena together, the occurrence of littoral shells and of marginal shingle, we may safely infer that we have at this place the indication of a coast line of no very distant geological period, buried under a great depth of water, and removed to a great distance from the nearest present coast-line."

The fact that shells are perishable owing to decay, corrosion, and the ravages of marine organisms, seems to me to militate against the probability that the shells in question are of geological antiquity; and their occurrence in connection with sand and shingle instead of mud would rather indicate the present action of currents strong enough to keep the sea-floor clean. This ordinary tidal currents cannot do, though wave and tidal currents combined can.

Under the joint influence of storm waves and storm-engendered currents, light shells may well travel down the channel bed to 40 or 50 fathom soundings. Theory and observation agree in the efficacy of wave and current to this extent. But to account for the presence of fresh littoral shells in 100 fathoms, we require the assistance of waves of sufficiently long period to affect the bottom at that depth, and to this extent theory in the case of ordinary ocean waves will not go.

In a paper submitted last year to the Dublin Society (*Proc.*, vol. iv. p. 241) I recorded observations of waves with an average period of $3\frac{1}{2}$ minutes, and suggested that these waves arose from wind pressure on the surface of the sea ; it would be interesting to know at what depth such very long, though irregular, waves would be capable of disturbing light deposits on the sea-bottom. In sheltered localities I have seen these waves attain the height of about three feet ; in exposed localities they would doubtless be higher.

ARTHUR R. HUNT

August 15

On the Terminology of the Mathematical Theory of Electricity

IN a letter (NATURE, vol. xxxii. p. 76) Mr. W. J. Ibbetson invites suggestions for a convenient abbreviation for "total or resultant pressure"; at the same time he suggests the adoption of "traction" for "intensity of tensile stress." As it seems a pity to employ two totally distinct words to express such closely related ideas as intensity of tensile stress and total tensile stress, I would suggest that, on the analogy of pressure, "*tensure*" should be introduced for "intensity of tensile stress ;" and then, on the analogy of "tension" for "total tensure," "*pression*" for "total pressure." New words are hard to grow in a language, but in this case pressure and tension might interchange their suffixes as grafts and yield two fresh useful words.

As regards physical and mathematical terminology in general, is not the time ripe for the introduction of a prefix which will modify the meaning of a term as the adjective "negative" does? *Mega* and *micro* have proved useful for multiplying and dividing by a million, but how much greater scope would there be for a prefix "ne" or "neg" for reversing the sign of a quantity. Thus negative electricity might be called "*nelectricity*," a quantity of negative electricity as so many "*necoulombs*," a negative magnetic pole as a "nepole," a negative potential as of so many "nevolts," a negative angle could be spoken as of so many "negradions" or "nedegrees," a negative temperature as of so many "nedegrees." In many cases there would be no appreciable advantage, but if there was a general understanding as to the operation of the prefix "ne" in any case, it could be used wherever it would render the phraseology less cumbersome.

Melbourne, July 10 WILLIAM SUTHERLAND

An Encysting "Myzostoma" in Milford Haven

I HAVE recently had occasion to examine a number of *Comatulæ* from Milford Haven which were kindly given to me by Mr. W. Percy Sladen, F.L.S., and appear to belong to the type that was figured by Miller under the name of *Comatula fimbriata ;* and I was surprised to find many of the pinnules presenting distinct traces of an encysting *Myzostoma*. In each of the dozen individuals the joints of one or more pinnules are abnormally developed, and in some cases they form definite cysts, which are, however, much smaller than those found on the pinnules of many *Comatulæ* and *Pentacrinidæ* from the Pacific and Oceania ; but they are obviously of the same character and due to the presence of a parasitic *Myzostoma*. According to Prof. L. von Graff eight species of encysting *Myzostoma* are at present known, but they are limited to depths of 120 to 600 fathoms in the Pacific, the Eastern Archipelago, and the Caribbean Sea, with the exception of one which was dredged by the *Hassler* in 35 fathoms off Cape Frio, Brazil.

Mr. Sladen's dredgings at Milford, therefore, have considerably extended both the bathymetrical and the geographical distribution of these encysting species. The five *Comatulæ* found in the British area have yielded six of the free-living *Myzostomas*, four of which were discovered by the *Porcupine* and *Triton ;* and we may probably take it for granted that the encysting form from Milford is another addition to the British fauna.

I propose to send all my material to my friend, Prof. von Graff, for examination ; and as there will, no doubt, be much shore-dredging carried on during the next few weeks, I would call the attention of British naturalists to the facts mentioned above, and ask them to look carefully on the pinnules of any *Comatulæ* which they may find for cysts or other enlargements of the joints.

P. HERBERT CARPENTER

Eton College, August 22

Solid Electrolytes

IN reference to Prof. S. P. Thompson's letter dated August 17 (NATURE, vol. xxxii. p. 366), may I be allowed to say that I too have observed the secondary currents which are produced by cells containing sulphides of silver and copper after being disconnected from a battery? I mentioned the fact at the meeting of the Physical Society on June 27, in a communication which will probably be printed in the *Phil. Mag.* next month. Indeed, the observation of these secondary currents preceded and led to the construction of the primary cells with solid electrolytes which I have recently described.

I should be glad to know whether Prof. Thompson can explain the curious effect produced by passing a battery-current for a moment through a cell containing a mixture of sulphide of copper and sulphur between silver electrodes. When the cell is first connected with the galvanometer the usual secondary current appears, but in a few minutes, or even seconds, this current falls to zero and is succeeded by a third, which is in the same direction as the battery-current, and generally continues for some hours.

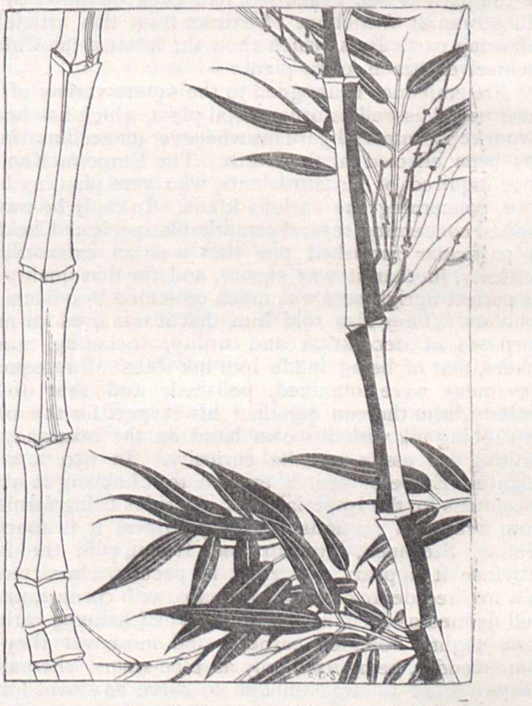
SHELFORD BIDWELL

August 23

THE SQUARE BAMBOO

THE cylindrical form of the stems of grasses is so universal a feature in the family that the report of the existence in China and Japan of a bamboo with manifestly four-angled stems, has generally been considered a myth, or, at any rate, as founded on some diseased or abnormal condition of a species having stems, when properly developed, circular in section.

Of the existence of such a bamboo there cannot, however, now be any kind of doubt. It is figured in a



Japanese book, the "Sô moku kin yô siû" (Trees and shrubs with ornamental foliage), published at Kyoto in 1829, and the figure is reproduced by Count Castillon in the *Revue Horticole* (1876, p. 32).[1] It is further figured in a work for a copy of which we are indebted to my friend Prof. Kinch (formerly of Tokiyo), called the "Ju-moku Shiri-yaku"—*i.e.* a short description of trees (of Japan). Finally, in 1880, Messrs. Veitch presented to the Kew Museum fine specimens of the stem of the square bamboo,

[1] The woodcut also appeared in the *Gardeners' Chronicle* for January 29, 1876, p. 147. I am indebted to the Editor for its use on the present occasion

brought from Japan by their intelligent collector Mr. C. Maries.

M. Carrière states, in an editorial note to Count Castillon's article, that the plant had been introduced into France at that date, and was indeed actually on sale in the nurseries near Antibes.

Nothing, however, till quite recently, appears to have been known as to the existence of the square bamboo in China, from which country, however, it is extremely probable that the Japanese procured it. Thus, Mr. F. B. Forbes, whose personal knowledge of the Chinese flora is only second, perhaps, to that of Dr. Hance, informs me:—"I have never seen the square bamboo growing, and I have always supposed that its 'squarity' was artificially produced."

The first authentic account of its occurrence in China is, as far as I know, due to Mr. Frederick S. A. Bourne, of H.B.M. Chinese Consular Service. Mr. Bourne wrote to us, October 15, 1882, that he had made a journey from Foochow to a distance of 300 miles to the western border of the Fokien province, reaching Wu-i-kung, the celebrated monastery in the Bohea hills—a place, Mr. Bourne states, "only visited by a European once before, I believe—*i.e.* by Mr. Fortune, about the year 1845." In the gardens of this monastery he found several clumps of the square bamboo growing to the height of about eight feet.

The *Tropical Agriculturist* (an astonishing repertory of everything relating to the economic botany of the East) contains in its issue for November, 1882 (p. 445), an article extracted from the *North China Herald*, also relating to the square bamboo, plants of which, destined for the park at San Francisco, had been obtained by Dr. Macgowan at Wenchow. I extract from this article the following particulars, which show the interest the Chinese themselves attach to the plant:—

"Pre-eminence is assigned to the square variety of this most useful as well as ornamental plant, which has been a favourite in Imperial gardens whenever its acclimatisation has been effected in the north. The Emperor Kao Tsu once inquired of his attendants, who were planting bamboos, concerning the various kinds. In reply he was informed concerning several remarkable species. Chekiang in particular furnished one that was an extraordinary curiosity, in that it was square, and for that quality and its perfect uprightness was much esteemed by officers and scholars. They also told him that it was used for many purposes of decoration and utility, including, among others, that of being made into ink-slabs. Subsequently specimens were obtained, polished, and sent to his majesty, who thereon signified his respect for the article by rubbing ink with his own hand on the inkstand, and inditing an essay on the curiosity. In 650 A.D. the reigning Emperor sent a eunuch to Chekiang to obtain specimens for the Imperial Park. Besides being furnished from scattered portions of this province, it is found in Honan, Szechuen, Yunnan and Hunan; in the latter province it appears to present its peculiar characteristic in a marked degree, being as square, with corners, and as well defined as if cut with a knife. The Chekiang varieties have slightly rounded corners, and moreover they are more slender, being used only as pipe-stems, whereas the western kind is large enough to serve as staves for the aged. In its early stage of growth the square bamboo is nearly round, assuming the anomalous figure it afterwards presents as it advances towards maturity. Like several other kinds of bamboo it is thorny, abounding in small spines."

Dr. Macgowan being well known to botanists for his intelligent interest in all that relates to the vegetable productions of China, I ventured to write to him to ask his aid in procuring living specimens of this interesting plant for the Royal Gardens. Through his kindness and that of Mr. E. H. Parker, late acting consul at Wenchow, we were fortunate enough to receive a Wardian case filled with plants of the square bamboo, some of which at least appeared to be alive and likely to grow. Besides these Dr. Macgowan sent us specimens of walking-sticks and pipe-stems made from it.

I quote the following passages from the very interesting communication with which Dr. Macgowan was also good enough to favour us:—

"Its geographical range is from 25° to 30° N. latitude, littoral, and westward farther than I have been able to discover. Unlike other varieties of bamboo here, its shoots are developed in the autumn, not in the spring. It sprouts in September or October, and the stems grow until they are arrested by December cold, by which time they attain a height of from two to four or five feet. In the spring following their growth recommences, when the grass attains its full height, ten to fourteen feet. The lower portion of the culms bristle with short spines; in the second or third year their squareness is far less striking than when matured by several years' growth; that quality is sometimes so marked that a native botanist describes them as appearing like rods pared by cutting instruments. I have seldom found the corners much more sharply defined than in the largest of the specimens herewith transmitted. It is cultivated chiefly for ornament in gardens, in temple courts, &c.; the larger stems (sometimes as much as an inch and a half through) are used for staves; the smaller and less squarish for stems of opium-pipes; and the smallest and less mature for tobacco-pipes."

He further adds:—"Its anomalousness is attributed by the Chinese to supernatural powers—occult agencies varying with each district. The *Ningpo Gazetteer* tells how Ko Hung, the most famous of alchemists (fourth century A.D.), thrust his chopsticks (slender bamboo rods pared square) into the ground of the spiritual monastery near that city, which, by thaumaturgical art, he caused to take root and to appear as a new variety of bamboo—square."

As no flowering specimens of the square bamboo have reached the hands of botanists, its taxonomic position must at present be regarded as doubtful. Rivière ("Les Bambous," p. 315) refers to it as the *Bambou carré*, and Fenzi, quoting from Rivière (*Bull. Soc. Tosc. di Ort.*, 1880, p. 401), gives it the name *Bambusa quadrangularis*.

I can discover no reference to it in the late General Munro's classical monograph of the *Bambusaceæ* (*Trans. Lin. Soc.*, vol. xxvi.). Of the three groups into which he divides the genera, in only one, *Triglossæ*, is there any tendency to depart from the habit of the order in having anything but round stems; and this only occurs in the small genus *Phyllostachys*, in which they are somewhat laterally flattened. The stems of *Phyllostachys nigra* are often used in Europe for walking-sticks and light broom-handles.

But I do not think the square bamboo will turn out to be a *Phyllostachys*. Munro has a *Bambusa angulata* which is distinguished from all its allies by having the branches of the panicle angular. This is the only tendency to angularity of stem which I have discovered among the true *Bambuseæ*.

For the present, at any rate, the species must be known provisionally as *Bambusa quadrangularis*, Fenzi.

W. T. THISELTON DYER

FORECASTING BY MEANS OF WEATHER CHARTS

THE Meteorological Office has issued a work on the "Principles of Forecasting by Means of Weather Charts," which has been prepared at the request of the Council by the Hon. Ralph Abercromby.[1] The object of

[1] "Principles of Forecasting by Means of Weather Charts." By the Hon. Ralph Abercromby, F.R.Met.Soc. Issued by the Authority of the Meteorological Council. Official No. 60. 8vo. Pp. 123 + viii. London: 1885.

the author has been to give an account of the modern method of forecasting weather by means of synoptic charts; and although the general principles laid down hold all over the world, the details he gives refer only to Great Britain. The whole system of synoptic forecasting depends entirely on the observed association of different sets of phenomena, and is totally independent of any theory of atmospheric circulation.

The synoptic charts prepared at the Meteorological Office are constructed in the following manner. Every day at 8 A.M. and 6 P.M. telegraphic reports are sent up to London from about fifty stations in the United Kingdom, giving the readings of the barometer and thermometer, the direction and force of the wind, and the state of the weather. These readings are then plotted on a map, and the "isobars" and "isotherms" drawn, representing lines of equal pressure and equal temperature. The isobars are the most important element in forecasting. The direction of the wind is shown by arrows which have a number of "fleches" proportional to the force, while the weather is indicated by the letters of Beaufort's notation. While the force of the wind depends on the closeness, and the direction on the trend, of the isobars, the weather is governed by the shape of the lines. Although the shape of the isobars is continually changing, several well-defined forms are always reproduced. Seven of these are described, to which the following names have been given:—1. Cyclone—an area of low pressure, bounded by circular or oval isobars; 2. Secondary cyclone—a small, circular depression, subsidiary to the cyclone; 3. V-shaped depression—an area of low pressure bounded by V-shaped isobars, something like a secondary, but differing from it in many important particulars; 4. Anticyclone—an area of high pressure bounded by circular or oval isobars; 5. Wedge-shaped isobars—an area of high pressure bounded by isobars converging to a point like a wedge; 6. Straight isobars—a barometric slope, down which the isobars lie in straight lines; and 7. *Col* or neck of low pressure lying between two adjacent anticyclones.

Cyclones.—A cyclone may be of any diameter, from 100 to 3,000 miles. The most common are between 1,000 and 2,000 miles; and anything less than 1,200 miles across is a small one. The path of a cyclone is the path described by the centre. In this country 90 per cent. move towards some point of east, the most frequent direction being about east-north-east. The velocity is that of the centre; it may be anything from 70 miles an hour eastwards to 10 miles an hour westwards. About 20 miles is a common velocity, but sometimes a cyclone is stationary. The life of a cyclone is measured by the number of days during which it can be traced on synoptic charts; the length of life may be anything from a few hours to 20 days. The details of wind, weather, &c., in the different portions of a cyclone may be briefly summarised as follows:—The temperature is always higher in the front than in the rear; the warm air in the front having a peculiar close, muggy character, quite independent of the actual reading of the thermometer. The cold air in the rear, on the contrary, has a peculiarly exhilarating feeling, also quite independent of the thermometer. The front is always very damp, especially the right-front, while the rear is dry to a marked degree. The wind blows around the centre in the direction contrary to the motion of the hands of a watch; but as the direction is slightly inclined to the isobars, on the whole the circulation is an ingoing spiral. The amount of incurvature is usually greatest in the right-front, and least in the rear; so that sometimes the passage of the trough is marked by a sudden shift of wind. The force of the wind depends almost entirely on the gradients; in the centre it is a dead calm, and the steepest gradients are usually found at some distance from the centre. The direction from the centre in which the strongest winds are found depends on the position of the surrounding areas of high pressure.

There is no difference between ordinary weather and a storm, except in that property called *intensity*, and in this country a summer breeze and winter gale are equally the product of cyclones which differ only in intensity. Hence in forecasting storms it is necessary not only to foresee the arrival of a cyclone, but of one possessing sufficient intensity to cause a gale, and in tracking cyclones it by no means follows that the same one causes a storm during every day of its existence. Observation has also shown that a deepening cyclone is increasing in intensity, while one which is filling up is decreasing. When in watching the progress of cyclones by telegraph it is very important for forecasting to note changes in depth, as well as any other indications derived from the configurations of the isobars, or even from weather prognostics, which experience has shown to be associated with intensity.

Though the general characteristics of a cyclone are invariably maintained, still, individual cases vary much in detail. The principal sources of variation which modify, but do not alter, the general characteristics are:—1, the type; 2, the intensity; 3, the size; 4, local; 5, diurnal; and 6, seasonal variation.

Secondaries.—The secondary is a small cyclone formed on the side of a larger one which is called the "primary." Secondaries are almost invariably formed either along the prolongation of the trough of a cyclone, or else on that side of the primary which adjoins the highest adjacent pressure. The most important feature about them is the manner in which they deflect the isobars of the primary, so as to leave an area of slight gradients and light winds on the side of the secondary next the primary, and a line of steeper gradients and stronger winds on the side furthest from the primary. Their motion is usually parallel to that of the primary. The most striking difference between a secondary and a primary cyclone is the great amount of rain and cloud with absence of wind developed in the former, compared with the less rain and cloud but the stronger wind developed in the latter. In a secondary when the rain comes on, it is usually very heavy and falls straight; and in its general appearance and surroundings is very different from the driving or drizzling rain, so characteristic of the front of a primary cyclone. In forecasting, the principal indication of secondaries is rain, without much wind, and thunder-storms in summer; and their sudden formation very often unexpectedly disturbs and vitiates former forecasts. Sometimes several secondaries are seen on the chart; this is a sign of great intensity, and the indication in such cases is always for wild, broken weather, often with thunder-storms, but not for widespread gales.

V-shaped Depressions.—These are generally formed along the southern prolongation of the trough of a cyclone, or in the col or furrow of low-pressure which lies between two adjacent anticyclones. Their motion is generally eastwards along with their associated cyclone, but they are very often short-lived. They are entirely non-cyclonic. The line of the trough, along which the barometer rises, marks out the line along which the weather changes very abruptly, and this change is very often accompanied by a violent squall.

Anticyclones.—Anticyclonic isobars enclose an area of high pressure which is associated with fine weather and light winds circulating in the direction of the hands of a watch, but a little outcurved. The whole system is usually stationary for many days together. The general characteristics are a cold, dry air, and fine, or at least quiet, weather. The calms, or light winds, give free play to the radiation of the season, so that very hot days occur in summer and very cold nights and fog in winter. In forecasting, the indications are for settled fine weather, the details depending much on the season and local peculiarities.

Wedge Isobars.—These consist of isobars converging almost to a point, but enclosing an area of high pressure, instead of a low one, as in the case of the V's. The wedges seem to shoot up in front of cyclones and V depressions and to travel along before them. On the front, or east side, the weather is very bright, and the wind is north-west and moderate, while the temperature is that due to excessive radiation. On the rear, or west side, where the barometer begins to fall, the wind turns to south-west, and the sky overcasts in that peculiar manner which first gives a halo, and then gradually becomes black, without true cloud as the cyclone approaches. At the extreme north point of the wedge a shower or thunderstorm is sometimes observed.

Straight Isobars.—In these the pressure is high on one side and low on the other, without any definite cyclone, the isobars running straight across the slope which joins the regions of high and low pressure. Straight isobars are never persistent, and the area which they have occupied is usually traversed by a cyclone of greater or less intensity. For forecasting purposes the indications are for cool, cloudy, unsettled weather, the wind from moderate to fresh, according to the gradients, to be followed soon by rain, as a cyclone forms or comes up.

Cols.—The col consists of a neck of low pressure between two anticyclones. The wind is always light and the weather quiet, but variable in appearance, owing to the local influence of radiation. Though the general position is sometimes nearly stationary, the weather is rather variable, owing to the tendency of the depression which lies on the north-west to develop a secondary in the col. Hence in forecasting, though it is possible to tell what the weather would be in the col at any moment, the future course of the weather is subject to much uncertainty.

Mr. Abercromby devotes a considerable portion of his work to a discussion of weather-types and sequence. With reference to Western Europe, there are at least four well-marked types of weather: 1. The Southerly, in which an anticyclone lies to the east or south-east of Great Britain, while cyclones coming in from the Atlantic either beat up against it or pass towards the north-east. 2. The Westerly, in which the tropical belt of anticyclones is found to the south of Great Britain, and the cyclones which are formed in the central Atlantic pass towards the east or north-east. 3. The Northerly, in which the Atlantic anticyclone stretches far to the west and north-west of Great Britain, roughly covering the Atlantic Ocean. In this case cyclones spring up on the north or east side, and either work around the anticyclone to the south-east, or leave it and travel rapidly towards the east. 4. The Easterly, in which an apparently non-tropical anticyclone appears in the north-east of Europe, rarely extending beyond the coast-line, while the Atlantic anticyclone is occasionally totally absent from the Bay of Biscay. The cyclones then either come in from the Atlantic and pass south-east between the Scandinavian and Atlantic anticyclones, or else, their progress being impeded, they are arrested or deflected by the anticyclone in the north-east of Europe. Sometimes they are formed to the south of the north-east anticyclone, and advance slowly towards the east, or, in very rare instances, towards the west.

Mr. Abercromby next explains the use of various aids to forecasting, and gives some detailed examples of successful and unsucessful forecasts.

In concluding his work, Mr. Abercromby gives some remarks on forecasting generally, and points out that in many cases of small disturbances the minor features are so local that it is only the general character of the weather which can ever be forecast. Owing to the rapid nature of all meteorological changes, forecasts can never be issued very long in advance. The British forecaster labours under peculiar difficulties from his geographical position. Situated on the most outlying portion of Europe, and in the very track of storms which almost always advance from the westward, he has no intimation of an approaching cyclone till it is actually on him. Mr. Abercromby's opinion is that, however carefully the relation of weather to isobars may be defined and the nature of their changes described, the judgment which experience alone can give to enable a warning to be issued must ever depend on the professional skill of the forecaster.

RADIANT LIGHT AND HEAT[1]

II.

The Theory of Exchanges

It was known at a comparatively early period that if a body be placed in an enclosure of constant and uniform temperature, it will ultimately attain the temperature of this enclosure.

To fix our ideas, let us suppose that we have a chamber surrounded on all sides by walls which are kept at the temperature of boiling water (100° C.), and let us further suppose for the sake of simplicity that there is no air in this chamber, so that no heat can be carried about by movable particles of gas. If under these circumstances we put a cold body into the chamber, it will ultimately reach 100°, at which temperature it will remain. This is a statement of the doctrine of temperature equilibrium; but this equilibrium may be of two kinds—for it may either be a statical equilibrium, in virtue of which two bodies at the same temperature cease to radiate to each other, or it may be a dynamical equilibrium, in virtue of which each of these bodies independently radiates heat to its neighbour, receiving back, however, just as much heat as it gives out. In either case the ultimate result will be equality of temperature, and the only difference is with regard to the physical machinery by which this is brought about. In the theory of statical equilibrium the behaviour of two bodies of equal temperature with respect to heat may be compared to that of a man with respect to money who is getting neither richer nor poorer, because he is neither giving away nor receiving any money, whereas in the theory of dynamical or movable equilibrium the comparison is with the man who is getting neither richer nor poorer because he is receiving back just as much money as he is giving out.

Now, we are all of us conversant with frequent examples of individuals of this latter class, but the condition of things in this world is such that we cannot have any permanent example of the former, and similar considerations might convince us that if radiant light and heat be in reality a kind of energy, the theory of a movable or dynamical equilibrium must be much more suitable to such a constitution of things than that of a statical or tensional equilibrium. Historically, however, the question of temperature equilibrium was not decided by considerations regarding energy, our conceptions of which were not then sufficiently advanced to be of much service to those who were engaged in the discussion.

As the subject is one of great theoretical and practical importance, we shall proceed to give a short account of the circumstances attending the origin and development of what is now known familiarly as the *theory of heat exchanges.* About a century ago Prof. Pictet of Geneva made the following experiment:—He took two concave metallic reflectors, and, reversing the ordinary mode of procedure, put ice or a freezing mixture in the focus of the one and a thermometer in that of the other, upon which the temperature of the thermometer was observed to fall. This effect would at once be explained if we could suppose that cold was a substantial entity capable of radiation and reflexion like heat. But it was immediately recognised that such an hypothesis is quite inadmissible, and Prof. Pierre Prevost, also of Geneva, was thus driven to propose for the explanation of this experiment the theory of a movable equilibrium of heat.

It is very evident that such a theory will explain the

[1] Continued from p. 327.

fact. For, n accordance with it, bodies of the same temperature continue to radiate heat to one another, and hence the thermometer will radiate heat to the concave reflectors, which we may suppose to be of the same temperature as itself.

This heat will ultimately in great measure be reflected upon the ice or freezing mixture. Now, had this ice been of the same temperature as the other portions of the apparatus, it would have given back to the reflectors, and through them to the thermometer exactly as many heat rays as the latter had given to it.

But the ice being of a lower temperature, does not radiate back as many rays to the thermometer as this instrument gives out to the ice, and the temperature of the thermometer falls in consequence. It will be noticed that the same laws of reflexion and arrangement of mirrors that in the case where a hot body is placed in the one focus would have heated the thermometer in the other will, in the case of a cold body, cool the thermometer in the other; so that, without resorting to the unlikely assumption that cold is a separate principle, we may explain the above experiment on the supposition that bodies of the same temperature radiate heat to one another, or, in other words, on the hypothesis of a movable equilibrium.

Prevost's first memoir was in 1791, and in 1804 Leslie published his inquiry into the nature and propagation of heat. He there demonstrated the fact that good reflectors of heat, such as metals, were bad radiators. Prevost, in a treatise on radiant heat, published in 1809, showed that Leslie's conclusions followed from his theory, remarking that in a place of uniform temperature a reflector does not alter the distribution of heat, which it would do if it possessed at the same time the power of being a good reflector and a good radiator. Prevost seems to have entertained very correct views upon this subject, inasmuch as he conjectures that a good reflector is a bad radiator because, as it reflects the heat from without, so it also reflects the heat from within. Internal radiation, we shall afterwards see, follows as a consequence from the theory of exchanges.

Some time afterwards Dulong and Petit published their well-known memoir on radiation, which affords evidence of a peculiar kind in favour of the theory of exchanges. To illustrate the bearing of the experiments by Dulong and Petit on this theory, let us imagine that we have a hollow, blackened enclosure which is at the same time a vacuum, and that we have in its centre a large thermometer likewise blackened, the temperature of which is higher than that of the enclosure. We are supposed to be engaged in observing the rate of cooling of this thermometer, or, in other words, the excess of its radiation to the enclosure above that of the enclosure to it. Now let A denote the total radiation of the thermometer, which we may imagine to have the temperature a. Also let B denote that of the enclosure, which we may imagine to have the temperature b. Then $A-B$ will, by the theory of exchanges, represent the rate of cooling of the thermometer. In the next place let the thermometer have the temperature b and radiation B, while the enclosure has the temperature c and radiation C. Here $B-C$ will, by the theory of exchanges, represent the rate of cooling of the thermometer. Finally, let a be the temperature of the thermometer, and c that of the enclosure. Then $A-C$ will, on the theory of exchanges, represent the radiation or rate of cooling of the thermometer. Now $A-C=(A-B)+(B-C)$, that is to say, the rate of cooling in the third case will represent the sum of the two preceding rates *if the theory of exchanges be true*.

It was found by Dulong and Petit that this was actually the case, for with $a=140°$ and $b=80°$, $A-B$ was found to be 2·17.

Again, with $b=80$ and $c=20$, $B-C$ was found to be 1·40.

Finally, with $a=140$ and $c=20$, $A-C$ was found to be 3·56. Now this is very nearly equal to 3·57, or the sum of the two preceding rates, so that the evidence deduced from these experiments is decidedly in favour of the theory of exchanges.

In 1848 Provostaye and Desains made a definite advance towards a clearer conception of this theory. It may be stated thus. If we place a thermometer in our hypothetical chamber of constant temperature it is well known that the instrument will give the same indication, in whatever manner we alter the substance of the walls, provided only that their temperature be left the same.

It follows from this that the heat radiated, together with that reflected from any portion of the walls, forms a constant quantity independent of the nature of the substance of which this portion is composed. We thus see that it is not correct to assert that the reflective power of a substance is inversely proportional to its radiative power, the true statement being that in the case of an enclosure of constant temperature such as that we are now considering, the sum of the heat radiated and reflected from any portion is a constant quantity.

It was likewise perceived by Provostaye and Desains that this constant sum, while equal to that of a lampblack radiator, must be unpolarised, since heat from lampblack is unpolarised; and hence that, since the reflected heat is frequently polarised, the radiated heat must be polarised in an opposite manner, that is to say, in a perpendicular plane, in order that the sum of the two should be virtually unpolarised. Experimentally these observers found this to be the case.

It will thus be seen that the inquiry had now reached a stage at which a perfectly clear conception had been formed of the character with respect to intensity and polarisation of the heat emanating from any portion of the surface of an enclosure of constant temperature.

No attempt had however been made to split up the heterogeneous body of heat into its constituent wavelengths, nor was it perceived that an extension of the argument must necessarily lead to a separate equilibrium for every individual description of heat.

Internal radiation too, as a subject for experiment (if we except the remark made by Prevost), appears to have been overlooked, and its essential connexion with the theory of exchanges does not appear to have been perceived.

In March, 1858, I communicated to the Royal Society of Edinburgh a memoir in which these desiderata were supplied. In this memoir it was shown by a simple process of reasoning that the heat-equilibrium must hold for every individual description of heat, and that as a consequence this would lead to various conclusions, all of which were experimentally verified. The following facts were thus established:—

(1) The radiating power of thin polished plates of different substances was found to vary as their absorbing power: so that the radiation of a plate of rock-salt was only 15 per cent. of the total lamp-black radiation for the same temperature.

(2) It was shown that the radiation from thick plates of diathermous substance is greater than that from thin plates, no such difference being manifested when the substances are athermanous.

(3) It was found that heat radiated by a thin diathermous plate is less transmissible through a screen of the same material than ordinary or lamp-black heat, the difference being very marked in the case of rock-salt.

(4) Lastly, heat from a thick diathermous plate is more easily transmitted through a screen of the same material than that from a thin plate.

All these facts can be explained by a legitimate extension of the theory of exchanges.

Let us recur to our hypothetical chamber, outside the walls of which we may suppose there is a boiling-water arrangement, in virtue of which these walls are kept at the temperature of 100° C. The inside we shall suppose to be a vacuum. Let us in the first place hang up in this chamber two thermometers, one covered on the outside of its bulb with lamp-black, the other with polished silver. The former of these will absorb all the rays that fall upon it from the walls of the chamber, the latter, on the other hand, will absorb very few of these rays. Ultimately, however, both thermometers will attain the temperature of the walls. Since, therefore, according to the theory of exchanges the equilibrium of temperature is kept up by an equality of absorption and radiation, it is manifest that the radiation from the lamp-black thermometer must be great, because the absorption is great, and the radiation from the silvered thermometer small, because the absorption is small.

It will be noticed that this connexion between the two qualities, absorption and radiation, is deduced from a hypothetical case where everything is at a constant temperature. To prove it experimentally we may without any breach of scientific propriety take out the two thermometers from the enclosure, exposing them to a lower temperature, and noticing their velocity of cooling, when it will be found that the blackened thermometer cools more rapidly than the silvered one.

Or we may allow their radiation to fall upon a thermopile, and to be registered by a galvanometer, when it will be found that the indication of the galvanometer will be much greater for the blackened than for the silvered thermometer.

Let us next hang up in our enclosure a plate of glass and one of polished rock-salt.

The plate of glass will absorb all or nearly all the rays of dark heat that fall upon it from the sides of the enclosure. The plate of rock-salt will, on the other hand, absorb only a few of these rays. A similar argument to that already given will enable us to see that if the theory of exchanges be true, the radiation from a plate of rock-salt must be decidedly less than from one of glass, and this is found to be the case.

Next, let us hang up two plates of rock-salt, a thick one and a thin one. The thick one will absorb more rays than the thin one, and we shall therefore expect it to radiate more. This, too, will be found to hold experimentally, thus proving the fact of internal radiation. On the other hand, we shall observe no sensible difference if we hang up two plates of glass, one thick and one thin, the reason being that the thin plate of glass already absorbs all the heat which falls upon it, so that no increment of absorption, and hence of radiation, can take place by increasing the thickness. We thus see that it is only in the case of diathermanous bodies that the radiation increases with the thickness, while for athermanous bodies there is no such increase.

We are now in a better position for realising what takes place in our hypothetical enclosure.

There is a stream of heat from the walls which falls upon any substance which we may introduce into our chamber. Now this stream is not altered in intensity by altering either the shape or substance of the walls. Suppose, for instance, that they are of polished metal instead of being covered with lamp-black, then, while the heat radiated from them will be less, the reflexion of this heat will be so bandied backwards and forwards between these walls as to swell up the total amount to an equality with the lamp-black radiation, the only difference being that in the lamp-black radiation there is little or no reflexion, while in the other there is much reflexion and comparatively little radiation. Nor will the stream from the walls be altered by hanging up a plate of any substance between them and the body we introduce. For the plate will radiate on its own account just as much heat as it absorbs from the walls, so that the joint radiation of the two will be the same as if the plate were taken away.

Our remarks have hitherto applied only to the total intensity of this stream of radiant heat, and not to its quality—that is to say we have left out of consideration the specific mixture of various kinds of rays differing either in wave-length or in polarisation which go to make up the whole heterogeneous radiation. Now a little reflection will convince us that this specific mixture—this *quality* of the radiation-stream—must, as well as its *quantity* remain the same under any change made in the shape or substance of the enclosure. For suppose that we introduce a thermometer coated with some substance which exercises a selective absorption for certain rays of the stream, and not for others, then a change of quality would mean for this thermometer a change of absorption as truly as if there were a change of quantity. But by the theory of exchanges the absorption must remain the same, being equal to the radiation, and hence this can only be brought about by the quantity and the quality of the radiation stream remaining each unaltered whatever change be made in the walls of the enclosure, or whatever substance be introduced between these walls and the thermometer. Carrying out this train of thought, we see why, as was proved by Provostaye and Desains, the sum of the radiated and reflected heat from any portion of the walls must be unpolarised, the reason being that the radiated heat from lamp-black is unpolarised and the one radiation must be equal to the other not only in quantity but in quality also. Again, the radiation of any surface or of any plate must be equal to its absorption, both as regards quantity and quality, so that the stream of heat may emerge from the surface or from the plate unaltered both in quality and in quality.

Thus the putting up of a plate between the walls and our coated thermometer will produce no effect, inasmuch as the stream of radiant heat which falls upon the coating will be unaltered both in quantity and quality by the interposition of the plate. We thus see why the radiation from a thin plate of rock salt should be of a quality which renders it much absorbed by a cold plate of the same material, the reason being that a body radiates that kind of heat which it absorbs.

We see, too, why heat from a thin plate of rock salt should be more absorbed by a cold screen of this material than that from a thick plate, inasmuch as the former consists of that kind of heat which is strongly absorbed, even by a thin plate, while the latter contains likewise a number of other rays which are not so strongly absorbed.

The conclusion to be derived from these remarks is that we have in reality a separate equilibrium for every description of heat, an equilibrium which is independent of the shape of the enclosure and of the substances of which it is composed. Furthermore, the stream of radiant heat may be supposed to circulate in the interior of a substance such as glass, water, or even metal, the radiation of each particle which it meets being exactly equal to its absorption, so that the stream proceeds through the interior, being virtually the same at one part of its path as at another. Again, it can be shown that it is essential to equilibrium that in the interior of a substance this stream of heat should be proportional to the *square of the refractive index*. That is to say, in an enclosure containing glass whose refractive index is 1·5 the stream of radiant heat in the heart of the glass will be 2·25 times greater than that proceeding through a vacuum; we cannot, however, tell what takes place in the heart of a crystal. It also appears that, for an enclosure of given temperature, the stream of a given kind of heat has a definite value, the amount of this increasing as the temperature increases.

We are, however, ignorant of the exact function of the temperature which expresses the value of this stream, but we know that this value increases more rapidly for the more refrangible rays of the spectrum than for those of greater wave-length and smaller refrangibility.

We now come to consider the luminous rays, and here the wondrous power of the eye can aid us to an extent far surpassing that of the most delicate pile and galvanometer for the dark rays.

Wollaston and Fraunhofer were the first to show that in the solar spectrum numerous dark bands occur, which indicate the absence of certain definite kinds of light.

Sir David Brewster afterwards showed that similar bands make their appearance when the spectrum is made to pass through nitrous acid gas, and it was thus rendered probable that the bands which appear in the solar spectrum were due to absorption likewise.

Brewster, J. Herschel, Talbot, Wheatstone, and W. A. Miller were amongst the first to make observations upon the luminous spectrum obtained by heating various substances, and it was soon perceived that such spectra consist of bright lines on a dark background, and thus appear to be a reversal of the solar spectrum, which consists of dark lines on a bright background. Fraunhofer was the first to notice a coincidence in spectral position between the dark double line D occurring in the solar spectrum and the bright yellow flame produced by incandescent sodium. Swan afterwards showed that the correspondence between the two black lines and the two bright lines is very exact, and that a very small quantity of salt is sufficient to call forth the bright lines. Ångström (*Phil. Mag.*, May, 1855), referring to a conjecture of Euler that a body absorbs all the series of oscillations which it can itself assume, expresses his conviction that the same body, when heated so as to become luminous, must emit the very rays which at ordinary temperatures are absorbed, and that the explanation of the dark lines in the solar spectrum embraces that of luminous lines in the electric spectrum. Probably, however, the first to give definite expression to this conception was Prof. Stokes, who, about the year 1850, commented on an experiment recently made by Foucault. This observer had found that, when a voltaic arc formed between charcoal poles was placed in the path of a beam of solar light, the double line D is thereby rendered considerably darker. If, on the other hand, the sun and the arc jut out the one beyond the other, the line D appears darker than usual in the solar light, and stands out bright in the electric spectrum. Thus the arc, remarks Foucault, presents us with a medium which emits the rays D on its own account, and which at the same time absorbs them when they come from another quarter.

The explanation given by Stokes of this experiment assumes that the vapour of sodium must possess, by its molecular structure, a tendency to vibrate in periods corresponding to the degrees of refrangibility of the double line D.

Hence the presence of sodium in a source of light must tend to originate light of that quality. On the other hand, vapour of sodium in an atmosphere around a source must have a great tendency to absorb light from the source of the precise quality in question.

In the atmosphere around the sun, therefore, there must be present vapour of sodium, which, according to the mechanical explanation thus suggested, being particularly opaque for light of that quality, prevents such of it as is emitted from the sun from penetrating to any considerable distance through the surrounding atmosphere.

It appears, from the historical sketch here given, that two independent lines of research were progressing towards the same conclusion. The one of these had for its basis the theory of exchanges, and endeavoured theoretically and experimentally to render this theory complete. The other was founded upon spectroscopic investigation, and endeavoured to apply to light an analogy deduced from sound, believing that, just as a string or tuning-fork when *at rest takes up* that *note* it gives out when *struck*, so a molecule when *cold absorbs* that *ray* which it gives out when *hot*.

In October, 1859, Prof. Kirchhoff of Heidelberg made a communication to the Berlin Academy on the subject of Fraunhofer's lines. His observations were made on this occasion by an examination of the spectrum of coloured flames made by Bunsen and himself, and he derived from them the following conclusions:—He concluded that coloured flames in the spectrum of which bright sharp lines present themselves so weaken rays of the colour of these lines, when such rays pass through the flames, that, in place of the bright lines, dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity in the spectrum of which these lines are otherwise wanting. He concluded further that the dark lines of the solar spectrum which are not evoked by the atmosphere of the earth exist in consequence of the presence in the incandescent atmosphere of the sun of those substances which in the spectrum of a flame produce bright lines in the same place.

Carrying out this train of thought, Kirchhoff, about the end of 1859, shows that as a mathematical consequence of the theory of exchanges, a definite relation must subsist between the radiating and absorbing power of bodies for individual descriptions of light and heat.

It will be noticed in this historical statement that I made my first experiments on dark heat; afterwards I proceeded to the subject of light. Meanwhile, however, Kirchhoff had independently been led to experiment in this direction, and, although his memoir slightly preceded mine in publication, I shall now give the experiments which I was led to make, more especially as they are very similar to those of Kirchhoff. In February, 1860, I communicated to the Royal Society of London a paper in which I showed that the light radiated by coloured glasses is intense, in proportion to their depth of colour, transparent glass giving out very little light. I also showed that the radiation from red glass has a greenish tint, while that from green glass has a reddish tint. It was likewise shown that polished metal gives out less light than tarnished metal and that when a piece of black and white porcelain is heated in the fire the black parts give out much more light than the white, thereby producing a curious reversal of the pattern.

Finally, in a paper communicated in May of the same year, it was shown that tourmaline, which absorbs in excess the rays of light polarised in a plane parallel to the axis of the crystal, also radiates, when heated, this kind of light in excess, but that when it is viewed against an illuminated background of the same temperature as itself, this peculiarity disappears. All these facts are a natural consequence of a movable equilibrium of temperature holding separately for every variety of heat, the word "variety" embracing any difference either in wave-length or polarisation which is the cause of unequal absorption.

The theory of exchanges, as here exhibited, has been founded upon the fact that in an enclosure of constant temperature all bodies will ultimately attain the temperature of the walls of the enclosure. This is the experimental foundation upon which our structure has been built, and we have not attempted to work under it or to find whether in its turn it be not founded upon some principle of a still deeper and more fundamental nature. We shall now briefly indicate that such is the case, and that this law of ultimate equality of temperature is a consequence of the theory of energy in which we are told that no work can possibly be got out of heat which is all at the same temperature. For if the ultimate result in our enclosure should be a variety of temperatures, then it would be possible to utilise this temperature-difference and convert heat into work, so that there would practically result a case of perpetual motion. Now, it is one of the most fundamental axioms of physical science that such a motion is impossible.

I have endeavoured to make use of this method of viewing the problem, in order to point out what forms the natural limit to our conception of a movable heat-equilibrium. Suppose, for instance, that we have a large spherical chamber of the temperature of 100° C., and that this chamber is removed from all gravitating influence, so that a solid spherical body, also of the temperature of 100° C., may rotate on its axis in the centre of this chamber without requiring the support of an axle. The chamber may likewise be supposed to be void of air, so that there is nothing but the ether to bring the revolving body to rest. Now, if a sort of diaphragm or rim be introduced into the chamber, as in Fig. 9, the result will

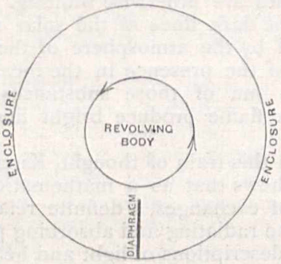


Fig. 9.

be that the particles of the enclosure to the left of the diaphragm will only receive heat from that portion of the revolving body which is approaching them, while those to the right of the diaphragm will only receive heat from those portions of the same body which are receding from them.

But the wave-length of light is altered in one way by a body which is approaching us, and in another way by a body which is receding from us, so that the particles to the left of the diaphragm will, in reality, receive a different kind of radiation from those to the right. Here, then, we have something which upsets the temperature equilibrium, and we may even conceive that the particles to the left of the diaphragm will absorb more heat, and therefore become hotter than those to the right. If so, we shall have the possibility of creating work out of this difference of temperature, or, in other words, of starting a kind of perpetual motion.

We thus begin to see that, somehow, the revolving body must lose as much energy as we gain by means of these differences of temperature, for otherwise we should have the transmutation of heat originally of the same temperature into work, which we cannot admit. But this means that a revolving body placed under these circumstances must gradually part with its energy of visible motion, although it is not in contact with anything else than the ethereal medium.

Before concluding this branch of my subject let me say a few words about phosphorescence and fluorescence.

It is well known that certain substances remain luminous—that is to say, continue to emit light for some time after they have been exposed to the light of the sun or of some other powerfully luminous body. Such substances are said to be *phosphorescent*.

It is likewise known that other substances, more especially certain liquids, emit light in a peculiar way while the luminous source acts upon them, but do not enjoy this property for an appreciable time after it has been withdrawn. Such bodies are said to be *fluorescent*.

It is manifest that the difference between phosphorescence and fluorescence is one of time, the bodies implied by the first term continuing to give out light for some time after the exciting source is withdrawn, while those implied by the second do not retain this property for an appreciable time after the withdrawal of the luminous source. Prof. Stokes, who has done much to advance this subject, has shown that the exciting cause of phosphorescence and fluorescence is more especially the rays of high refrangibility—even rays beyond the violet of the visible spectrum. On the other hand, the rays which the body gives out are generally of a lower refrangibility than the exciting rays. Hence invisible rays may, by means of a phosphorescent or fluorescent body on which they fall, render themselves visible. This phrase, however, is perhaps not strictly correct, inasmuch as, before becoming visible, they have been changed into other rays of lower refrangibility.

The object of introducing this subject here is rather, however, to discuss its bearing upon the theory of exchanges than to treat it as a separate branch of inquiry; and I may commence by remarking that at first sight it seems to contradict the general law that the quantity and quality of the light and heat given out by a body depend upon its temperature, and upon this only. Thus, a thermometer at 100° C. is supposed to radiate from the surface of its bulb heat which will be the same in quantity and quality whether the instrument has been heated by the sun's rays or by plunging it into boiling water. Now in such a body as luminous paint we have the light which we usually associate with a high temperature given out long after the sun has ceased to shine upon it, and when we know its real temperature to be that of the bodies around it. Do phosphorescent bodies form, therefore, an exception to the general law which represents the quality of the radiant heat as a function of the temperature?

I think we shall find, on examination, that in this general law it is taken for granted that no chemical change is taking place in the body in question, and no other molecular change than that implied in the cooling of the body. In a chemical action we have generally the transmutation of chemical energy into heat, and in molecular action we have generally the transmutation of molecular energy into heat likewise. That is to say, the body undergoing these changes becomes heated, and so gives out light and heat peculiar to the temperature to which it has been raised. But there seems to be no reason why molecular energy should not be somehow changed at once into radiant light and heat. In this case there would no doubt be an apparent breaking of the law above mentioned, which associates a certain temperature with a certain quantity and quality of radiant heat, but the exception would be only apparent. for, as we have stated, the law presupposes that no molecular change of this nature is taking place.

In like manner our argument regarding an enclosure of a constant temperature and the theory of exchanges in general, while it allows of the greatest possible variety of substance and form in the enclosure, virtually assumes that no chemical or molecular change is going on amongst the substances introduced. We are, in fine, supposed to be dealing with radiant energy and absorbed heat, and with no other form of energy, and indeed we have just seen that if we have a body in visible motion in the enclosure, the equilibrium no longer holds.

Thus we get rid of the difficulty by rejecting the bodies in question as not fulfilling strictly our requirements. No doubt the phenomena of phosphorescence and fluorescence are comparatively trivial exceptions, but we may imagine an enclosure in which all the substances are at the temperature of 100°, while some one substance is gradually changing its molecular state, until at length we have a violent explosion accompanied with light and heat. Here the result is so obvious that we have no hesitation in recognising such a body as an exception not contemplated by the theory of exchanges. We are persuaded that phosphorescent bodies are equally an exception, the only difference being that the character of this exception is not nearly so pronounced.

It has been pointed out by Prof. Tait that the conclusions of the theory of exchanges are only statistically true. That is to say, if we take a sensible time, such as a second, and a sensible quantity of any substance, such as a milligramme, then in an enclosure of constant temperature the absorption of that matter during one second is equal to its radiation during the same time, and this holds for all kinds of heat. On the other hand, if we take a single molecule and a billionth of a second, we cannot affirm the same equality. This is no doubt correct; in fact, if the equality between radiation and absorption were to hold for the smallest conceivable mass and the smallest conceivable increment of time, our equilibrium would in reality be a tensional one instead of being movable or dynamical. I shall conclude by repeating the words of Tait ("Heat," p. 253):—"It is vain, at least in the present state of science, to look for a truly *rigorous* investigation of the relation between radiating, absorbing, and reflecting powers. In all the professedly rigorous investigations which have been given the careful reader will detect one or more steps which are to be justified only by the statistical process of averages."

BALFOUR STEWART

(*To be continued.*)

THE LIFE OF AQUATIC ANIMALS AT HIGH PRESSURE[1]

THE magnificent expeditions of the *Talisman* and the *Travailleur* have called the attention of naturalists and physicists to the conditions of life at the bottom of the sea. A learned physiologist, Dr. Regnard, has conceived the happy idea of studying experimentally these condition of life at high pressure. With apparatus designed by M. Cailletet, he has subjected aquatic animals to enormous pressure, such as prevails in the depths of the ocean, and has examined the results when those inhabiting the surface are suddenly placed at great depths. Since his first experiments Dr. Regnard has invented an ingenious method by which he can see, notwithstanding the great pressure, what goes on inside the apparatus.

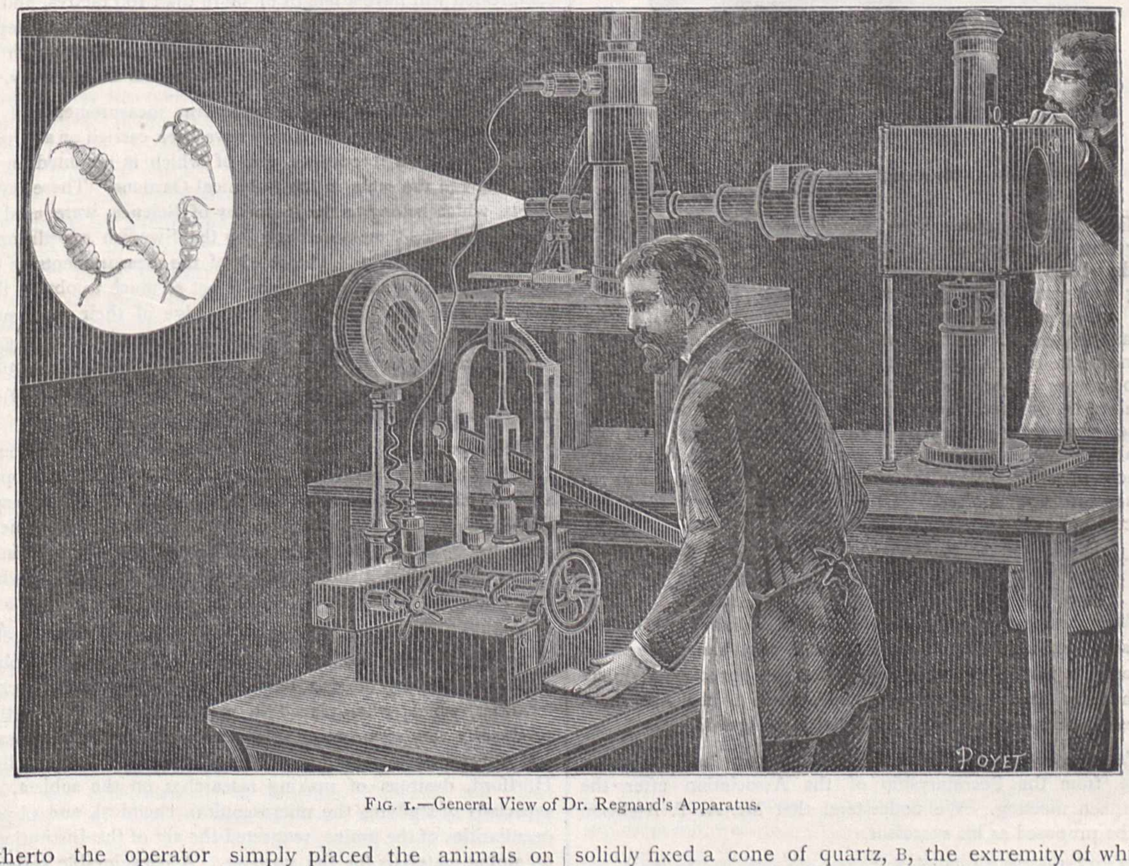


FIG. 1.—General View of Dr. Regnard's Apparatus.

Hitherto the operator simply placed the animals on which he experimented in the iron block of the Cailletet pump, and subjected them to the pressure corresponding to a given depth; he then released them, sometimes very slowly (after several days), sometimes rapidly and even instantly. He examined then, physiologically and microscopically, the lesions produced. But all the intermediate stages between the entrance of the animals and the time they were taken out escaped the observer. But now the apparatus in Fig. 1 allows him to follow each minute the effects. The following is Dr. Regnard's description of his apparatus to the Academy of Sciences:—

Two holes are pierced through and through across the lower part of the Cailletet block, M (Fig. 2). In these two holes, placed in a straight line, are inserted two tubes in *r* and *r'*. These are hollow, and in each of them is solidly fixed a cone of quartz, B, the extremity of which joins the edges of the hole which is pierced in the screw nut E. A ray of light thrown by the orifice *r* will thus traverse the apparatus and emerge at *r'*. Experiments have shown that a similar apparatus will resist easily a pressure of 650 atmospheres, which represents that of the greatest depths that have been dredged—about 6500 metres. Across one of the quartz cones are sent the concentrated rays of an electric lamp. These rays cross the block full of water, and emerge on the opposite side, where they are received by an achromatic object-glass which projects them on to a screen. The observer therefore works at a distance from the apparatus, where he is sheltered from all danger (Fig. 1). This arrangement has another advantage. The orifice pierced at *r* is hardly half a centimetre in diameter, and one can experiment with animalculæ so small as to be scarcely perceptible

[1] From *La Nature*

with the naked eye in the vessel immersed in the block M. By projecting them with a lens they are increased about 200 times, and it is even possible to see by transparence the state of their organs." In the experiment represented in Fig. 1, one of the operators is occupied in regulating the electric lamp and in setting the microscope of projection, while the other commences to apply the pressure. The animalculæ projected on the screen are the *Cyclops*, small crustaceans which are met with at this time of the year in brooks, and which are scarcely a millimetre in length. These are so enlarged, and appear with such transparency, that we can follow on the screen the movements of their branchia, and even of their heart, during the experiment. Dr. Regnard is pursuing at present his

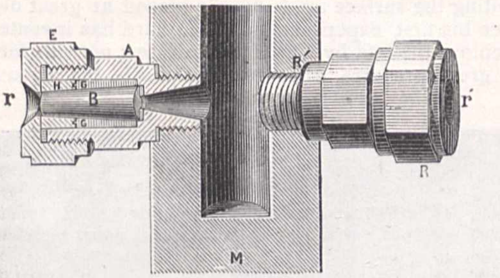


FIG. 2.—Details of apparatus in Fig. 1.

studies into life under high pressures. He showed last year that the unequal compressibility of the liquids and solids which form the organism caused the latter after a long pressure, to be soaked with water, become turgid, and consequently lose their functions. But, with the apparatus here described, he has been able to follow the phenomena which precede this. From the pressure at 1000 metres (about 200 atmospheres) the object shows inquietude, at 2000 metres it falls to the bottom of the vessel struggling; towards 4000 it remains inert and benumbed. When its normal pressure returns it recommences moving, unless the pressure has been long and its tissues are not soaked. This seems to show that the effect is a compression of the nervous system.

NOTES

WE understand that Mr. Francis Galton has already obtained valuable results from the Family Records sent him last year in response to his offer of prizes, and that he purposes to make much use of them in his Presidential address to the Anthropological Section of the British Association at Aberdeen.

WE have already intimated that Prof. Bonney has decided to retire from the Secretaryship of the Association after the Aberdeen meeting. We understand that Mr. A. T. Atchison will be proposed as his successor.

MANY interesting excursions have been arranged by the Local Committee of the Aberdeen meeting of the Association. One of them will, of course, be to the great granite quarries in the neighbourhood of Aberdeen. Her Majesty has invited 150 of the members to Balmoral, where they will be shown over the grounds and have lunch. It is not to be expected that the Queen will personally receive all the members, though it is possible that a few representative men of science may be presented to Her Majesty. Other excursions will be to Haddo House, Dunecht, Dunnottar, Drum and Crathes, Loch Kinerd, on the Saturday; while on the Wednesday and Thursday of the second week parties will be taken to Braemar, Invercauld, Haddo House, Huntly Castle, Elgin, Banff, Portsoy, and other places. The efforts which the Local Committee are making to render the meeting a success are all that could be desired. It is only to be hoped that they may succeed in persuading the Aberdeen hotel and lodging-house keepers to reduce their exorbitant charges. The arrangements for important discussions in Sections A and B we have already referred to.

IN connection with the meeting we venture to recommend to our readers the new edition of Baddeley's "Guide to Scotland," Part 1, a copy of which has been sent us. It includes all the country from the Borders to as far north as Aberdeen, Inverness, Gairloch, and Stornoway. No more useful, practical, and trustworthy guide to the region exists, while the thirty-seven admirably executed maps and plans will be found a great comfort and convenience. Dulau and Co. are the publishers.

M. JANSSEN will shortly begin a new series of experiments on the influence of gases in spectrum analysis, in continuation of those which he made about fifteen years ago at La Villette gasworks. The tubes in which the gas will be contained and compressed will have a length of more than 100 metres, and be able to bear an unusual amount of pressure. Thus a new degree of accuracy may be expected from these researches, which are progressing favourably at the Meudon Physical Observatory.

FOR more than a year some important measurements of the altitude and movements of clouds have been carried on at Upsala by the aid of two theodolites, one of which is mounted in the Linnæus and the other in the Botanical Gardens. These instruments, which belong to the Academy of Science, were used for auroral and cloud measurements by the Swedish expedition to Spitzbergen, 1882–83. The object of the measurements of the altitude and movements of clouds is not so much to obtain their mean altitude as to derive some knowledge of their movements in the upper part of the atmosphere, a matter which is of great importance to meteorology. The researches have advanced so far that it has been found possible to fix astronomically the movements and altitude of the cirrus clouds.

ACCORDING to the *Tägliche Rundschau* the population of Ratisbon has been greatly frightened by the sudden disappearance recently of thousands of jackdaws, which dwelt in the spire of the cathedral of the town, on account of a similar phenomenon occurring before the outbreak of the last cholera epidemic in the place. In Munich a similar phenomenon is also stated to have taken place.

REFERRING to "sonorous sand," the report of the secretary of the Smithsonian Institution says that an interesting problem to physicists and geologists has been the sand found in certain localities, which, when placed in motion by sliding, sometimes produces a very sonorous or resonant sound, peculiar in character and difficult of explanation. Prof. Bolton, of Trinity College, Hartford, desirous of making researches on the subject, and especially of studying the microscopical, chemical, and physical peculiarities of the grains, requested the aid of the Institution in obtaining materials for the purpose. A considerable variety of specimens was collected in the Sandwich Islands, the coast of Oregon, Germany, and many other places. These are now in Prof. Bolton's hands, and he will prepare a report on the subject.

THE Chesapeake Zoological Laboratory, as the marine station maintained by the Johns Hopkins University is designated, is *Science* states, established for the present summer session at Beaufort, on the coast of North Carolina. Dr. W. K. Brooks, the director, who was prevented last year by ill-health from giving as much time as usual to the laboratory, is fortunately quite restored to his usual strength, and is in full activity at his post. Twelve collaborators are with him. Several of these are already teachers in various branches of zoological science, and all of them are well prepared to make use of the opportunities which are afforded at this station. An unusual number are engaged in original researches. The season of 1885, although uncomfortably hot, has thus far been exceptionally favourable for collection. The weather has been calmer than heretofore in June and July, and specimens were found in June which have usually not appeared until the middle of August. The company, notwithstanding their personal discomfort from the heat, have maintained their full enthusiasm in the work upon which they are engaged; and it now appears as if the eighth session of the laboratory would be more fruitful in results than its predecessors, good as they have been.

A DUNFERMLINE correspondent writes to us that one of the most important and certainly the most complete cemetery of the Stone Age which has been laid bare in recent times has just been discovered in the grounds of Pitreavie, Dunfermline, Fifeshire. In connection with rebuilding operations a sand-pit was opened, and here, in a space of 15 yards by 10 yards, no fewer than five cists have been discovered. The cists were constructed of rough sandstone flags, and four of these measured about 42 inches in length, 20 inches in breadth, and 16 inches in depth. The fifth was little more than 18 inches square. A cinerary urn of baked clay was found in each of the large cists, but in the small "grove" nothing was found but a quantity of apparently calcined bones. A couple of flint scrapers and a bottle-shaped piece of limestone—which may have done duty as a hammer—were also among the finds. The urns measure from 5 to 6 inches across the mouth and from 4½ to 6 inches in height, and, strange to say, the construction of the bowls indicate that they have been made at different successive periods. No. 1 urn is an unshapely piece of sun-dried pottery; No. 2 showed an advance in the shape; and Nos. 3 and 4 are neatly formed and ornamented with a simple dotted pattern. The explorations will be continued, and it is expected that several other important finds will be made. Dr. Munro, the author of "Ancient Scottish Lake Dwellings," has visited the tumuli with a view to place a report in the hands of the Antiquarian Society of Scotland. A tradition exists that the site of the mound was an old graveyard, and some people who have been engaged in the district in agricultural pursuits for the past half a century state that numerous flagstones and pieces of urns have been turned up by the plough or grubbed, and Dr. Munro attaches great importance to the flint scrapers, and was of opinion that the bones found in the small cist were human bones.

AT the recent Railway Congress at Brussels the question whether it would be economical and desirable to use iron or steel instead of wooden sleepers was fully discussed. It was stated that metal sleepers of various patterns are being used in Holland and India to a considerable extent, and that they are being tried experimentally in Belgium, England, and other countries. An opinion was expressed that sleepers of the description which is being tried in England would afford good material support for the rails on main lines, although some inconvenience might be felt from a quoin of wood being used with it. It was also considered that other metal sleepers which are being tried in Holland and elsewhere had given satisfactory results. The cost of metal sleepers is higher than that of wood. They require good ballast, and there had not been sufficient experience from their use, in regard to their duration and maintenance, to enable the section to state specifically the relative advantages of the new description of sleepers. It was therefore considered that further experience is necessary. The difficulty of arriving at a conclusion as to what would be applicable in all countries and under all circumstances was exemplified in the discussion of this subject by the representative of the Egyptian railways. He stated that iron or steel sleepers cannot be economically used in Egypt, because they become corroded by the sand. The representative of the Indian railways, on the other hand, informed the section that iron or steel sleepers only can be used in India, because the white ant destroys wooden sleepers. Considerable discussion took place as to the construction of railways in regard to the curves, gradients, and works generally, including the question whether lines with a comparatively small traffic should be laid with heavy or light rails. It was, however, found impossible to lay down any general propositions which could be adopted under all the circumstances in which railways have to be made.

IT may be remarked that François Arago was born at Estagel in the beginning of February, 1786, so that a centennial celebration may be expected next year. A statue was erected in this place twenty-nine years ago at the expense of the late M. Pereire.

AN exhibition of labour was opened a few weeks ago at the Palais de l'Industrie, Paris. An electrical railway with a single rail was exhibited by M. Lartigue, and is carrying passengers with regularity on a zigzag line of about 200 metres' length. A series of popular exhibitions with magic lanterns on the new features of microscopy is largely attracting public attention. So-called antediluvian music is played on a series of irregular stones which have been selected so that they represent two octaves when suspended by strings.

THE American Ornithologists' Union will hold its next meeting in New York on Tuesday, November 17.

WE have received catalogues of electrical apparatus from two new firms: the first of these is the Kinetic Engineering Company, who are agents in this country for the well-known firm of Breguet. They are now exhibiting Lippmann's ingenious mercurial galvanometer. The second catalogue is that of Messrs. P. Jolin and Co., of Bristol. This enterprising firm describes several instruments of great use in the physical laboratory, especially the dead-beat galvanometer of D'Arsonval's type, and adjuncts therefore. This instrument appears to be specially adapted for private laboratories. We are glad to see new firms taking such good standing in the character of the apparatus they offer to the scientific world.

THE Java newspapers report that volcanic activity in the island continues to increase. Another mountain, called Raun, broke out on June 21, casting out much steam and ashes. In the evening smoke was ejected in such quantities as to darken the horizon on the windward side, until a shower of ashes fell, upon which the sky cleared up. Raun appears to be an active volcano, but no such violent eruption has been known in recent years. On the night of July 8 a new eruption of Mount Smeru took place; it was a heavy explosion followed by a stream of red-hot lava, which came down to the same spot which was laid waste by the former eruption. In the evening of July 9 another explosion followed.

"RESULTS of Twenty Years' Observations on Botany, Entomology, Ornithology, and Meteorology, taken at Marlborough College, 1865-84," is the title of a large pamphlet embracing a summary of twenty years' work. The tables are accumulations of facts properly registered. In the botanical notices the first appearances in each year are given, the day being noted as the day of the *year*, not of the month. This method is readiest for comparison and for striking the average. In addition the average for the twenty years, the earliest and latest days, the amplitude and the number of observations are given. The entomological notices are arranged in the same way, except that the earliest and latest appearances and the amplitude are omitted; these are not a great loss, for they can be ascertained from the tables in a moment by any reader. In ornithology the observations include the date when first seen, and when an egg and the young have been found. The meteorological notices include for each month of each year the highest, lowest, and mean readings of the barometer, the maximum and minimum temperature in the shade, the number of times the thermometer stood above certain points varying with the seasons of the year, the maximum in the sun, the minimum on the grass, amount of rain collected, and the number of rainy days. The wettest year of the twenty was 1882, when the rainfall was 43·79 inches; the dryest, 1870, with 23·41 inches. The weather records in these tables have been kept by one observer, with properly verified instruments, and all the observations have been critically examined at the Royal Meteorological Society; the botanical notices, though obtained by a large staff of observers, have all been recorded by one person, who saw all the specimens; but entomological and ornithological notes were taken by a series of recorders, and there is therefore not the same uniformity as in the two previous cases.

WE have received the annual report of the West Kent Natural History, Microscopical, and Photographic Society for the past year. It contains abstracts of several papers read during the year. It is a pity there is no abstract of the discussion introduced by the president at the annual dinner at Gravesend, on "Bacon and Beans." There are two papers on subjects connected with photography.

MR. W. F. STANLEY has recently brought out a new form of protractor and goniometer, which has the special merit of measuring an angle right up to the vertex. This new form of protractor will be very convenient to civil engineers in measuring angles upon ordnance maps which are most frequently subtended by short lines, and many other cases. Used as a goniometer, it will be very convenient to measure the angles of large crystals and planes of cleavage, also to draw the same direct from the instrument. The instrument consists of two concentric circles, the outer one carrying the graduation, the inner a Vernier; each supports an arm with an edge extending to the centre. The angles are measured by slipping the inner circle with its attached arm and Vernier round the groove on the outer circle, which keeps it in position. We believe the instrument has all the good points which Mr. Stanley claims for it, and it will be useful to artists as well in determining angles of perspective.

THE whitefish (*Coregonus albus*) now in the ponds at the Delaford Fishery are growing rapidly, some of them reaching seven inches in length. It will be remembered that the ova of these fish were brought from America last spring, and hatched out at South Kensington.

A REMNANT of the great forests which once covered the south of Sweden was recently dug out of a bog at Kiuneved, consisting of a boat 6 feet in diameter hollowed out of a log. The tree from which it was obtained must have been 20 feet in circumference. The wood, which was blue in colour, was very hard, and the boat so heavy that two bullocks could not move it.

MR. HENRY PHILLIPS, jun, one of the secretaries to the American Philosophical Society, has performed a very useful work in compiling a register of all the papers published in the *Transactions* and *Proceedings* of the Society since its commencement. The "register" forms a small pamphlet of fifty-six pages, the titles being arranged according to the authors' names. It is therefore an index to all the publications of the Society—but a name, not a subject, index.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mr. E. Pelditch; a Bosmani Potto (*Perodicticus potto* ♂) from West Africa, presented by Mr. C. R. Williams; two Gerbilles (*Gerbillus* ——) from Suakim, presented by Surgeon-Major J. A. Shaw; two White-faced Tree Ducks (*Dendrocygna viduata*) from West Africa, presented by Mr. Cecil Dudley; three Green Turtles (*Chelone viridis*) from the West Indies, presented by M. C. Angel, F.Z.S.; a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Mr. J. C. O'Halloran; two Narrow-barred Finches (*Munia nisoria*) from Java, an Indian Silver Bill (*Munia malabarica*) from India, an Amaduvade Finch (*Estrelda amandava*) from India, presented by Mr. Horace Sanders; a Short-toed Eagle (*Circaetus gallicus*) from Southern Europe, presented by Mr. Henry Sotheran; a Mona Monkey (*Cercopithecus mona* ♂) from West Africa, presented by Mr. White; a White-necked Crow (*Corvus scapulatus*) from West Africa, deposited; nine Gold Pheasants (*Thaumalea picta*), received from the Right Hon. George Sclater-Booth, M.P.; a Barred-shouldered Dove (*Geopelia humeralis*), a Coquerel's Lemur (*Chirogaleus coquereli*), a Collared Fruit Bat (*Cynonycteris collaris*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE BINARY-STAR 70 OPHIUCHI.—Notwithstanding the care with which the orbit of this double-star has been discussed, the companion appears to be again deviating from its predicted position to a considerable extent. It will be remembered that from the anomalous motion of the smaller star Madler was led to the suspicion that the law of gravitation does not apply in this system, while Jacob thought there was indication of disturbance from a third body.

M. Perrotin gives the following epoch resulting from his measures made at Nice in 1883:

1883·49 ... Position 45°·6 ... Distance 2″·28

On comparing with the orbit assigned in No. 1 of "Astronomical Observations made at the University Observatory, Oxford," which accords closely with the measures up to 1878, and with the orbits Flammarion, Tisserand, and Schur, we find the following differences taken in the order, observation—calculation:—

	Position.	Distance.
The Oxford orbit	− 9°·9	−0″·60
Flammarion	−12·8	−0·18
Tisserand	−13·5	−0·57
Schur	−17·4	−0·73

It is very possible that in this case the difficulty of representing the position of the companion-star may be attributed to the paucity of measures near the peri-astron, rather than to an anomalous motion which has not been remarked in most of the other binaries. However this may be, the object no doubt is one deserving of continued attention. The Oxford orbit, which, it will be seen, is the nearest as regards the position angle in 1883, gives for 1885·5—position, 44°·6; distance, 2″·64.

TUTTLE'S COMET.—On September 10, at midnight, this comet will be in about R.A. 136° 33′, Decl. +3° 48′, rising at Greenwich two hours before the sun, and with an intensity of light one-third greater than when first observed at Nice on August 8. It may perhaps be observed after perihelion in the southern hemisphere if the more powerful telescopes are utilised. On August 13 the correction to Herr Raht's ephemeris was −13s. in right ascension and +5′·5 in declination. The comet is about 2′ in diameter, without very apparent central condensation.

THE COMET OF 1652.—At present we have only one calculation of the orbit of this comet—that of Halley, founded upon the observations of Hevelius in the scarce volume of the "Machina Cœlestis." It would be interesting to investigate the orbit anew from the observations made by Richard White at Rome, though he gives no nearer time for his distances of the comet from stars between December 21, 1652, and January 3, 1652, than "hora 2 post occasum solis." The observations will be found in *Zeitschrift für Astronomie*, vol. iv., where they are entitled "Observationes Cometæ, qui exeunte anno 1652 comparuit, habitæ Romæ per Riccardum Albium, Anglum." Zach supposed the observer to be Richard White, and there can be little doubt that he is the Mr. White repeatedly mentioned by Evelyn in his Diary. Zach has the remark, "Diese Beobachtungen können leicht besser als die des Hevelius seyn," and an examination of the latter will show that there is some foundation for this remark. On December 21, according to Halley's elements, the distance of the comet from the earth was only 0·14; on January 3 it had increased to 0·42.

The fact that the place of the ascending node of the comet of 1698, as it is printed in Halley's "Synopsis of Cometary Astronomy," is 180° in error, or, in other words, the place of the descending node has been given for that of the opposite one, furnishes a hint that it is not safe to accept a single calculation of the orbit of any of the earlier-computed comets without examination.

ASTRONOMICAL PHENOMENA FOR THE WEEK, 1885, *AUGUST* 30 *TO SEPTEMBER* 5

(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 August 30

Sun rises, 5h. 11m.; souths, 12h. 0m. 23·0s.; sets, 18h. 49m.; decl. on meridian, 8° 52′ N.: Sidereal Time at Sunset, 17h. 26m.

Moon (at Last Quarter on Sept. 2) rises, 20h. 28m.*; souths, 3h. 15m.; sets, 10h. 12m.; decl. on meridian, 8° 11′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	6 1	12 17	18 33	2 28 N.
Venus	8 7	13 57	19 47	2 47 S.
Mars	0 36	8 48	17 0	22 50 N.
Jupiter	5 48	12 28	19 8	7 6 N.
Saturn	23 43*	7 52	16 1	22 25 N.

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

Occultations of Stars by the Moon

Sept.	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
1	θ^2 Tauri	4½	22 1	22 52	62 247
1	θ^1 Tauri	4½	22 2	22 51	82 227
1	B.A.C. 1391	5	23 1	23 32	117 189
1	81 Tauri	5½	23 9	near approach	333 —
1	85 Tauri	6	23 21	0 1†	20 284
2	Aldebaran	1	1 40	near approach	154 —
2	117 Tauri	6	22 34	23 20	84 221
3	B.A.C. 1728	6	0 13	0 48	9 288
4	26 Geminorum	5½	4 57	near approach	146 —
5	68 Geminorum	5½	0 58	near approach	323 —

† Occurs on the following day.

The Occultations of Stars are such as are visible at Greenwich.

Sept.	h.	
2	18	Mercury in inferior conjunction with the Sun.
4	3	Saturn in conjunction with and 4° 17′ north of the Moon.
5	7	Mars in conjunction with and 5° 33′ north of the Moon.

GEOGRAPHICAL NOTES

Sad news has been received from the Dutch African Expedition; its leader, Mr. D. D. Veth, died from disease on May 19, in the camp on the banks of the Kala-Kanga River, between Benguella and Humpata. This is a real loss for science as well as to his venerable father, Prof. P. J. Veth, who has given his whole industrious life to scientific work.

The Austrian Government, with the consent of the Porte, has undertaken to make a geographical survey of the Albanian coast, with a view to preparing new maps. Two Austrian gunboats have accordingly left for Corfu with officials of the Chart Department on board. Here they will be joined by the Turkish officers, under whose superintendence the survey will be made.

It is stated in the latest *Ergänzungsheft* to *Petermann's Mittheilungen*, that there are in Peking four institutions at which astronomical and meteorological observations have been made for a number of years: (1) the Chinese Observatory, called *Kuan sang tai*, which has existed for about six centuries. In 1674 the Jesuits provided it with new astronomical instruments, without lenses, which are well preserved to this day. It is situated on the eastern wall of the Manchu town. (2) Bethang, or the Northern Church, the *Collegium Gallorum*, near the Imperial palace. Here in the middle of the eighteenth century the Jesuits erected an observatory, and made many astronomical observations, amongst them the transit of Venus of June 3, 1769. Besides these Père Amiot made meteorological observations for six years, from 1757 to 1762. (3) The Russian Legation, near the southern wall of the Manchu town. The astronomer Fuss, who made a great journey between 1830 and 1832 from St. Petersburg to Eastern Siberia, and by Kiachta to Peking, at the orders of the Academy of Sciences of St. Petersburg, spent seven months here, and organised astronomical, geographical, magnetic, and meteorological observations. (4) Beguan, about 300 metres from the north-eastern corner of the wall surrounding the Manchu city. Here the members of the Russian missionary body, and the native Christians under their direction, carried out a series of magnetic and meteorological observations between 1841 and 1860. In 1864 this Observatory was separated from the missionary establishment, and in 1867 the St. Petersburg Academy of Sciences selected Dr. H. Fritsche for its director, a position which he held for sixteen years. For twelve of these he lived in Peking, while the other four were spent for the most part in journeying through the Chinese Empire and Siberia, in order to inspect the meteorological stations and the three magnetic observatories at Ekaterinburg, Barnaul, and Nerchinsk, to establish new stations, and specially to obtain astronomical, geographical, and hypsometric observations in as large a number of places as possible. His investigations into the meteorology of Eastern Asia were published by the Academy in 1877, and he now publishes in the *Ergänzungsheft* above alluded to the results of his sixteen years' observations in other departments. He describes his numerous journeys in China, Mongolia, and Manchuria, and gives a mass of data with regard to the latitude and longitude of places, and their heights above the sea-level. There are also, in the second part of the paper, a large number of measurements connected with earth magnetism. The title of the paper, which is a long one, and represents a vast amount of travel and labour, is "Ein Beitrag zur Geographie und Lehre vom Erdmagnetismus Asiens und Europas," von Dr. H. Fritsche, *Petermann's Mittheilungen Ergänzungsheft*, No. 78.

In the current number of *Petermann's Mittheilungen* the principal article is an account, historical and geographical, of "a lava desert in the interior of Iceland," and the largest lava area in Europe. The "desert" in question is situated in that part of the plateau in the interior which lies between the Vatnajökull and the rivers Skjálfandafljót and Jökulsá. It is known to the inhabitants of the neighbouring coasts as Odádahraun. The author, Th. Thoroddsen, describes his journey from Myvátn in detail.—Prof. Nell explains Fischer's perspective projection for maps, and gives a map of Asia on this system; while Herr Flegel describes his journey in 1879 with the Henry Venn expedition up the Pico Grande from the Cameroons.

The *Zeitschrift* of the Gesellschaft für Erdkunde at Berlin (Band 20, Heft 3) is almost wholly occupied with an account by Herr Schmidt of the travels of the friar Rubruk between 1253 and 1255 into the heart of Central Asia, and to the borders of China. This remarkable journey is described and explained with much painstaking learning. The only other contribution to the number is a table of lengths of the principal Russian rivers from General Tillo's survey.

From the latest reports the Australian New Guinea expedition appears to have progressed satisfactorily so far. The Government of Queensland had offered to hold frequent communication with the party by means of the steamer *A lvance*, with a view of obtaining information of the progress of the work of exploration. A branch of the Geographical Society of Australasia is to be formed at Brisbane.

A Parliamentary blue-book (Corea, No. 3, 1885) lately published contains the report of a journey made by Mr. Carles, the Vice-Consul at Seoul, from that place to Phyöng Kang, where some gold mines exist. These lie to the west of the main road between Seoul and Gensan, and were stated to be of greater extent than any existing in Corea. They are in the Phyöng Kang district, in the neighbourhood of the town of Pai-namou-tjang, about 100 miles from the capital. Part of the road lay across a vast lava-field, which appears to exceed in extent even the largest in Iceland. Between Chhöl-wön and Pai-namou-tjang, a distance of 40 miles, there is only one break in its bed, which Mr. Carles attributes to the action of the stream which flows near Phyöng Kang; the uniform depth of the lava is about 100 to 140 feet, and it has a continuous and gradual ascent towards the north. Local statements as to its extent beyond Pai-namou-tjang were vague, but the plain could be seen stretching 13 miles farther up the divide of the eastern and western watersheds. Twenty miles north of this divide Mr. Carles left a similar plane last year, stretching from Anbyön to Kosan, but nearly 1000 feet below the level of the present plain. There are thus three great oval fields of lava passing almost in a straight line through the mountain chain which runs from the north to the south of Corea at a height of about 1500 feet above the sea near the divide, and of 500 feet in the lower levels. There is also another plain about 4 miles wide and 12 miles long to the east in Keum-söng district, the direction of which is not so well defined, but in which the depth of lava is apparently even greater than that in the others. No crater is visible in any direction to account for the enormous mass of lava; no hot springs were heard of within 30 miles, and sulphur is said to be imported from China, so that the gigantic overflow would appear to have taken place in the valley, and to have completely buried the volcano from which it came, if such were its source. At the first gold-washing visited about 270 men were said to be employed. Trenches were being dug in a bed of shingle by the river-side, and being driven parallel with the course of the stream. The men worked in parties of six, with one washer, who managed his wooden bowl very cleverly. Only small particles of gold are found, but the results seemed uniform and far superior to those of any other place visited by Mr. Carles in Corea. At two different washings which he witnessed, and which were said to give about the average yield, three basins of good earth, representing less than an hour's labour of six men, produced about fifteen pieces of gold—small indeed, but clearly visible at three yards' distance. Farther up the valley, where the men were working in smaller gangs, the yield was about the same in proportion to the number of men. On the western slope are other workings, where some 300 men are engaged, but these do not appear to be so productive. It appears that this valley has never before been worked for gold; in other places it has been sought for ages, and always found after the summer floods had brought down fresh detritus; but here the shingle seemed never to have been disturbed, or, rather, arranged in walls, before. The country here also seemed more promising than elsewhere, and to be worth the visit of an experienced miner.

FOR many years it was believed that the highest mountain in Sweden was Sulitjelma, on the frontier between Sweden and Norway, the height of which is about 6000 feet. A couple of years ago it was, however, discovered that the mountain of Sarjektjåkko, in Swedish Lappland, was a thousand feet higher. Lately, Dr. Svenonius, well known for his explorations of this province, has declared that neither of these mountains is the highest in Sweden, the honour belonging to Kebnekaisse, another peak in the same province, which the topographical surveyor of the province of Norrland has measured and found to be 7192 feet in height.

ACCORDING to recent advices from the Faroe Islands, a well-known landmark has disappeared there, viz. the rock called "the Monk," situated about five miles south of Sumbö. Its height was nearly 100 feet. On the top of it lay some large boulders, which could be seen distinctly. Already last year part of the top fell down, but the body remained until last winter or this spring, when its disappearance was discovered.

MINERAL PRODUCTS OF THE UNITED STATES

THE second Report on "The Mineral Resources of the United States," by Albert Williams, jun., Chief of the Division of Mining Statistics and Technology, United States Geological Survey, is now in press and will be issued shortly. This Report is for the calendar years 1883 and 1884, and contains detailed statistics for these periods and also for preceding years, together with much descriptive and technical matter. The following are the totals of the production of the more important mineral substances in 1884:—

Coal.—The only statistics in which the trade is interested are those relating to the amount of coal which is mined for and reaches the market. There is, besides, a local and colliery consumption which is usually disregarded in statistics, and which ranges from 5 to 6½ per cent. of the total shipments. Of what may be called the commercial product the quantities in 1884 were as follows:—Pennsylvania anthracite, 30,718,293 long tons; bituminous and brown coal, lignite, and small lots of anthracite mined elsewhere than in Pennsylvania, 66,875,772 long tons; total, 97,594,065 long tons. The spot value of the commercial product was: Pennsylvania anthracite, $61,436,586; bituminous and all other coals, $70,219,561; total, $131,656,147. Including the local consumption, &c., the total product in 1884 may be stated at 106,906,295 long tons—namely, 33,175,756 long tons of Pennsylvania anthracite and 73,730,539 long tons of bituminous and all other coals; and the value at the mines was: Pennsylvania anthracite, $66,351,512; bituminous and all other coals, $77,417,066; total, $143,768,578. The total production (that is, including colliery and local consumption) of anthracite was 1,160,713 long tons less than in 1883, while its value was $10,905,543 less, the disproportionate decline in value being due to a fall of 25 cents. per ton in spot price ($2·25 to $2). The total bituminous coal production increased 5,199,039 long tons over that of 1883, but its value was $4,820,734 less, the average valuation at the collieries having fallen from $1·20 to $1·05. The total output of all coals showed a net gain in tonnage of 4,038,326 long tons, and a decline in value of $15,726,277.

Coke.—There were 4,873,805 short tons of coke made in 1884, worth $7,242,878 at the ovens. This production consumed 7,951,974 short tons of coal. The amount of coke made was 590,916 tons less than in 1883, and the value was $878,729 less.

Petroleum.—The production of crude petroleum in 1884 was 24,089,758 barrels of 42 gallons each, of which the Pennsylvania and New York oil-fields produced 23,622,758 barrels. The total value, at an average spot price of 85 cents, was $20,476,294. As compared with 1883 the production was 689,529 barrels greater; but the total value was $5,263,958 less, the average sp t price having fallen from $1·10, or 25 cents per barrel.

Natural Gas.—The estimated value of the natural gas used in the United States in 1884 was $1,460,000, as against $475,000 in 1883. The value is computed from that of the coal superseded by natural gas.

Iron.—The principal statistics for 1884 are as follows:—Iron ore mined, 8,200,000 long tons; value at mine, $22,550,000. Domestic iron ore consumed, 7,718,129 long tons; value at mine, $21,224,854. Imported iron ore consumed, 487,820 long tons; total iron ore consumed, 8,125,949 long tons. Pig iron made, 4,097,868 long tons—a decrease of 497,642 tons as compared with 1883; value at furnace, $73,761,624, or $18,148,576 less than in 1883. Total spot value of all iron and steel in the first stage of manufacture, excluding all duplications, $107,000,000, a decline of $35,000,000 from 1883. Fuel consumed in all iron and steel works, including blast furnaces, 1,973,305 long tons of anthracite, 4,226,986 long tons of bituminous coal, 3,833,170 long tons of coke, and 62,110,660 bushels of charcoal, besides a notable quantity of natural gas. Limestone used as flux, 3,401,930 long tons; value at quarry, $1,700,965.

Gold and Silver.—The mint authorities estimate the production in 1884 at $30,800,000 gold and $48,800,000 silver (coining rate); total, $79,600,000. This was an increase of $800,000 gold and $2,600,000 silver as compared with 1883. The gold production was equivalent to 1,489,949 troy ounces, and the silver to 37,744,605 troy ounces.

Copper.—The production in 1884, including 2,858,754 pounds made from imported pyrites, was 145,221,934 pounds, worth $17,789,687, at an average price of 12¼ cents per pound in New York City. The amount was 28,070,139 pounds greater than the production of 1883; but the value was $275,120 less than that for 1883, owing to the decline in price. In 1884 4,224,000 pounds of bluestone (sulphate of copper, "blue vitriol") were made; worth, at 4·3 cents per pound, $181,632.

Lead.—Production, 139,897 short tons. Total value, at an average price of $75·32 per ton on the Atlantic sea-board, $10,537,042. The production was 4060 tons less than that of 1883, while the decrease in value was $1,785,677. The production of white lead (carbonate) is estimated at about 65,000 short tons, worth, at 4⅞ cents per pound, $6,337,500, almost all of which was made from pig lead. The production of litharge and red lead has not been ascertained.

Zinc.—Production of metallic zinc, 38,544 short tons; worth, at an average price of 4·44 cents per pound in New York City, $3,422,707. The output was 1672 tons greater than in 1883, and the value increased $111,601. Besides the spelter and sheet zinc, about 13,000 short tons of zinc white (oxide) were made directly from the ore, the total value of which, at 3½ cents per pound, was $910,000.

Quicksilver.—Production, 31,913 flasks (of 76½ pounds net = 2,441,344 pounds), or 14,812 flasks less than in 1883. Total value, at an average price of $29·34 per flask at San Francisco, $936,327, a decline of $317,305 as compared with the total value of the product of the previous year. During the year 600,000 pounds of quicksilver vermilion were made, worth $288,000.

Nickel.—Production of nickel contained in copper-nickel alloy, 64,550 pounds, worth, at 75 cents per pound, $48,412; an increase of 5,750 pounds, but a decline of $4508 in total value, owing to the falling off in price.

Cobalt.—The amount of cobalt oxide made in 1884 was about 2000 pounds, as against 1096 pounds made in 1883. Its value, as $2·55 per pound was $5100. The value of cobalt ore and matte cannot be ascertained, as it is chiefly dependent on the nickel contents.

Manganese.—The output of manganese ore in 1884 was about 10,000 long tons, or 2000 tons more than in 1883. The total value, at $12 per ton at the mines, was $120,000, or about the same as in 1883, the average price having declined $3 per ton.

Chromium.—The production of chrome iron ore, all from California, was about 2000 long tons, or about two-thirds as much as in 1883. At an average value of 17·50 per ton at San Francisco, the total value was $35,000.

Tin.—A little tin ore was taken out in the course of development work in Dakota, Wyoming, Virginia, and Alabama, but the only metallic tin made was a few hundred pounds from ore of the Black Hills (Dakota) mines, made in sample tests at New York City pending the building of reduction works at the mines.

Platinum.—The amount mined in 1884 was about 150 troy ounces, worth, crude, $3 per ounce.

Aluminium.—The amount made in the United States in 1884 was 1800 troy ounces, an increase of 800 ounces over the production in 1883. At 75 cents per ounce the total value was $1350.

Building Stone.—It is estimated that the value of the building stone quarried in 1884 was $19,000,000, as against $20,000,000 in 1883, the decline being due partly to dullness of trade and partly to the increased use of other structural materials.

Brick and Tile.—The output was about the same as in 1883, but as manufacturers cut down expenses still further, meeting a lower market, the total value is estimated at $30,000,000 as against $34,000,000 in 1883.

Lime.—There were 37,000,000 barrels (of 200 pounds) made in 1884, the average value per barrel at the kilns being not over 50 cents, or $18,500,000. The production was about 5,000,000 barrels greater than in 1883, but owing to the fall in price the total value was about $700,000 less.

Cement.—About 100,000 barrels (of 400 lbs.) of artificial Portland cement were made, or 10,000 barrels more than in 1883; the total value, at $2·10 per barrel, being $210,000. The production of cement from natural cement rock was 3,900,000 barrels (of 300 lbs.), or 200,000 barrels less than in 1883; worth, at 90 per cents per barrel, $3,510,000. The total production of all kinds of cement was about 4,000,000 barrels, valued at $3,720,000.

Precious Stones.—The estimated value of American precious stones sold as specimens and souvenirs in 1884 was $54,325, and the value of the stones sold to be cut into gems was $28,650; total, $82,975. About $140,000 worth of gold quartz was saved as specimens or made into jewelry and ornaments.

Buhrstones.—The value of the buhrstones yearly made in the United States is about $300,000.

Grindstones.—Dealers estimate the value of the grindstones made in 1884 at $570,000.

Phosphates.—The production of washed phosphate rock in South Carolina during tne year ending May 31, 1884, was 431,779 long tons, worth $2,374,784, or 53,399 tons more than in the previous year, wtth an increase of $104,504 in value. The average spot price, $5·50 per ton, was 50 cents less than in the preceding year. The recent discoveries of phosphate rock in the adjoining States of North Carolina, Alabama, and Florida will probably lead to a still further increase in production. Of manufactured fertilisers, 967,000 short tons, worth 26,110,000, were made in the year ending April 30, 1884, and 1,023,500 short tons, worth $27,640,000, were made in the year ending April 30, 1885.

Marls.—In New Jersey about 875,000 tons, worth $437,500 at the pits, were dug in 1884. In addition, small quantities were produced for local use in some of the Southern States. The production is declining, owing to competition with fertilisers made from phosphate rock, &c.

Gypsum.—In the Atlantic States, from Maine to Virginia, 65,000 long tons of land plaster and 60,000 tons of stucco, total 125,000 tons, were made in 1884, of which nearly all was from Nova Scotia gypsum. The statistics for Michigan have not been reported, but the production did not vary greatly from that in 1883, in which year it was 60,082 short tons of land plaster and 159,100 barrels (of 300 lbs.) of stucco. In Ohio 4217 short tons of land plaster and 20,307 barrels of stucco were produced. There was also a small production in other parts of the country; but the total amount of domestic gypsum used is not known.

Salt.—The production in 1884 was 6,514,937 barrels of 280 pounds (equivalent to 1,824,182,360 pounds, or 32,574,685 bushels, or 912,091 short tons, according to the unit used). The total value, computed on average wholesale prices at the point of production, was $4,197,734. The apparent output was 322,706 barrels greater than in 1883, while the value was $13,308 less; but the production figures do not include a considerable stock on hand in the Onondaga district, not officially reported because not inspected.

Bromine.—The production is estimated at 281,100 pounds, all from the Ohio and West Virginia salt district; worth, at 24 cents per pound, $67,464.

Borax.—Production about 7,000,000 pounds, or 500,000 pounds more than in 1883. The total value, however, was less than that of the product of 1883, being about $490,000 at San Francisco rates, as against $585,000 in 1883.

Sulphur.—No exact statistics. The production was only 500 tons, worth about $12,000.

Pyrites.—About 35,000 long tons were mined in the United States, worth about $175,000 at the mines. Some 33,500 tons of imported pyrites were also burned, making a total consumption of 68,500 tons.

Barytes.—Full statistics not received. The production is estimated to have been about 25,000 tons; worth, at $4 per ton, unground, at the point of production, $100,000.

Mica.—The production of merchantable sheet mica, not including mica waste, was 147,410 pounds, valued at $368,525.

Feldspar.—The production was 10,900 long tons, or 3200 tons less than in 1883. Its value at the quarries was $55,112.

Asbestos.—The amount mined was about 1000 short tons, worth about $30,000.

Graphite.—Production nominal, the supply being drawn from the stock accumulated in 1883.

Asphaltum.—The annual production is about 3000 tons, having a spot value of $10,500.

Alum.—About 38,000,000 pounds were made in the United States in 1884, or 3,000,000 pounds more than in 1883. At an average spot value of 1⅞ cents per pound, the product was worth $712,500.

Copperas.—The amount made in 1884 was 15,500,000 pounds, worth, at 60 cents per hundredweight, $93,000.

Mineral Waters.—The sales of natural mineral waters in 1884 amounted to 68,720,936 gallons, valued at $1,665,490, an apparent increase of 21,431,193 gallons, and $526,007 upon the figures for 1883. While the sales are undoubtedly increasing, it is possible that the excess in the reported quantity and value of the waters sold in 1884 as compared with 1883 may be partly due to the greater fullness of the returns for 1884. Besides the waters bottled and placed on the market there is a large local consumption, not included in the foregoing figures.

Totals.—As was remarked in the former report, it is impossible to state the total mineral product in any form which shall not be open to just criticism. It is evident that the production statistics of such incongruous substances as iron ore, metallic gold and silver, the spot value of coal mined, and the market value of metallic copper after having been transported hundreds of miles, the spot value of a crude substance like unground, unrefined barytes, and the value of a finished product like brick (in which the cost of manufacture is the leading item) cannot well be taken as items in a general summary. The statistics have been compiled with a view to giving information on those points which are of most interest and utility, and are presented in the form usual in the several branches of trade statistics. The result is that the values stated for the different products are necessarily taken at different stages of production or transportation, &c. Theoretically perfect statistics of mineral products would include first of all the actual net spot value of each substance in its crudest form, as taken from the earth; and yet for practical purposes such statistics would have little interest other than the fact that the items could be combined in a grand total in which each substance should be rated on a fairly even basis. The following groupings, therefore, are presented with a full realisation of the incongruity of many of the items. The grand total might be considerably reduced by substituting the value of the iron ore mined for that of the pig iron made, by deducting the discount on silver, and by considering lime, salt, cement, borax, &c., as manufactures. It will also be remarked that the spot values of copper, lead, zinc, and chrome iron ore are much less than their respective values after transportation to market. Still the form adopted seems to be the only one which admits of a comparison of the total values of the mineral products from year to year.

Résumé of the Values of the Metallic and Non-metallic Mineral Substances produced in the United States in 1884.

Metals	$186,097,599
Mineral Substances named in the foregoing Table	220,007,021
	406,104,620
Fire-clay, kaolin, potter's clay, common brick clay, terra cotta, building sand, glass sand, limestone used as flux in lead smelting, limestone in glass making, iron ore used as flux in lead smelting, marls (other than New Jersey), gypsum, tin ore, antimony, iridosmine, mill-buhrstone, and stone for making grindstones, novaculite, corundum, lithographic stone, talc and soapstone, quartz, fluorspar, nitrate of soda, carbonate of soda, sulphate of soda, native alum, ozokerite, mineral soap, strontia, infusorial earth and tripoli, pumice-stone, sienna, umber, &c., certainly not less than	7,000,000
Grand Total	$413,104,620

The total value of the metals and minerals produced in 1884 was $39,100,008 less than in 1883, and the decline in 1883 from 1882 was $3,012,061; that is, the falling off in value began on a small scale in 1883, but was accentuated in 1884. The net decline has been due rather to a depression in price than a decrease in quantity; indeed, several important substances show a decided increase in production, notwithstanding the general dullness of trade. The over-production, taking the whole field into consideration, has been less than was generally feared.

PROF. L. SOHNCKE ON THE ORIGIN OF THUNDERSTORM ELECTRICITY[1]

IN order to express more than mere surmises as to the origin of thunderstorm electricity we must, above all, be familiar with the atmospherical conditions under which thunderstorms usually occur. For this purpose we must first take into consideration two general facts in meteorology: first, the average decrease of temperature with increase of height in free air; and secondly, the nature of the upper clouds.

With regard to the first point, a considerable amount of data is available in the observations of several scientific balloonists, especially those of Mr. J. Glaisher. Glaisher has constructed a table, based upon his numerous ascents, showing the average decrease of temperature for the altitudes of 1000, 2000, 3000 feet, &c. This table shows that even in the warm summer months the temperature of the freezing-point is met with generrally at the level of between 3000 and 4000 metres (say 10,000 to 13,000 feet).

Generally speaking, the aggregate of those points of space in which the temperature of 0° C. prevails at any given moment must lie on a certain surface, which may be denoted as the "isothermal surface of zero C." It is of especial interest to ascertain whether the result yielded by Glaisher's ascents as to the height of this surface in midsummer is confirmed by other ascents. In order to obtain an opinion upon this point I have grouped together those ascents which afford a sufficient number of data, in order to deduce therefrom the height of the isothermal surface of zero. This table includes twenty-three ascents by eight different balloonists at different seasons of the year; about half the ascents were made during the summer months. The following are the conclusions drawn from this table:—

In the warmest summer months the isothermal surface of zero was found to be at an height of about 3000 to 4000 metres, but occasionally sinks even at this season to about 2000 metres (say 6500 feet) above the level of the sea. It generally rises in the course of the forenoon, and, apparently, more rapidly the nearer noon is approached. It sinks in the course of the afternoon, and, apparently, more rapidly with the greater distance from noon. Its level may vary about 2000 metres in from one to two hours. The change from rising to sinking does not occur exactly at noon, but perhaps one hour or even more after noon, according to season.

A knowledge of the decrease of temperature on days of thunderstorms, especially just before the storm, presents therefore especial interest. Only few data exist on this point.

Glaisher made an ascent at 6 p.m. on the 31st August, 1863, after a thunderstorm had taken place at 8 a.m. He did not reach the isothermal surface of zero, but found a temperature of 1° C. at a height of 2300 metres (say 7500 feet). I have never found such a low temperature at a similar height in any of the six ascents in August and the beginning of September.

Flammarion made an ascent during the night of the thunderstorm of the 14–15 July, 1868, and met with 0° C. at a height of 2400 metres (say 6500 feet), but this was at 4h. 26m. a.m. Among all the midsummer ascents there is only one in which the isothermal surface of zero was met with at a lower level.

Welsh made an ascent in the afternoon of the 17th August, 1852, two hours before the occurrence of a thunderstorm; at 5 p.m. the isothermal surface of zero lay at a height of 3500 metres (say 11,500 feet), but it was rapidly sinking. Welsh did not find such a rapid decrease of temperature upwards in any of his other three ascents as in this one.

Kämtz has drawn the conclusion, based upon the great refraction which has often been observed with sultry thunderstorm air, that the rapid change of temperature with height is an important condition for the formation of thunderstorms, especially in summer. In order to obtain more precise data upon this point I have undertaken a small meteorological investigation as to the difference of temperature existing just before thunderstorms between Freiburg in the Breisgau and the Höchenschwand in the Black Forest, 2326 feet above it. I found that in seventeen cases which were suitable for comparison, in the years 1880 and 1881, the difference of temperature just before the thunderstorm was less than the average for the day and season in three cases only; in other cases it was greater.

From this it appears that, in most cases, the abnormally rapid decrease of temperature with height, and, in connection with this, the abnormally low position of the isothermal surface of zero may be taken as characteristic of the condition of weather before thunderstorms.

Secondly, attention must be paid to the nature of the upper clouds not only generally, but also more especially before thunderstorms. The clouds which lie above the isothermal surface of zero must of course mainly consist of ice particles, although, of course, the formation of clouds of superfused water particles is not excluded. The appearance of the ice clouds is, moreover, somewhat different from that of the water clouds. The former are known as "cirrus" and the latter as "cumulus" clouds. Observations on the height of clouds, made either in balloon ascents or on the ground, agree in showing that the limit of both kinds of clouds in midsummer lies about 4000 metres (say 13,000 feet) high, which agrees pretty well with the above calculation of the level of the isothermal surface of zero. It is not surprising, therefore, that balloonists frequently reach snow-clouds even in midsummer—for instance, Glaisher on June 26, 1863, between 3300 and 4200 metres (say 11,000 and 14,000 feet); Fonvielle on July 4, 1875, at 3450 metres (say 11,300 feet); Barral and Bixio on July 27, 1850, between 4500 and 6300 metres (say 15,000 to 20,500 feet); Welsh on August 17, 1850, at 5990 metres (say 19,500 feet).

While the distinction between ice and water-clouds, from their mere appearance as seen from the earth, is always somewhat difficult to be made out, we have in many cases an infallible

[1] Extract from "Sitzungsberichte der Jenaischen Gesellschaft für Medecin und Naturwissenschaft." Jahrg. 1885. Sitzung vom 1 Mai.

means of distinguishing between them—namely, by the character of the solar and lunar halos which are very often seen in thin veils of clouds. It has been established beyond doubt that the rings of light of larger size, or halos of about 22° diameter, are caused by refraction in ice crystals. (This angle is that of the least deviation for rays of mean refrangibility in passing through ice-prisms of 60°). On the other hand the smaller rings (coronæ) of from 1° to 6° diameter owe their origin to the refraction of light through spheres of uniform size. Halos are not nearly so rare as is commonly supposed. M. Galle observed seventy-eight halos and about as many parhelia in a year and a half, and often even in the heat of summer. Kämtz laid great stress on the importance and infallibility of this optical means of distinguishing between the two classes of clouds.

After these preliminary considerations let us turn to thunderstorms. The local or heat thunderstorms (identical with most summer thunderstorms) are best known, while the large cyclonic thunderstorms have been less investigated. In the first case, the appearance of the clouds which rise high in the sky as gigantic columns of cumulus, show that they owe their origin to a strong ascending current of great humidity. According to Dr. Reye, the principal condition for the formation of a continuous ascending moist air current is the abnormally rapid decrease of temperature in its vicinity, while in the current itself the decrease of temperature with height is essentially retarded, owing to the latent heat set free by the condensation of vapour. Under these conditions, the distribution of temperature in the atmosphere is therefore such that the isothermal surface of zero in the ascending current is raised especially high, while outside this current the surface has an abnormally low position. In this way, therefore, water still in a liquid state reaches the ice-region; ice-clouds and water-clouds must exist side by side. If the moist current rises sufficiently high its temperature sinks below 0° C. and this gives rise to cirrus clouds of snow and hail, which latter frequently accompanies thunderstorms. Kämtz has shown from his observations in high mountains that the height of the locus of thunderstorms must not be placed too low; the usual estimations of the height of thunderstorms based upon observations of lightning and thunder cannot be taken into account here, for they only show (and that very inaccurately) the position of the lowest structure of the thunderstorm clouds.

Both Hann and Kämtz agree that water and ice clouds always exist simultaneously in the sky, and not only during local thunderstorms but also during those of the other kind. Hann describes the layers of "cirrostratus" cloud as always existing with thunderstorms. Kämtz has always been able to recognise halos *i.e.* the characteristic indication of the presence of ice particles before thunderstorms, as soon as he could trace the change from clear sky to thick clouds. And in all three of the previously mentioned balloon ascents on days of thunderstorms ice particles have been observed in the air.

We may take it that it has been established that in every thunderstorm clouds composed of water particles and others of ice particles exist simultaneously side by side, and that, of course, they are mutually changing places it is very easy to suppose that the friction of water particles and ice particles may serve as a source of electricity. But this is in no way a mere supposition, for it is a fact already established by Faraday. In his experiments on the cause of the production of electricity in Armstrong's steam electrical machine, which was considerably modified by him, he frequently caused compressed air to strike against solid objects. The cooling arising from the expansion of the air caused a copious formation of fog, and the friction of these particles against the objects always excited the particles with + electricity and the solid objects with − electricity. It was only by the friction of the particles against ice that the latter became + on every occasion, while wood and metal were excited with − electricity by the friction of the particles.

I have frequently repeated these experiments of Faraday's, and, as was to be expected, entirely confirmed them. Of course several precautions must be taken if we do not wish to be checked by evidently contradictory results. The principal causes of disturbance may arise by the carrying away of particles of grease from the greasing of the taps, and on the other hand by the friction of the particles on the walls of the tube, if the turning on is not quick enough. In the latter case the particles become + and communicate this electricity to the objects they meet, and thus, therefore, the character of the electricity by friction with these bodies is partially or wholly masked. The colder the ice the more powerfully it becomes electrified—a fact which appears to be in connection with the increase of its insulating power with decreasing temperature.

If, therefore, air-currents flow against each other, one being of ice particles and the other of water particles, the ice particles become positively and the water particles negatively electrified, and as by no means a rapid mixing of both kinds of air-currents is requisite, which may be seen *inter alia* from various observations on smoke-laden air-currents in laboratories, the oppositely electrified bodies are quickly repelled from each other.

The real cause of thunderstorm electricity appears to me to lie in the sequence of phenomena above described. It is not my intention to discuss the behaviour of the further phenomena connected with thunderstorms.

A more detailed exposition of the theory very briefly sketched here, as well as the observations used for its proof, will be found in my Treatise on the Origin of Thunderstorm Electricity and of the Ordinary Electricity of the Atmosphere, just published by G. Fischer, Jena. ("Der Ursprung der Gewitter-Elektricität, und der gewöhnlichen Elektricität der Atmosphäre.")

CYSTOLITHS

M. J. CHAREYRE[1] has made a detailed examination of these structures in plants belonging to the Urticaceæ, and to many other families. The following are some of the chief results:—

In the Urticaceæ the prolonged action of darkness causes complete disappearance of the calcium carbonate in the cystolith, though without their mass sustaining any diminution; they retain completely their original form. This takes place before the etiolation which is the result of placing the plants in question in the dark. It is due probably to the cessation of the action of chlorophyll. Calcium oxalate disappears in the same way from the cystoliths. The lime set free by the decomposition of these salts collects in the stem, where it exists in combination with some other acid.

In the Acanthaceæ, on the other hand, none of these phenomena are exhibited, and the cystoliths undergo no change from the action of darkness; and this difference in the behaviour of the cystoliths in these two natural orders appears to correspond to no less important differences in their constitution. The calcium carbonate appears in one family in the crystalline, in the other family in the amorphous, form.

All the seeds of Urticaceæ examined before germination presented reservoirs of food-material consisting exclusively of aleurone, in each of which was a rounded globoid. The same was the case with the Acanthaceæ, except *Acanthus* and *Hexacentris coccinea*, plants containing no cystoliths, and in which the reserve-material of the seeds consists chiefly of starch. The calcareous reserve-material contained in the seeds in the form of globoids disappears more rapidly when they germinate in a soil formed of pure silica than in ordinary soil, or in one composed of calcium carbonate. But no part of this reserve-material contributes to the formation of deposits of calcium carbonate, whether as cystoliths or in any other form, nor to the production of crystals of calcium oxalate. In seeds which germinate in pure silica the cystoliths do not arrive at full development; the pedicel is formed, but no deposition of either cellulose or lime takes place at its extremity. In soil composed of calcium carbonate the cystoliths appear at the same time as in ordinary soil, *i.e.* at the moment when the green cotyledons are disengaging themselves from the seminal envelopes, but their development is somewhat more rapid. Seeds sown in ordinary soil or in calcium carbonate, but kept in darkness, give rise to rudimentary cystoliths without any calcium carbonate.

Yellow dying leaves of many Urticaceæ present, as contrasted with green leaves, cystoliths containing a smaller quantity of calcium carbonate, but this is not the case with Acanthaceæ or with *Pilea*.

Both the calcium carbonate in the cystoliths and calcium oxalate are believed by the author not to be merely products of excretion, but to play an important part in the life of the plant.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE following list has been issued by the Science and Art Department of successful candidates for Royal Exhibitions,

[1] See *Revue des Sciences Naturelles* for 1884 and 1885.

National Scholarships, and Free Studentships, May, 1885:—William Burton, aged 22, science teacher, Manchester, National Scholarship; Philip L. Gray, 19, assistant master, Southampton, National Scholarship; Charles Lang, 22, engineer, Johnstone, N.B., National Scholarship; Thomas Clarkson, 20, engineer, Pendleton, Manchester, National Scholarship; Harry E. Hadley, 18, student, Worcester, Royal Exhibition; William Scudamore, 16, student, Northampton, National Scholarship; Frederic W. Lanchester, 16, architect's assistant, Southampton, National Scholarship; Thomas H. Holland, 16, student, Helston, National Scholarship; Harold E. Hey, 14, student, Manchester, National Scholarship; William Blackmore, 18, student, Sheffield, National Scholarship; Hugh O. Bennie, 20, engineer, Glasgow, Royal Exhibition; William Kelsall, 17, student, Bradford, National Scholarship; Henry Sowerbutts, 17, student, Manchester, National Scholarship; Frederick Chattaway, 24, chemist, Birmingham, National Scholarship; James Young, 23, shoemaker, Belfast, Royal Exhibition; Arthur J. Moulton, 20, engineer's apprentice, Preston, Royal Exhibition; Harold C. Coote, 17, student, London, Royal Exhibition; Robert H. Unsworth, 20, engineer, Pendleton, Manchester, Royal Exhibition; Sidney H. Woolhouse, 15, student, Weaste, Manchester, Royal Exhibition; David Wilkinson, 21, agent, Preston, Free Studentship; Henry P. Motteram, 19, student, Small Heath, Birmingham, Free Studentship; Albert E. Briscoe, 17, machinist, Birmingham, Free Studentship; Orlando J. Preston, 16, student, Bristol, Free Studentship; James McKenzie, 20, engineer, Glasgow, Free Studentship; Philip C. Coultas, 18, student, Bristol, Free Studentship.

We have received the calendars for the ensuing session of the University College, Dundee, and of the Durham College of Science, Newcastle-on-Tyne. A very important addition to the educational programme of the former is the establishment of a new Chair of Biology. This chair was needed on account of the recognition of the science department of the college by the University of Edinburgh, and also because the science curriculum was incomplete for graduation in London University. The new chair has been filled by Mr. D'Arcy Thomson, of Cambridge.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, August 17.—M. Bouley, President, in the chair.—Observations of the minor planets made at the Observatory of Paris (large meridian instrument) during the second quarter of the year 1885, communicated by M. Mouchez.—Note on typhoons and the so-called "grains arqués" of the eastern hemisphere (two illustrations), by M. Faye. From the descriptions of these meteorological phenomena made by observers in the Indian and Pacific Oceans, and in Senegal, the author concludes that all such cyclonic movements of the atmosphere affect a circular form. The importance of this conclusion is obvious in connection with the theory which regards these atmospheric disturbances as sharply defined whirlwinds of circular shape, not as the result of currents converging towards a common centre of attraction without definite outer limits.—Researches on the present and the prehistoric races of Brazil, in connection with the sixth volume of the Natural Museum of the *Archives* of Rio de Janeiro, presented to the Academy on behalf of the Emperor Don Pedro II., by M. de Quatrefages. The contents of this volume are of great importance for the comparative study of the Brazilian races past and present, and of the primitive cultures of the more civilised populations in North and South America. It contains papers by M. Hartt on the river and marine shell heaps occurring in various parts of Brazil; by M. J. B. de Lacerda on the human remains found from time to time in these deposits, and by M. Peixoto on the Botocudos still surviving in the eastern provinces of Brazil. But the chief attraction of the volume is the valuable and richly illustrated memoirs of M. Ladislaus Netto, on the remarkable artificial hill of Pacoval, which is now fully described for the first time. This hill, entirely the work of man, stands on the margin of a lake in the island of Marajo, and although reduced by erosion and weathering to a fifth or a sixth of its original size, is still 300 metres long, 250 broad, and 6 high. It presents the outlines of a gigantic Jabuti turtle (*Emys faveolata*), in this respect showing analogies with the works of the mound-builders in the Mississippi basin. Its contents are of the most varied character, including stone implements of all kinds, idols, amulets, and a vast quantity of earthenware, funeral urns, vases, *tangas*, &c., covered with ornamental designs remarkable for their delicacy and taste, either painted, incised, or modelled in relief. Some of the signs appear to be of a hieroglyphic character, presenting certain analogies to the early Chinese, Egyptian, and Mexican writings. One inscription, which M. Netto has attempted to interpret, seems to speak of long migrations, most probably from the Andes highlands down the Amazons basin, towards the Atlantic seaboard.—Experimental researches on cholera, by MM. Paul Gibier and van Ermengem. Appointed by their respective Governments to study Dr. Farran's method of preventive vaccination, these biologists have independently arrived at the same conclusion, that the sub-cutaneous injections of the cultivated virus (comma bacillus) does not preserve from cholera the animals on which their experiments have been made.—Observations of Tuttle's comet made at the Observatory of Nice (Gautier's equatorial), by M. Perrotin.—Equatorial observations of Barnard's comet (*a*) made at the Observatory of Algiers with the 0·50 m. telescope, by M. Ch. Trépied.—Account of a remarkable solar protuberance observed on the eastern edge of the disk at Paris on August 16, by M. E. L. Trouvelot.—Description of a new magnifying apparatus for the projection of microscopic objects as well as of images of large dimensions, by MM. Théodore and Albert Duboscq.—On the action of the iodide of phosphonium on the oxide of ethylene, by M. J. de Girard.—On the elective alcoholic fermentation of sugar, by M. H. Leplay. Against the objections of MM. Bourquelot and Maumené the author maintains from his own experiments that the elective alcoholic fermentation as originally discovered and described by M. Dubrunfant really exists and must be accepted as a scientific although hitherto unexplained phenomenon.—On the organisation, anatomy, and digestive function, of *Pachydrilus enchytræoides*, by M. Remy Saint-Loup.—Extraction and composition of the gases contained in the foliage of floating and submerged aquatic plants, by MM. N. Gréhaut and J. Peyrou.—Recurrence of the superficial earthquake at Escarpel and in the neighbourhood of Douai, Département du Nord, by M. Virlet d'Aoust.

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