THURSDAY, MARCH 14, 1889.

THROUGH THE HEART OF ASIA.

Through the Heart of Asia: Over the Pamir to India. By Gabriel Bonvalot. Translated from the French by C. B. Pitman. (London: Chapman and Hall, 1889.)

THIS is a translation of the account of a very remarkable journey taken in the year 1886 by M. Bonvalot through Central Asia. It is not the first time that M. Bonvalot has traversed that region, for in the years 1880-82, accompanied by M. Capus, and starting from Moscow, he entered Turkestan from Siberia, explored part of Bokhara and the mountains of Kohistan and Chitral, and returning home by Samarcand and Bokhara, descended the Amu-daria to Khiva, and finally crossed the desert of Ust-Urt in the depth of winter. That journey, however, only embraced the country north of the Oxus, and the present one was undertaken to complete the exploration of Central Asia. Shortly speaking, the latter journey—that of which this work treats—began at Batum, whither MM. Bonvalot and Capus had gone by ship from Marseilles, and continued through the Caucasus along the south-west shore of the Caspian Sea into Persia, thence into Bokhara. At Chitral, on the borders of Afghanistan, they were detained as prisoners by Ishak Khan, of whom we have heard so much lately. He was then in revolt against Abdurrahman, and his captives were not released till the Indian Government interfered on their behalf.

There is very much to interest us in M. Bonvalot's account of the feelings of the native tribes in that vast region towards England and towards Russia, but that we shall pass over, merely pointing out what is most important from a geographical or ethnological point of view in the work. At Adshi-Cabul, which is on the south-west shore of the Caspian, he says that the population is Turkish, though the country is Tartar. The men are tall, with good features, and are plainly a mixture of Caucasian and Persian. Their dress differs from that of other Mahometans in that the tight-fitting upper garments with long skirts are not frequently seen; they seldom reach below the knee, and are often open at the breast. The head-dress is higher and broader at the top than the usual Mahometan head-dress, and in place of high boots they wear broad slippers or sandals, with very large wooden heels and curled-up toes. The feet are bare, and are covered with woollen socks, which have fantastic coloured figures worked on them. It was the festival of the New Year when the travellers arrived at Adshi-Cabul, and the whole place was *en fête*. The Mahometans of the region on such occasions dye their hands, beard, and hair a bright red, with henna. The Turkish language was the only one spoken there, but a few stages further on it seemed as if a new world was reached, where the Russian tongue, Russian fashions, Russian cattle and horses, were everywhere. This, it appears, was due to the descendants of a body of Russian settlers, called Malakanes, who had left their homes on account of their religion, but it is not quite clear in what respect they differed from the orthodox Russian Church. The country—that is, the region north-west of Resht—is wonderfully fertile, but the inhabitants suffer terribly from fevers. The waters swarm with duck, teal, herons, and cormorants; eagles and foxes abound there.

Further on, still on the shores of the Caspian, may be seen "all the birds of creation." Generally speaking, the ground held by the Tartars is badly cultivated: the men are incorrigible idlers, lying in the fields while the women sow the seed; and in harvest-time, instead of gathering in the crops themselves, they sell them as they stand in the fields to the mountaineers for about a third of their value. Though there is an abundance of good timber at hand, they live in wretched hovels with thatched roofs, or in huts made of reeds, with a thin facing of mud, and in the coldest weather prefer to lie shivering in their huts to gathering the firewood which lies almost at their hand. They are badly clad, and, as has been said, suffer terribly from the fevers that are so prevalent. A little further south, towards the confines of Persia, though the month was March, the foliage and the flowers were marvellous: wild pomegranate trees and wild briars growing under acacias and beeches were well covered with leaves; anemones, violets, and daisies bloomed everywhere. At Chifa Rud, near Resht, Talichi, a peculiar Turkish dialect is spoken, but some of the people also use Guilek. Rice and fish, of which there is abundance, are the food of the inhabitants, bread being unknown. From Teheran the travellers journeyed to Meshed, travelling almost due east, and skirting the Great Salt Desert, which presented the appearance of heaps of sand that had been lately washed by the waves of the sea. The journey through the mountains at this part was very trying. The sun was scorching, and there was not the slightest shelter to be had. The River Tchaï flows through the valley which they traversed, and was the only thing in motion in the whole region. The waters of the river are salt. Dehinemek (the "Salt Village") consists of only a few houses, and the inhabitants of it are as idle as those on the border of the Caspian, cultivating just enough to feed their families and to supply passing pilgrims. Here, as in the whole country, everything seems covered with salt. It is seen on the walls of the houses and on the banks of the rivers, and and the water one drinks is very salt. There are many travelling saltpetre-makers to be found in this region, who in summer go from place to place wherever they can find materials to work upon. Their mode of operation is a very rough and ready one. Holes in the earth serve as vats and boilers, and below these are placed ovens. Abundance of brushwood supplies them with material for their fires. They collect from the surface of the earth heaps of a compost of salt and animal manure. This is soaked for twenty-four hours in water, then filtered, and then boiled for twenty-four hours, cleansed, and placed in the sun, so that the water may evaporate. An ordinary workman can make about fifty pounds in a day, and this he sells at the rate of a penny a pound. The workers appear quite contented with their lot, and the industry is preserved in their families for generations.

Around Bostan and Shahrud, numbers of gypsy encampments were met. The inhabitants said they were natives of Seistan, and gained their living by making brass wire and working metal. Each family pays a small yearly contribution to the chief of the Shah's footmen.

They are constantly on the move, and make their tents or huts so lightly that they can dismantle them at a moment's notice. They resemble the natives very much in features, and have the same filthy habits, but they are more swarthy in complexion, and much thinner. With regard to the Turkomans, they are, or rather were—for they have ceased their depredations—the terror of the Persian pilgrims and traders. M. Bonvalot thinks that they have received a bad name which they do not deserve. They are frank, gentle, hospitable, affable, and truthful; while their victims, the Persians, are the incarnation of deceit and lying. The Turkomans, he says, have all the good qualities of the Turks, a race which has been more calumniated than any other, but from which some of the more highly esteemed European nations might with advantage learn many things. From Samarcand they journeyed to the Amu-daria, whence, completing an irregular curve, they returned to Samarcand. Upon leaving Sari-jui, they came up with a number of Uzbegs, who were coming down from the summer encampments in the mountains to their winter-quarters in the valleys. The men of the tribe ride in front, driving the cattle and horses before them, and the younger children look after lambs, goats, and calves. Some are bareheaded; others wear a dirty turban. Though they are filthy in their habits, they are wonderfully strong. Their dwellings are strong, but easily taken to pieces. As a rule, the Turkoman is not a nomad, like the Uzbeg; he has not very many cattle, so that it is not necessary for him to move to different pastures with the changes of the seasons. In the valley of the Surkhan they met many encampments of gypsies, who appear to lead the same sort of life all over Central Asia. Their tents are of the most primitive kind—two poles, with a piece of cloth spread over them. From this to Regar the country is very rich, well watered, abounding in rice-fields, but, like the other parts of Central Asia where there is a copious rainfall, fevers abound. At Kara-tagh, besides English, French, Russian, and Indian goods, various products of the district itself are sold in the market, as, for instance, rice, barley, wheat, grapes, and dried apricots. In the towns there are many blacksmiths and saddlers, and a few potters, who are very skilful in making and enamelling cups and dishes. Strange to say, the occupation which has the most numerous representatives is that of druggist. A funeral procession amongst the Uzbegs, who regard death as a blessing and not as an evil, is thus described:—

"A small stream which crosses the road is salt, and we notice a group of men ascending its course with very rapid steps. They are carrying something on a stretcher, but, instead of going as slowly as they can, like bearers of a dead body in the West, they march at full speed, as if they were conveying some one who was very ill, or had met with an accident, to the doctor's. They go across the fields, and stumble in their hurry. They talk loudly, as if they were quarrelling, and there is not the least trace of sadness upon their faces. There are about ten of them, and they are dressed as plainly as usual. . . . In advance are several men with poles, which will be used for forming a vault on the grave, and behind comes an aged mollah, leaning upon a stick."

The travellers reached Samarcand in December, and were about to abandon the idea of going further, when General Karalkoff suggested that they should try to enter India by Pamir. It was worth trying, for nobody had ever before thought of attempting such a thing in winter. The undertaking was a great one, and they took every care in their outfit before setting out. Boots made of double felt with leather soles, with strips of skin protecting the seams, were made for them. Long stockings, made of felt, reaching up to the thigh, lined trousers with leather trousers over them, were the protections for the legs. The body was covered with two garments, one of them made of Kasghar sheepskin with the wool left on. A sheepskin cap covered the head and ears, and a huge hood of the same material enveloped the whole head and face, with the exception of the eyes. With regard to the food supply, the more remarkable portions were immense numbers of small slabs of bread soaked in fat and baked twice over; mutton boiled down, salted, and placed in bladders; roasted millet seed, and dried apricots. The journey really began at Osh on March 6, and for over a month the whole party underwent the severest privations; the goal, however, was reached at last. We have not space to say much about this journey over the Pamir, one of the most remarkable feats of travel ever accomplished, but a few extracts in M. Bonvalot's own words will show what the enterprising explorer and his companions went through:—

"March 13. . . . The variations of temperature are very great, for at 9 a.m. the thermometer marks 75° F. in the sun and 10° F. below freezing in the shade, while at 2 p.m. it is nearly 100° F. in the sun and 3° below freezing in the shade; at 6 p.m., there are 18° of frost, while at 9.20 p.m. the glass is several degrees below zero."

"March 17 (when crossing the Alai). We are all of us exhausted and out of breath, devoid of all strength, and nearly blind. We have splitting headaches and a feeling of suffocation. One man is stretched out on his back, close beside his horse, which is lying on its side; another man is asleep as he stands with his head resting against the saddle; a third is whipping his poor horse, to the tail of which he clings like a drowning man to a buoy. Some of the men were bleeding from the nose, and so were the horses, the blood freezing as it trickled down their muzzles, and looking like ruby stones. . . At 6 in the morning there were 75° of frost."

On March 22 they reached an altitude of 15,000 feet on Kizilart. On March 27, when at the height of 15,700 feet—that is, at the summit of the pass of Kizil-Djek—a terrific snow-storm burst on them.

"March 30. . . . At 2.20 a.m., with the moon still so luminous that I could distinguish objects inside the tent, I go out to look at the thermometer, and find that the mercury has vanished. It has evidently been frozen. Thinking that I may be mistaken, I show the instrument to Capus, and we light a candle, the result being that we find the mercury really has frozen up, and is no bigger than a leaden pellet."

About the beginning of April they approached human habitations, and here, near the source of the Oxus, they found numbers of monuments to the dead:—

"The tumuli are built south-west to north-east, so that the dead may have their faces turned towards the holy city. They extend around four mausoleums made of earth, almost double the height of an *oui* (felt tent), and with a frontage of about 30 feet. The cupolas are pointed, and the architecture very simple, as there are no materials handy to attempt anything ambitious. Moreover, if a higher building had been erected, the wind, which is the terror of the Pamir, would soon have brought it to the ground. At the four corners of the largest of the mausoleums a rude attempt has been made to carve pigeons. At the end of the humbler tombs are some stones sunk in the earth. Some of them have a sort of railing round them, formed of stakes bound together by wooden cords."

The numerous illustrations are excellent, and the translation is well done, but it is a pity that Mr. Pitman has not given the English names of places. Many readers will not recognize in French forms names which in English forms are perfectly well-known.

THE TESTING OF MATERIALS OF CONSTRUCTION.

The Testing of Materials of Construction. By William Cawthorne Unwin, F.R.S., M. Inst.C.E. (London: Longmans, Green, and Co., 1888.)

IN a volume of about 480 pages, Prof. Unwin has condensed probably all our knowledge of the strength of materials used in construction. The work is unique in its way, there being no other equally trustworthy, as far as we are aware, in existence.

Engineering structures of to-day are designed with due regard to the strength of their individual parts, and each part is so proportioned that it will safely carry its load, yet not be of greater strength than is necessary for this special purpose. In the Forth Bridge, for example, the strains in each member are calculated to a nicety, and the section of metal is duly proportioned. This could not be done if we did not know exactly the actual strength of the various materials used, as well as their behaviour under varying conditions of load less than the breaking load of the material. It is therefore necessary that these data should be obtained by using the most perfect apparatus obtainable, and that the experiments should be made by persons who have given much time and thought to the subject. Bearing this in mind, we gladly welcome Prof. Unwin's work; his name is well known among engineers as one thoroughly able to write such a work successfully.

The volume consists of three parts. In the first, the mechanical properties of materials are explained—that is, the phenomena of elasticity and plasticity, and the relations between stress and deformation, so far as they have been scientifically ascertained. In the second, the apparatus used in the engineering laboratory is described. Lastly, the third part contains a collection of the most complete and trustworthy results of testing of all the ordinary materials in use. Chapters IV. to VII. contain admirable descriptions of various kinds of testing machines, measuring instruments, and other useful appliances used in an engineering laboratory. These chapters are freely illustrated with excellent woodcuts of the different instruments, the larger ones of the testing machines being remarkably good.

The chapters dealing with iron and steel show a large knowledge of their practical working and characteristics. We agree with the author that a standard form for tensile test pieces ought to be accepted by engineers in this country, so that results may be more easily compared.

In all the large steel-works in this country there is now to be found a testing machine with the necessary apparatus for testing the material. This is intended for the makers' own information, and it is also used by the engineers or their assistants under whose specification the steel is being made. Take, for instance, steel plates for the bridge work under the Indian Government specification. Every plate rolled is tested in the following manner in order to guard against the acceptance of brittle or dangerous steel. As each plate is delivered from the rolls, and before it is sheared to dimensions, four samples in duplicate are marked off in the spare material for testing purposes. Tensile test pieces are taken both lengthwise and across the plate, and similar ones for the quenched bend tests. These test pieces are all stamped with suitable numbers before they are sheared off the plate to be tested. The steel must be of such a strength and quality as to be equal to a tensile strength of between twenty-seven and thirty-one tons per square inch, and to indicate a contraction of the tested area at the point of fracture of not less than 35 per cent.; the percentage of elongation in a length of 10 inches must be not less than 20.

The bend test pieces, heated to a low cherry heat, and cooled in water at a temperature of 82° F., must stand bending double round a curve of which the diameter is not more than three times the thickness of the piece tested. If these requirements are not in every case satisfactory, the plate represented by a defective test is rejected; unless it can be shown that the tested specimen has surface or other defects, in which case the duplicate test-piece is duly tested to take its place.

It must be evident, therefore, that a tremendous amount of testing is now being carried out in this and other countries, and the value of the material, the acceptance or rejection of which depends on the results of the tests, will reach a large amount. On this account we are always glad to increase our knowledge of the behaviour of the material under test, and of the machinery used in the process.

Prof. Unwin is to be congratulated on having successfully fulfilled his task. The work is worthy of his reputation, and should find a place in every engineer's library.

OUR BOOK SHELF.

Entomology for Beginners. For the Use of Young Folks, Fruit-growers, Farmers, and Gardeners. By A. S. Packard, M.D., Ph.D. Small 8vo, pp. xvi. and 367. (New York: Henry Holt and Co., 1888.)

DR. PACKARD'S works on entomological subjects are so well and favourably known, that the book he has now written under the above modest title will certainly obtain a large circulation.

As its name implies, it is a work especially addressed to beginners in the study of entomology, and gives them a brief outline of the extent of the subject, and descriptions of the various methods pursued in its research.

In some respects this work is based upon the same author's "Guide to the Study of Insects," many of the paragraphs being the same in both books and also many of the woodcuts which illustrate them. But the portion relating to classification is much abridged in the smaller book, whilst that which treats of the mounting of microscopic preparations, &c., is given at much greater length. It is this latter part that will be found of more permanent value to the student, who will soon master the brief classification put before him.

The demand for works like the present, we are glad to think, is likely to increase. It is in the examination of the embryology and in the dissection of insects in their various stages of development that the labours of entomologists will be largely engaged when the discovery and description of new species of insects begin to grow slack.

It is when we consider the vastness of the subject of entomology, and the practically inexhaustible field of investigation lying before us, that we feel grateful for such works as Dr. Packard's, which bring before us in a concise form recent methods employed in the treatment of subjects always difficult to manipulate and to render fit for microscopic examination. O. S.

The First Ascent of the Kasai: being some Records of Service under the Lone Star. By C. S. L. Bateman. (London: George Philip and Son, 1889.)

WHEN Lieutenant Wissmann made his memorable descent of the River Kasai, he was accompanied by a number of natives whose good-will he had secured by wise and kindly treatment. At Léopoldville, whence Lieutenant Wissmann returned to Europe, it was decided that Dr. Wolf should take the various members of this escort back to their homes, and that the Expedition under his command should be sent on to establish a station at the confluence of the Lulua with the Luebo. This Expedition was joined by Mr. Bateman as second in command; and in the present volume he records his experiences in the ascent of the Kasai, and during the time when he was engaged in directing the making of the Luebo station. Mr. Bateman is sometimes tempted to indulge rather too much in fine writing, but, notwithstanding this drawback, his book is one of considerable interest. He gives a good general idea of the manner in which he was impressed by the scenery through which he passed or in the midst of which he lived; and he is at great pains to describe, as accurately as possible, the various native tribes with whom he came in contact. Above all, the book is valuable for the light it throws on the influence that is being exerted by the Congo State. Mr. Bateman worked as one of its officials at the Luebo station, and no one who reads the record of what he accomplished can doubt that acting through such agents the new State is preparing the way for the growth of legitimate commerce, and for the development of wholesome relations generally between Europeans and the natives. The volume contains many illustrations, produced from the author's original sketches in pencil, water-colour, pen and ink, or sepia.

Tabular List of all the Australian Birds. By E. P. Ramsay. Pp. 1–38, with Map. (Sydney: Published by the Author, 1888.)

THE present list by Dr. E. P. Ramsay shows that a considerable increase in our knowledge of Australian ornithology has taken place since the year 1878, when the author last issued a list of the birds of Australia. The method adopted in this most useful publication is of the simplest and most effective nature, fourteen natural districts being recognized by Dr. Ramsay, and the distribution of each species being shown by an indication of a number corresponding to the number of the district in the table, so that the known range of every Australian species is seen at a glance. References are given to all the newly-described species, and on the opposite page to each table Dr. Ramsay gives critical notes of great importance to the student. A map is also given which indicates the natural areas of the different provinces of Australia, and enumerates most of the places where well-known collections have been made. A list of the 39 species known from Lord Howe Island and Norfolk Island is also appended, and a new species of owl from the former is described as *Ninox albaria*.

R. B. S.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Meteoric Theory of Nebulæ, &c.

IN his interesting letter (p. 436), Mr. Preston draws attention to a point which was not referred to in my paper.

It has generally been supposed that the incandescence of the trail of a falling star was an evidence of the volatilization of the solid materials of a meteorite, and in the lower regions of the atmosphere this must certainly be true to a greater or less degree. But, in a paper read before the French Academy on February 18 (see abstract in NATURE, February 28, p. 432), M. E. Minary argues that the incandescence of meteorites cannot be explained by the transformation of motion into heat; and, after the reading of the paper, M. Cornu remarked that the luminosity of the trail of a meteorite might be an electrical phenomenon, without any considerable rise of temperature. I quote this in order to show that we should be cautious in accepting Mr. Preston's conclusion as to the rapid retardation and volatilization of meteorites when moving through a highly rarefied gas.

It may be admitted that, when a swarm of meteorites is closely packed, it will soon assume the form of gas throughout, but I cannot believe that gaseous friction affords a valid objection to the meteoric theory, when the meteorites are in loose order. The solidification of volatilized metals would, I suppose, take place in a few minutes or seconds, and the mean interval between collisions was shown, in my paper, to be conveniently measured in hours or days. It would not be unreasonable to suppose that the so-called permanent gases also solidify, when cooled to the low temperature which must obtain. But, apart from this possible solidification, Mr. Lockyer suggests that the permanent gases would be quickly occluded in the solids volatilized at the same instant.

The fusion of meteorites, so as to compensate fractures, forms, as I have myself said, perhaps the most serious difficulty in the theory. It cannot be supposed that fusions take place except under favourable circumstances; but, if a swarm of meteorites does not degrade into dust, these favourable circumstances must occur often enough to counterbalance fractures.

Mr. Preston does not seem to be correct in respect to Clausius's theory of the constant ratio of internal to translatory energy in a gas. Clausius assumes, and does not prove that, in the average of a number of collisions, the molecule will absorb an amount of energy proportional to the mean violence of the blows with which it has been struck. Clausius's law must be at least approximately true within considerable variations of temperature, but it is certainly not a rigorous law of mechanical systems.

The case of meteorites in collision is totally different; they are incapable of taking up more than an infinitesimal amount of vibratory energy. The energy which, in the molecules of a gas, is absorbed, in the case of meteorites goes in volatilizing solids. There seems to be no reason why the particular ratio which Clausius determines from the numerical value of the ratio of the two specific heats (γ) in a gas, should hold good in a swarm of meteorites. We have no idea of the ratio of the two quasi-specific heats in a swarm of meteorites, and therefore we are not entitled to use the equation $\beta = \frac{2}{3}\frac{1}{\gamma - 1}$, and to put $\gamma = 1{\cdot}4$, deducing $\beta = \frac{5}{3}$.[1] In other words, I do not see room for making any deduction as to the ratio between the kinetic energy of a swarm of meteorites and the thermal energy existing in the volatilized gases.

I fail to see any parallelism between Clausius's law and the conclusion drawn as to the lost potential energy in the formation of a swarm of meteorites. The loss of energy is an outcome of the law of gravitation, as applicable to the formation of a gaseous star, and is deducible without any reference to the kinetic theory of gases, or to its analogue for meteorites.

G. H. DARWIN.

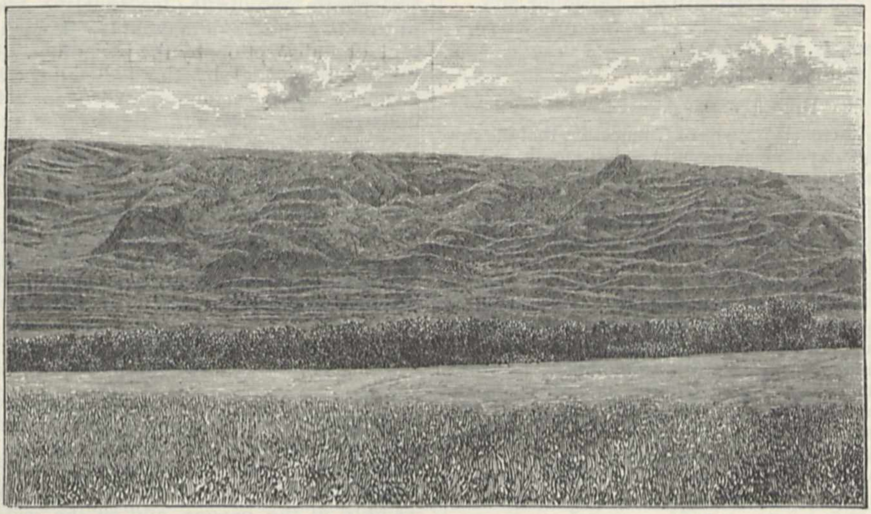
[1] Maxwell, "Theory of Heat" (1875), p. 317. Mr. Preston's statement of the law is inaccurate.

The Formation of Ledges on Hill-sides.

IN NATURE for February 28, Mr. Ernst draws attention to the formation of these ledges as observed by himself in Caracas. They are probably to be found in many places, if carefully looked for. The following diagram, taken from a photograph, illustrates one of the most striking instances I know, to be found near Ballantrae, on the Ballantrae-Girvan road, Ayrshire. The ledges, which are very numerous and fairly regular, occur on the western face of a series of low hills, very near to the sea-shore. The subsoil is thin and open. The angle of slope ranges from perhaps 30° to 60°, seldom higher. Where the angle is much higher, the soil slips away bodily, and the grass with it, leaving a bare space; indeed, at one point of the road the precipitated soil forms readily visible mounds at the base of the cliff.

The whole locality is very unfavourable to earthworms, and I agree with Mr. Ernst that the earthworm theory must, as far as any practical effect is concerned, be surrendered.

On the other hand, had the ridges been due to anything like glacier action, as Mr. Ernst suggests, I should have expected



detrital mounds below the ledges at the foot of each slope. These, however, do not occur, and the soil meets the narrow strip of plain with surprising angular sharpness.

It seems clear that the ledges owe their origin to the action of rain-water, which would naturally penetrate below the surface covering of grass, and dissolve with comparative rapidity portions of the porous soil below. The grass layer would eventually have nothing to support it in places, and would collapse to a lower level. The effect of collapse, supposing the layer to hold together, would necessarily be a wrinkle or ledge at right angles to the ground slope. EDMUND J. MILLS.

Glasgow, March 4.

Weight, Mass, and Force.

IF Mr. Gray, as in his teaching he no doubt unconsciously often does, will always say "force of a pound" instead of "weight of a pound" when he wishes to express the force of attraction of the Earth on a pound weight, there will be no divergence between his theoretical instruction and the language of practical men and of every-day life.

But to the majority the expression "weight of a pound" will always call up the mental picture of a "pound weight," so that the idea of the mass of a pound and of the force with which it is attracted by the earth cannot be dissociated in the use of the word "weight."

Supposing, however, we accept the definition of the "weight of a body" as never meaning anything else than the "force with which the earth attracts the body," how are we to interpret "the weight of the Sun, of the Moon, of Jupiter, &c.," and what is the "weight of the Earth?"

As Mr. Gray declines my previous challenge, will he condescend to point out the fallacy in the following argument? "The weight of the Moon being the force with which the Moon is attracted by the Earth, *ergo*, by the law that Action and Reaction are equal and opposite, the weight of the Earth is equal to the weight of the Moon."

With our present system of instruction in elementary theoretical dynamics we run the risk of wasting our time on a mechanics which is as unreal as is the mediæval Greek grammar taught in our schools, a grammar that was never vernacular even in the palmiest days of Attic literature.

The warning note in the introduction to "Numerical Examples in Practical Mechanics," by R. G. Blaine, is well timed and deserves careful attention. A. G. GREENHILL.

The Inheritance of Acquired Characters.

A VERY strong *a priori* objection to the line on which most experiments on the inheritance of acquired characters are carried on is the following. These experiments involve mutilation; and a tendency to transmit characters so produced would, considering that every accident or fight produces some slight mutilation, involve the animals in a process of degeneration. Hence the tendency to transmit the characters acquired *by mutilation* would be constantly bred out by natural selection. But a tendency to transmit characters acquired *by habit* in youth rests on quite another basis, and would tend to the conservation of the race.

I do not know if observations have been made on the physique of the offspring of persons engaged in trades where apprenticeship begins before puberty: they would be most valuable.

But the following case seems to me to be thoroughly to the point. A. B. is moderately myopic and very astigmatic in the left eye; extremely myopic in the right. As the left eye gave such bad images for near objects, he was compelled in childhood to mask it, and acquired the habit of leaning his head on his left arm for writing, so as to blind that eye; or of resting the left temple and eye on the hand, with the elbow on the table. At the age of fifteen the eyes were equalized by the use of suitable spectacles, and he soon lost the habit completely and permanently. He is now the father of two children—a boy and a girl—whose vision (tested repeatedly and fully) is emmetropic in both eyes, so that they have not inherited the *congenital* optical defect of their father. All the same, they both have inherited his early acquired habit, and need constant watchfulness to prevent their hiding the left eye, when writing, by resting the head on the left fore-arm or hand. Imitation is here quite out of the question.

Considering that every habit involves changes in the proportional development of the muscular and osseous systems, and hence, probably, of the nervous system also, the importance of inherited habits, natural or acquired, cannot be overlooked in the general theory of inheritance. I am fully aware that I shall be accused of flat Lamarckism; but a nickname is not an argument. MARCUS M. HARTOG.

Cork, March 6.

A Fine Meteor.

A FINE meteor was visible here to-night at 6.36 p.m. It fell perpendicularly almost due north-north-east, disappearing about 20° above the horizon, and was then as nearly as possible of the brilliancy and colour of Venus, which was shining in the south-west at the time. Length of path, I think, about 20°, but I am not positive that I saw the beginning of it.

B. WOODD SMITH.

Hampstead Heath, N.W., March 11.

Bishop's Ring.

I AM informed by Miss E. Brown, of Cirencester, that she saw Bishop's ring in full day-time as recently as last month, not far from 12 o'clock one day, the sun being hidden behind a cloud at the time. It appeared very similar in extent, as well as colour, though not intensity, to what it did after the Krakatão eruption. Also another day she noticed something like it when looking at the sun simply through a dark glass.

I have very occasionally myself in the last few years seen a somewhat similar phenomenon, but duller and dirtier in colour than Bishop's ring usually was, and which gave me the impression of being lower down in the atmosphere than that. I attributed this to smoke or some other local impurity. It would appear that the phenomenon seen by Miss Brown, out in the country, cannot have been thus caused; but I suppose that she must really have seen the corona produced by volcanic dust, through an exceptionally pure lower atmosphere.

Sunderland, March. T. W. BACKHOUSE.

The Philosophical Transactions.

MOST people who interest themselves in science would be glad to possess a complete set of the Philosophical Transactions. But in the first place complete sets are scarce, and in the second it would be much too expensive for ordinary people; furthermore, it is not in everyone's library that room could be found for its reception. Now, Drs. Hutton, Shaw, and Pearson brought out a first-rate abridgment of this valuable publication from its commencement to the end of the year 1800. May I venture to suggest that the Royal Society would be doing a good work by publishing on the same lines a continuation up to the year 1900? If they commenced now, and brought out a volume at intervals, the whole thing might be completed in the early years of the twentieth century. The cost ought not to be very great, and probably nearly every free library would subscribe. S.

ON THE COMPOSITION OF WATER.[1]

DURING the past year I have continued the work described in a former communication on the relative densities of hydrogen and oxygen (Roy. Soc. Proc., February 1888, vol. xliii. p. 356; see also NATURE, vol. xxxvii. p. 418), in the hope of being able to prepare lighter hydrogen than was then found possible. To this end, various modifications have been made in the generating apparatus. Hydrogen has been prepared from potash, in place of acid. In one set of experiments the gas was liberated by aluminium. In this case the generator consisted of a large closed tube sealed to the remainder of the apparatus; and the aluminium was attached to an iron armature so arranged that, by means of an external electro-magnet, it was possible to lower it into the potash, or to remove it therefrom. The liberated gas passed through tubes containing liquid potash,[2] corrosive sublimate, finely powdered solid potash, and, lastly, a long length of phosphoric anhydride. But the result was disappointing, for the hydrogen proved to be no lighter than that formerly obtained from sulphuric acid.

I have also tried to purify hydrogen yet further by absorption in palladium. In his recent important memoir (*Amer. Chem. Journ.*, vol. x. No. 4), "On the Combustion of Weighed Quantities of Hydrogen and the Atomic Weight of Oxygen," Mr. Keiser describes experiments from which it appears that palladium will not occlude nitrogen—a very probable impurity in even the most carefully prepared gas. My palladium was placed in a tube sealed, as a lateral attachment, to the middle of that containing the phosphoric anhydride; so that the hydrogen was submitted in a thorough manner to this reagent, both before and after absorption by the palladium. Any impurity that might be rejected by the palladium was washed out of the tube by a current of hydrogen before the gas was collected for weighing. But as the result of even this treatment I have no improvement to report, the density of the gas being almost exactly as before.

Hitherto the observations have related merely to the densities of hydrogen and oxygen, giving the ratio 15·884, as formerly explained. To infer the composition of water by weight, this number had to be combined with that found by Mr. Scott as representing the ratio of volumes. The result was—

$$\frac{2 \times 15 \cdot 884}{1 \cdot 9965} = 15 \cdot 914.$$

The experiments now to be described are an attempt at an entirely independent determination of the relative weights by actual combustion of weighed quantities of the two gases. It will be remembered that in Dumas's investigation the composition of water is inferred from the weights of the oxygen and of the water, the hydrogen being unweighed. In order to avoid the very unfavourable conditions of this method, recent workers have made it a point to weigh the hydrogen, whether in the gaseous state, as in the experiments of Prof. Cooke and my own, or occluded in palladium, as in Mr. Keiser's practice. So long as the hydrogen is weighed, it is not very material whether the second weighing relate to the water or to the oxygen. The former is the case in the work of Cooke and Keiser, the latter in the preliminary experiments now to be reported.

Nothing could be simpler in principle than the method adopted. Globes of the same size as those employed for the density determinations are filled to atmospheric pressure with the two gases, and are then carefully weighed. By means of Sprengel pumps the gases are exhausted into a mixing chamber, sealed below with mercury, and thence by means of a third Sprengel are conducted into a eudiometer, also sealed below with mercury, where they are fired by electric sparks in the usual way. After sufficient quantities of the gases have been withdrawn, the taps of the globes are turned, the leading tubes and mixing chamber are cleared of all remaining gas, and, after a final explosion in the eudiometer, the nature and amount of the residual gas are determined. The quantities taken from the globes can be found from the weights before and after operations. From the quantity of that gas which proved to be in excess, the calculated weight of the residue is subtracted. This gives the weight of the two gases which actually took part in the combustion.

In practice, the operation is more difficult than might be supposed from the above description. The efficient capacity of the eudiometer being necessarily somewhat limited, the gases must be fed in throughout in very nearly the equivalent proportions; otherwise there would soon be such an accumulation of residue that no further progress could be made. For this reason nothing could be done until the intermediate mixing chamber was provided. In starting a combustion, this vessel, originally full of mercury, was charged with

[1] A Paper read at the Royal Society, by Lord Rayleigh, Sec.R.S., on March 7.

[2] Of course, this tube was superfluous in the present case, but it was more convenient to retain it.

equivalent quantities of the two gases. The oxygen was first admitted until the level of the mercury had dropped to a certain mark, and subsequently the hydrogen down to a second mark, whose position relatively to the first was determined by preliminary measurements of volume. The mixed gases might then be drawn off into the eudiometer until exhausted, after which the chamber might be re-charged as before. But a good deal of time may be saved by replenishing the chamber from the globes simultaneously with the exhaustion into the eudiometer. In order to do this without losing the proper proportion, simple mercury manometers were provided for indicating the pressures of the gases at any time remaining in the globes. But even with this assistance close attention was necessary to obviate an accumulation of residual gas in the eudiometer, such as would endanger the success of the experiment, or, at least, entail tedious delay. To obtain a reasonable control, two sparking places were provided, of which the upper was situate nearly at the top of the eudiometer. This was employed at the close, and whenever in the course of the combustion the residual gas chanced to be much reduced in quantity; but, as a rule, the explosions were made from the lower sparking point. The most convenient state of things was attained when the tube contained excess of oxygen down to a point somewhat below the lower sparking wires. Under these circumstances, each bubble of explosive gas readily found its way to the sparks, and there was no tendency to a dangerous accumulation of mixed gas before an explosion took place. When the gas in excess was hydrogen, the manipulation was more difficult, on account of the greater density of the explosive gas retarding its travel to the necessary height.

In spite of all precautions several attempted determinations have failed from various causes, such as fracture of the eudiometer and others which it is not necessary here to particularize, leading to the loss of much labour. Five results only can at present be reported, and are as follows:—

December 24, 1888	15·93
January 3, 1889	15·98
,, 21, ,, ...	15·98
February 2, ,, ...	15·93
,, 13, ,, ...	15·92
Mean	15·95

This number represents the atomic ratio of oxygen and hydrogen as deduced immediately from the weighings with allowance for the unburnt residue. It is subject to the correction for buoyancy rendered necessary by the shrinkage of the external volume of the globes when internally exhausted, as explained in my former communication.[1] In these experiments, the globe which contained the hydrogen was the same (14) as that employed for the density determinations. The necessary correction is thus four parts in a thousand, reducing the final number for the atomic weight of oxygen to—

15·89,

somewhat lower than that which I formerly obtained (15·91) by the use of Mr. Scott's value of the volume ratio. It may be convenient to recall that the corresponding number obtained by Cooke and Richards (corrected for shrinkage) is 15·87, while that of Keiser is 15·95.

In the present incomplete state of the investigation, I do not wish to lay much stress upon the above number, more especially as the agreement of the several results is not so good as it should be. The principal source of error, of a non-chemical character, is in the estimation of the weight of the hydrogen. Although this part of the work cannot be conducted under quite such favourable conditions as in the case of a density determination, the error in the difference of the two weighings should not exceed 0·0002 gramme. The whole weight of the hydrogen used is about 0·1 gramme;[1] so that the error should not exceed 3 in the last figure of the final number. It is thus scarcely possible to explain the variations among the five numbers as due merely to errors of the weighings.

The following are the details of the determination of February 2, chosen at random:—

Before combustion ... $G_{14} + H + 0·2906 = G_{11}$ ... pointer 20·05
After ,, ... $G_{14} + H + 0·4006 = G_{11}$ ... pointer 20·31

Hydrogen taken = 0·1100 − 0·00005 = 0·10995 gramme.

Before combustion ... $G_{13} + O = G_{11} + 2·237$ ... pointer 20·00
After ,, ... $G_{13} + O = G_{11} + 1·357$ ... pointer 19·3

Oxygen taken = 0·8800 + 0·0001 = 0·8801 gramme.

At the close of operations the residue in the eudiometer was oxygen, occupying 7·8 cubic centimetres. This was at a total pressure of 29·6 − 16·2 = 13·4 inches of mercury. Subtracting 0·4 inch for the pressure of the water vapour, we get 13·0 as representing the oxygen pressure. The temperature was about 12° C. Thus, taking the weight of a cubic centimetre of oxygen at 0° C., and under a pressure of 76·0 centimetres of mercury to be 0·00143 gramme, we get as the weight of the residual oxygen—

$$0·00143 \frac{7·8}{1 + 12 \times 0·00367} \frac{13·0 \times 2·54}{76·0} = 0·0046 \text{ gramme.}$$

The weight of oxygen burnt was, therefore, 0·8801 − 0·0046 = 0·8755 gramme.

Finally, for the rate of atomic weight—

$$\frac{\text{Oxygen}}{\frac{1}{2}\text{ Hydrogen}} = 15·926.$$

In several cases the residual gas was subjected to analysis. Thus, after the determination of February 2, the volume was reduced by additions of hydrogen to 1·2 cubic centimetre. On introduction of potash there was shrinkage to about 0·9, and, on addition of pyrogallic acid, to 0·1 or 0·2. These volumes of gas are here measured at a pressure of $\frac{1}{3}$ atmosphere, and are, therefore, to be divided by 3 if we wish to estimate the quantities of gas under standard conditions. The final residue of (say) 0·05 cubic centimetre should be nitrogen, and, even if originally mixed with the hydrogen—the most unfavourable case—would involve an error of only $\frac{1}{2000}$ in the final result. The 0·1 cubic centimetre of carbonic anhydride, if originally contained in the hydrogen, would be more important, but this is very improbable. If originally mixed with the oxygen, or due to leakage through india-rubber into the combustion apparatus, it would lead to no appreciable error.

The aggregate impurity of 0·15, here indicated, is tolerably satisfactory in comparison with the total quantity of gas dealt with—2000 cubic centimetres. It is possible, however, that nitrogen might be oxidized, and thus not manifest itself under the above tests. In another experiment the water of combustion was examined for acidity, but without definite indications of nitric acid. The slight reddening observed appeared to be rather that due to carbonic acid, some of which, it must be remembered, would be dissolved in the water. These and other matters demand further attention.

The somewhat complicated glass-blowing required for the combustion apparatus has all been done at home by my assistant, Mr. Gordon, on whom has also fallen most of the rather tedious work connected with the evacuation of globes and other apparatus, and with the preparation of the gases.

[1] The necessity of this correction was recognized at an early stage, and, if I remember rightly, was one of the reasons which led me to think that a re-determination of the density of hydrogen was desirable. In the meantime, however, the question was discussed by Agamennone (*Atti* (*Rendiconti*) *d. R. Accad. dei Lincei*, 1885), and some notice of his work reached me. When writing my paper last year I could not recall the circumstances; but since the matter has attracted attention I have made inquiry, and take this opportunity of pointing out that the credit of first publication is due to Agamennone.

[1] It was usual to take for combustion from two-thirds to three-fourths of the contents of the globe.

EXAMINATIONS IN ELEMENTARY GEOMETRY.

IN the spring of 1870 a letter appeared in the columns of NATURE suggesting the formation of an Association which should have for its object the improvement of geometrical teaching. As to the great desirability of such improvement there could be no doubt. It was felt that one of the greatest obstacles in the way of effecting it was the nature of the examinations, and that no change in that nature was likely to be brought about except by the combined action of those who desired it The idea took root, and a circular was drawn up in which the objects of the proposed Association were stated to be—

(1) To collect and distribute information as to the prevailing methods of instruction in geometry practised in this and other countries, and to ascertain whether the desire for change was general.

(2) To use its influence to induce examining bodies to frame their questions in geometry without reference to any particular text-book.

(3) To stamp with its approval some text-book already published, or to bring out a new one under its own auspices.

The movement led to a conference at University College in 1871, which resulted in the formation of the Association for the Improvement of Geometrical Teaching. Many difficulties beset the way of the desired improvement. In addition to all the arguments which may be used against radical change in any department of affairs, the uncomprising supporters of Euclid (or rather of Simson's edition of his works) had the very cogent one that a Commission appointed by the Italian Government to inquire into the state of geometrical teaching in Italy found it "so unsatisfactory, and the number of bad text-books so great and so much on the increase, as to compel them to recommend the adoption of Euclid pure and simple,"[1] and were able to quote the authority of distinguished French mathematicians as to the great merits of Euclid as compared with modern French writers. We need not wonder, then, and need perhaps scarcely regret, that the outward and direct progress of the Association towards the realization of the aims set forth in its programme was slow.

One of its first steps was to attempt to draw up a Syllabus of Geometry in place of Euclid's scheme of propositions, which might form the basis of future text-books. Members were requested to send in, and the Committee of Management to report upon, programmes of the subjects which a text-book ought to include. Eleven programmes were sent in, which agreed upon many fundamental points; and after much work by a sub-committee, and much discussion from time to time at the general meetings of the Association, a Syllabus of Geometry was at last published as the work of the Association. It is worthy of remark that a Committee of the British Association, containing such eminent mathematicians as Profs. Cayley, Clifford, H. J. S. Smith, and Sylvester, appointed for the purpose of considering the possibility of improving the methods of instruction in elementary geometry, and reappointed to consider this Syllabus, reported that "it appears to have been drawn up with such care and with such regard to the essential conditions of the problem as to render it highly desirable that it should be considered in detail by authorized representatives of the Universities and other great examining bodies of the United Kingdom, with a view to its adoption, subject to any modification which such detailed consideration may show to be necessary, as the standard for examinations."

The secretaries then applied to many of the principal examining bodies, submitting the Syllabus for their consideration, with a view to its adoption as the basis of examination.

[1] See First Annual Report of the A.I.G.T.

The direct result of this application was small. It was felt that the non-existence of a text-book based on the Syllabus was a bar to its adoption, and a sub-committee was appointed to prepare proofs of the propositions. A text-book embodying these, and entitled "The Elements of Plane Geometry," was published by Messrs. Sonnenschein and Co. The appearance of Part II. of this work in 1886 seemed to afford a fitting opportunity for memorializing the Universities as to the advisability of relaxing their regulations. The following petition was drawn up, and received about 180 signatures, among which may be noticed those of Sir R. S. Ball, Prof. Chrystal, Prof. Henrici, Dr. Hirst, and Prof. Tait:—

"We, the undersigned members of the Association for the Improvement of Geometrical Teaching, and others interested in the teaching of elementary geometry, believing that greater freedom in the teaching of that subject than is consistent with a rigid adherence to the letter of Euclid's 'Elements' is highly desirable, would welcome such a change in the examinations in elementary geometry conducted by the Universities and other examining bodies as would admit of the subject being studied from text-books other than editions of Euclid, without the student being thereby placed at a disadvantage in those examinations."

The Council forwarded the petition to the Universities, praying them to take the subject of the petition into their favourable consideration, with a view to adapting the regulations for the examinations in elementary geometry conducted by the Universities so as to meet the desire for greater freedom felt by a large number of teachers, and supported by the judgment of many eminent mathematicians.

The Hebdomadal Council of the University of Oxford having referred the application of the Association to the Board of the Faculty of Natural Science, the Board reported as follows:—

(1) "That a rigid adherence to the ordinary text-books of Euclid should no longer be insisted on, but that a greater freedom of demonstration should be allowed, both in geometrical teaching and in examination.

(2) "That, nevertheless, Euclid's *method* should be required in all pass examinations in geometry in so far as that no axioms other than those of Euclid shall be admitted, and that no proof of a proposition be allowed which assumes the truth of any proposition which does not precede it according to Euclid's order.

(3) "That the University should not prescribe the use of any particular text-book or text-books."

This Report was adopted by the Hebdomadal Council on June 20, 1887.

The Senate of the University of Cambridge having referred to the Special Board for Mathematics, the Board reported as follows:—

"The majority of the Board are of opinion that the rigid adherence to Euclid's text is prejudicial to the interests of education, and that greater freedom in the method of teaching geometry is desirable. As it appears that this greater freedom cannot be attained while a knowledge of Euclid's text is insisted upon in the examinations of the University, they consider that such alterations should be made in the regulations of the examinations as to admit other proofs besides those of Euclid, while following, however, his general sequence of propositions, so that no proof of any proposition occurring in Euclid should be accepted in which a subsequent proposition in Euclid's order is assumed.

"To give effect to this view, the Board recommend that for Regulation 10 (2) of the previous examination (*Ordinationes*, p. 8) the following be substituted:—

"Elementary geometry, viz. the substance of the first three books, the definitions 1–10 of Book V., and the substance of the first nineteen propositions of the Sixth Book of Euclid's 'Elements.' Euclid's definitions will be required, and no axioms or postulates except Euclid's may be assumed. The actual proofs of propositions as given in Euclid will not be required, but no proof of any proposition occurring in Euclid will be admitted in which use is made of any proposition which in Euclid's order occurs subsequently."

The subject came on for discussion in the School on January 26, 1888, and a grace finally passed the Senate (March 8, 1888), making the alteration in the regulations for the previous examination recommended in the Report.

The Council of the Association then forwarded an Address to Her Majesty's Civil Service Commissioners, drawing their attention to the recent Reports of the University Boards, and asking for corresponding alterations in the regulations for the Woolwich and Sandhurst examinations, as follows:

"In view of the above facts, the Council feel themselves justified in inviting the attention of Her Majesty's Civil Service Commissioners to the desirability of a change in one of the existing regulations for the examinations conducted under their authority, especially those for admission to Sandhurst and Woolwich. By this a candidate is required to 'satisfy the Commissioners in Euclid, Books I.-IV. and VI.' The Council, having reason to believe that this regulation is very generally understood to imply that Euclid's text is required without any but mere verbal variations, beg to suggest that the subject should be defined as 'Elementary Geometry to the extent of Books I.-IV. and VI. of Euclid's "Elements,"' with a note to the effect that 'any proofs will be admitted which are themselves sound, do not assume other axioms than those of Euclid, and are not inconsistent with the logical sequence of Euclid's theorems.'

"The Council would prefer to have no particular text-book prescribed, and to have the candidate's general knowledge of geometry tested, rather than his power of reproducing Euclid's text.

"While making this request, however, the Council wishes to express its acknowledgment of the high character of the papers in Euclid set by the Civil Service Examiners, and particularly of the frequent introduction of stimulative exercises on the book work set.

"The Council would esteem it a favour if they were allowed to express their views on this important question more at large by a deputation from their body at an interview with the Commissioners."

The following reply was received from the Commissioners:—

"*Civil Service Commission, June* 30, 1888.

"Sir,—I am directed by the Civil Service Commissioners to acknowledge the receipt of a circular signed by you in behalf of the Council of the Association for the Improvement of Geometrical Teaching, requesting that certain changes may be introduced into the regulations for the examinations held under their authority, and in particular those for admission to Sandhurst and Woolwich;

"And, in reply, I am to acquaint you, for the information of the Council, that the regulations for the military examinations are framed and issued by the military authorities, and that the Commissioners have no power to alter them, but that the interpretation which the Commissioners have given them has been substantially in accordance with the views expressed by the Cambridge Special Board for Mathematics and the Oxford Board of the Faculty of Natural Science. They have, that is to say, instructed their occasional examiners to make no deduction from the marks allotted to a question because a candidate substitutes another proof for Euclid's, if this proof is a sound one and keeps to Euclid's sequence of propositions. To remove any misconception which may exist, the Commissioners propose to introduce a note to this effect in the examination papers. It will be understood, however, that they do not intend thereby to fetter their discretion of changing their practice whenever they may consider it expedient.

"In conclusion, while thanking the Council for their offer to express their views on this question more at large by the medium of a deputation from their body, the Commissioners think that the agreement of their opinion on this matter renders it unnecessary that the members of the Council should give themselves the trouble of an interview.

"I have the honour to be, Sir,
"Your obedient servant,
"E. POSTE."

Doubtless these concessions are in form the sorriest minimum of change that could well be granted; but the spirit of the Reports is excellent, particularly if examiners allow themselves, as it is quite open for them to do, to interpret "Euclid's order" as his *logical* and not merely his *numerical* order. Probably, if there is a strong desire for further relaxation generally felt and expressed by teachers, there will be no great difficulty in getting it granted; but it is by no means clear that such a desire exists at present to a sufficient degree to bring pressure to bear on the authorities. One does not easily see why the sequence of the Syllabus and the "Elements" should not have been granted as an alternative to that of Euclid, as is done by the University of Edinburgh. These works are the outcome of considerable discussion and deliberation by practised teachers, and it seems scarcely right to ignore them so entirely. As far as Cambridge is concerned, there is one detail of reform which might be effected without going beyond the terms of the grace of the Senate; *i.e.* the authorization of a list of additional propositions not contained in Euclid's text, to be freely used in the demonstration of all propositions subsequent to the position they may be regarded as occupying in Euclid's sequence. Such a list need not be long, and its adoption would be a boon to those teachers who wish to use the Syllabus.

ELECTROSTATIC MEASUREMENT.[1]

THE number of electrostatic units of potential or electromotive force in the electro-magnetic unit of potential is essentially a velocity, and experiments have proved it to be so nearly equal to the velocity of light that from all the direct observations hitherto made we cannot tell whether it is a little greater than, or a little less than, or absolutely equal to, the velocity of light.

Sir W. Thomson is engaged now in making a set of electrometers which will measure by electrostatic force potentials of from 40 volts to 50,000 volts. The standardization of these instruments up to 200 or 300 volts is made exceedingly easy, by aid of his centiampere balance and continuous rheostat, with a voltaic battery of any kind, primary or secondary, capable of giving a fairly steady current of $\frac{1}{10}$ of an ampere through it and the platinoid resistance in series with it. The accuracy of the electro-magnetic standardization, within the range of the direct application of this method, is quite within $\frac{1}{20}$ per cent. A method of multiplication by aid of condensers, which was explained, gives an accuracy quite within $^1/_5$ per cent. for the measurement in volts up to 2000 or 3000 volts; and with not much less accuracy, by aid of an intermediate electrometer, up to 10,000 volts. Such a potential as 10,000 volts is convenient for measurement by an absolute electrostatic balance, which was fully explained in the lecture, and illustrated by a drawing. But hitherto he has not been able to make sure of the absolute accuracy of the electrostatic balance to closer than $^1/_2$ per cent. The results of a great number of measurements made in the Physical Laboratory of the University of Glasgow during the last

[1] Abstract of part of Sir William Thomson's Royal Institution Lecture of Friday evening, February 8, relating to the velocity of light.

two months, give the required number, commonly called "v," within $^1/_2$ per cent. of 300,000 kilometres per second; the velocity of light is known to be within $^1/_4$ per cent. of 300,000 kilometres per second. Results of previous observers for determining "v" had almost absolutely proved at least as close an agreement with the 300,000,000 metres as this. He expressed his obligations to his assistants and students in the Physical Laboratory of Glasgow University, Messrs. Meikle, Shields, Sutherland, and Carver, who worked with the greatest perseverance and accuracy, in the laborious and often irksome observations by which he had attempted to determine "v" by the direct electrometer method, as exactly as, or more exactly than, it has been determined by other observers and other methods.

The measurement of "v" by Sir William Thomson and Profs. Ayrton and Perry, communicated to the British Association at Bath, was too small (292) on account of the accidental omission of a correction regarding the effective area of the attracted disk in the absolute electrometer. When this correction is applied their result is brought up to 298, which exactly agrees with Profs. Ayrton and Perry's previous determination by another method, in Japan. Prof. J. J. Thomson's result is 296·3. It is understood that Rowland has found 299. The result of Sir William Thomson's latest observations, founded wholly on the comparison of electrometric and electro-magnetic determinations of potential in absolute measure, is 30·1 legal ohms, or 30·04 Rayleigh ohms. Assuming, as is now highly probable, that the Rayleigh ohm is considerably nearer than the legal ohm to the true ohm, the result for "v" is 300,400,000 metres per second. Sir William does not consider that this result can be trusted as demonstrating the truth within $^1/_3$ per cent.

NOTES.

THE Royal Society's Bakerian Lecture for the present year is to be "On a Magnetic Survey of the British Isles for the epoch January 1, 1886," by Prof. A. W. Rücker, F.R.S., and Prof. T. E. Thorpe, F.R.S.

CAPTAIN W. J. L. WHARTON, R.N., F.R.S., Hydrographer to the Admiralty, has been elected a member of the Athenæum Club, in accordance with the rule which empowers the Committee to elect annually a certain number of persons of distinguished eminence in literature, science, or art, or for public services.

THE American Association for the Advancement of Science will meet this year at Toronto from August 27 to September 3. It is expected that the attendance will rival that at the Boston meeting of 1880. The President of the Association is Prof. Mendenhall. Major Powell, as retiring President, gives the address.

THE French Association for the Advancement of Science will meet at Paris from August 8 to 15.

THE French Ministry of Public Instruction has decided to create a laboratory of pathological physiology at the École des Hautes Études, Paris. The Director will be M. François-Franck, assistant of M. Marey at the Collège de France.

DR. SELMAR SCHÖNLAND, of the Botanic Garden, Oxford, has been appointed to the Curatorship of the Albany Museum, Grahamstown, Cape Colony.

IN the spring, M. Hasselberg will go from Pulkowa to Stockholm, having been elected Fellow of the Royal Academy of Sciences, and Director of the Physical Institution of the Academy, in succession to Prof. Edlund.

THE Bombay Anthropological Society has resolved to communicate with the executors of Mr. E. T. Leith, the founder o the Society, with a view to secure the publication of his literary remains under the supervision and at the expense of the Society.

FURTHER experiments are to be carried out shortly at Chatham by the Balloon Department with the Bruce system of electric balloon signalling. The apparatus used will be that which the Government purchased of the inventor, Mr. Eric Stuart Bruce.

WE regret to hear of the death of Captain John Ericsson, the famous Swedish engineer. He died the other day at New York, at the age of eighty-six. Captain Ericsson was a man of extraordinary intellectual resource, and his name stands high among the great inventors of the present century. An effective screw-propeller was invented independently both by him and by Francis Pettit Smith. Smith's patent was taken out in May 1836, a little before Ericsson's; but the date of Ericsson's invention was considerably earlier than that of his English rival. The screw-propeller of Ericsson was at once adopted by the United States Navy, but in England he had the mortification of being officially informed that it was useless, because, "the power being applied at the stern, it would be absolutely impossible to make the vessel steer." Afterwards he had occasion to learn that an inventor's difficulties may be not less formidable in the New World than in the Old, for he was badly treated in connection with the *Princeton* screw-steamer, designed by him for the United States Government in 1844, and in connection with the *Monitor*, which he built during the Civil War. During his last years he was much occupied with what he called "the sun motor," an article on which, by himself, recently appeared in NATURE (vol. xxxviii. p. 319).

DR. JOHN CALL DALTON, the physiologist, died at New York on February 12, at the age of sixty-four.

AFTER the ordinary meeting of the Royal Meteorological Society, at 25 Great George Street, Westminster, on Wednesday, the 20th inst., the Fellows and their friends will inspect the Society's tenth annual Exhibition of Instruments. The Exhibition will remain open until Friday, the 22nd inst. It promises to be very interesting and instructive. It will be specially devoted to actinometers and solar radiation apparatus, but will also include several new instruments, and a number of photographs of flashes of lightning, clouds, &c. Persons, not Fellows, wishing to visit the Exhibition, can obtain tickets on application to Mr. W. Marriott, Royal Meteorological Society, 30 Great George Street, S.W.

WE have received, from the Meteorological Reporter to the Government of India, the "Registers of Original Observations" made at seven selected observatories during the months of January to July 1888. These observations, although not exactly in the form prescribed, are published in pursuance of a decision of the Meteorological Congress held at Vienna in 1873, that each country should publish daily observations for a certain number of stations. The Indian observations have been published in this monthly form since January 1879, while for the years 1875–78 they formed an appendix to the Annual Reports. They contain complete information of all the principal elements for four hours daily, together with daily means, and monthly means for each of the four hours. The registers also contain means of the chief elements for each hour of the day at Alipore (Calcutta), and the hourly movement of the wind at Lucknow and Nagpur. The more rapid dissemination of the information by the publication of monthly parts, instead of annual volumes, renders it more valuable to persons interested in meteorological investigations.

M. KLOSSOVSKI, who published some time ago an important work on the thunderstorms of Russia, has now made another valuable contribution to the meteorology of South

Russia, dealing with the amounts of rain and snow in the Governments of Kherson and Bessarabia (Memoirs of the Odessa Society of Naturalists, vol. xiii. Part 1). Pluviometric observations were made in 1887 at 158 stations, and 1160 descriptions of thunderstorms have been sent in. It appears that the average yearly amount of rain and snow in the Government of Kherson is about 411 millimetres (434 millimetres at Odessa), a line drawn from the mouth of the Dniester to Orenburg separating the regions which receive more than 400 millimetres of rain every year from those in the south which have less than that. The Caucasus shore of the Black Sea, with its heavy rains (1500 to 2000 millimetres every year), makes, of course, an exception to the rule. The rains and the snow are usually brought to Bessarabia by cyclones, those which come from the south bringing with them the heaviest rains. No fewer than 96 per cent. of the cyclones which come from Hungary and the Balkan Peninsula are sure to bring with them more or less heavy rains to Kherson, and the like is true of 51 per cent. of those which follow a course to the south of Odessa altogether. The approach of cyclones can be easily foreseen, but the changes of weather are so sudden in South Russia that storm-warnings ought to be given from Odessa instead of St. Petersburg.

AN earthquake was noticed at Kasina, in Croatia, on February 23. At Aquila, in the Abruzzi, on February 28, five shocks occurred between 4 and 5 a.m. No damage was done.

A SLIGHT shock of earthquake occurred in Eastern Pennsylvania on March 8. It was felt at Lancaster, Harrisburg, Gettysburg, and their environs, and also at Wilmington (Delaware), and several places in Maryland and New Jersey.

ANOTHER beautiful instance of the formation of geometrical isomers, compounds precisely similar in constitution, and differing only in the relative position of their atoms in space, has been discovered by Prof. Wislicenus and Herr Hölz. The compound in question is dibromide of crotonylene, $CH_3—CBr{=}CBr—CH_3$. Crotonylene itself, $CH_3—C{\equiv}C—CH_3$, is the third member of the acetylene series of hydrocarbons, and combines directly with bromine to form a dibromide of the above constitution, which is now shown to have its atoms arranged in space in the manner

$$\begin{array}{l} CH_3—C—Br \\ \quad\;\; \| \\ CH_3—C—Br \end{array}$$

, the two similar groups being symmetrical to an imaginary plane lying between them. On attempting, however, to prepare crotonylene dibromide from the tetrabromide by abstraction of two atoms of bromine, or from one of the tribrombutanes, $CH_3—CHBr—CBr_2—CH_3$, by splitting off a molecule of hydrobromic acid, quite a different substance was obtained. Analyses indicated exactly the same empirical formula as before, but the boiling-point was found to be about 3° higher than that of the first isomer, and its behaviour with zinc dust was markedly different. These facts can only be explained on the supposition that its arrangement in space differs in being centro-symmetrical,

$$\begin{array}{l} CH_3—C—Br \\ \qquad\;\; \| \\ Br—C—CH_3 \end{array}$$

, and this second isomer is therefore termed iso-crotonylene dibromide. Both compounds combine directly with a further quantity of bromine to form the same crystalline tetrabromide, $CH_3—CBr_2—CBr_2—CH_3$. In order to prepare the ordinary compound, the equivalent of a molecule of liquid bromine is allowed to drop from the fine jet of a dropping-funnel into a quantity of crotonylene contained in a flask surrounded by a freezing mixture, as crotonylene boils at 18° C. The reaction is very violent, each drop producing loud hissing; the product is washed with soda, dried over calcium chloride, and distilled. After two fractionations, the dibromide is obtained boiling constantly between 146° and 147°. On the addition of another molecule of bromine, the whole solidifies in large octahedral crystals of the tetrabromide. The iso-compound is best obtained by dropping in a similar manner one molecular equivalent of bromine into cooled monobrom-butylene; distillation of the resulting product yields tribrom-butane. This is diluted with alcohol, and a molecular equivalent of sodium ethylate added: a most violent reaction again occurs with elimination of a molecule of hydrobromic acid. On addition of water, the iso-compound is precipitated as an oil, which, on drying and subsequent distillation, is found to boil constantly between 149° and 150°. Addition of another molecule of bromine resulted in the production of octahedral crystals of the tetrabromide identical with those given by the ordinary compound. On reduction of each of the compounds in alcoholic solution with zinc dust, the plane-symmetrical ordinary compound is much more rapidly attacked than the centro-symmetrical iso-dibromide; when the reaction in each case was allowed to proceed under the same conditions for one hour, 99·6 per cent. of the ordinary compound was converted back to crotonylene, while only 60·6 per cent. of the iso- had been attacked.

THE Bureau des Longitudes has just issued an extract from the *Connaissance des Temps* (1890), for the use of schools of hydrography and aspirants to the grade of captain, either for coasting or ocean service, a recent resolution of the Minister of Marine having rendered such an extract very desirable. It contains the tables of the rising and setting of the sun and moon, and the place of the sun and sidereal time at mean noon for each day of the year. The moon's position is given only for every twelve hours throughout the year, instead of for every hour as in the complete edition, and the tables of lunar distances have been reduced to one star for each day. The positions of only thirty of the principal stars are given, as against 300 in the complete edition. The tables of refraction, and corrections for parallax, are reproduced in full, as are also the tables for conversion of mean time to sidereal time. The positions of the planets, Jupiter's satellites, &c., have been entirely omitted. Useful extracts from the data relating to tides already published in the *Annuaire des Marées* are given, with instructions for their use. The "establishment of the port" for every important port in the world is given, as well as the unit from which the height of the tide can be calculated. Our own authorities would do well to follow the example of the Bureau des Longitudes, and publish a similar extract from the *Nautical Almanac*.

THE Director of the Marine Biological Association reports that the breeding season of a large number of marine animals has begun, and that embryologists may find ample opportunities for study at the sea-side. Of Teleostean fishes, the spawning season of the herring has passed some time since, that of the plaice and flounder is just over, whilst the whiting, whiting pout, and merry sole (*Arnoglossus megastoma*) are now ripe. Nudibranch Mollusks are now visiting the shores to deposit their eggs, and large specimens of *Doris tuberculata* and *Eolis papillosa* may commonly be taken on the rocks at Plymouth. The uncommon *Capsilus hungaricus* has been found with ova attached. Among the Crustacea the common shrimp, the prawn, and *Pandalus annulicornis* are hatching out, and the surface net begins to show numerous zoeæ of *Porcellana longicornis*. The Nauplii of Balanus and of Copepods are also abundant at this season of the year. The trochosphere larvæ of Chætopods (*Terebella* and *Phyllodon*) are beginning to make their appearance, and ova and larvæ of Echinoderms are very abundant, particularly the ova of Ophiura, and Bipinnaria and Pluteus larvæ of Asterids and Echinids. The gonophores of the Hydrozoa are for the most part well developed, and filled with ripe ova during the coming month.

AT a meeting of the Royal Botanic Society on Saturday, a branch of coffee thickly set with ripe fruit from a plant growing in the Society's conservatory was shown. Dr. Prior mentioned as a curious fact that in parts of Abyssinia the fleshy outside husk only was eaten, the part we use being thrown away as worthless.

In the February number of *Himmel und Erde*, Herr O. Jesse, of the Berlin Astronomical Observatory, calls attention to the importance of an accurate study of the luminous night-clouds which, since 1885, have been visible in Europe in the months of June and July. Herr Jesse is of opinion that these phenomena are interesting from an astronomical as well as from a meteorological point of view, because their periodic movement, taken in conjunction with their extraordinary height, suggests that they manifest the activity of cosmical forces. He holds, therefore, that they may throw some light upon the question whether cosmical space is filled with a resisting medium, and that their action may be a repetition of occurrences which played a great part in the earlier period of the development of the earth and of the planets generally. He urges that the phenomena should be carefully observed, and appeals to all who may take this task in hand to send him the results of their observations.

Mr. A. T. Drummond, in recent Canadian publications, takes the view that Lake Superior is the most ancient of the St. Lawrence Great Lakes, dating back to Cambrian and, it may be, earlier times, and that whilst its waters at perhaps more than one period found an outlet to the ocean through the Mississippi valley, the lake formed in other ages one of the sources of a great river system which terminated on the Atlantic seaboard. He traces the course of this river from the Michigan basin, and from Lake Superior across Lake Huron to the headlands at the entrance to the Georgian Bay. Here its waters were hurled over the cliffs in a great fall more than rivalling Niagara. At the lower level another considerable stream joined it from the north, and the united rivers then skirted the face of the continuous, shaly, precipitous cliffs which cross Ontario to the Lake Ontario valley. Following the course of the escarpments which they created in this valley, the waters eventually reached the sea through the Mohawk and Hudson Rivers. At a recent period, the elevation of the land between the Georgian Bay and Lake Ontario blocked the course of the river, and, filling the Georgian Bay with water, created a new outlet, not by the present St. Clair River, which was of later birth, but to the south-eastward of Lake Huron, where, through a channel now buried by clays, the Erie basin was reached. The course from this basin to the Ontario valley was through the great fracture in the limestones at Hamilton, and not over the Niagara Falls. The change in the flow of these waters to the Niagara River was one of the opening episodes in the later history of Lake Erie. The Ottawa River was at this time a large stream flowing much in the same course as now, whilst the St. Lawrence was a less important river, taking its rise in the Adirondack Mountains, which, at their then greater elevation, blocked the present outlet of Lake Ontario.

Mr. Thomas Wilson, of the Smithsonian Institution, Washington, writing in the current number of the *American Naturalist*, gives a rather gloomy account of the treatment accorded by public authorities in the United States to science in general, and to archæology in particular. "The States of Ohio," he says, "or Wisconsin, or West Virginia, or Mississippi, not to mention New York or New England, have either of them within their borders as much unstudied, unsearched, and unclassified archæologic riches as has any one of the great countries of Europe: England, France, Germany, Spain, or Italy. Yet these countries, each of them, do more for archæology than equals the combined efforts of the United States and all the State Governments. I confess to a feeling of depression when, on visiting the Prehistoric Museum at Salisbury, England, I found there stored and displayed, in a beautiful building, erected in the midst of a lovely park, for its sole occupancy, the prehistoric collection of Squier and Davis, gathered by them from the mounds of the United States in the Ohio and Mississippi valleys. It went begging through the United States, knocked at the door of Congress, and besought a purchaser at the ludicrous price of $1000, but without finding a response. And in disgust with their countrymen, and in despair of ever being able to interest their Government or fellow-citizens, they sold their collection to England and retired from the field of archæologic investigation."

We learn from an article in *Science* that the pottery industry in the United States gives employment directly to about ten thousand people, to whom wages amounting annually to four million dollars are paid, this amount being nearly 50 per cent. of the total value of the output of the potteries. In addition to these, there are many thousand more employed in the preparation of the materials for the potters' use, such as mining the clays, quartz, and felspar, and grinding and washing the materials. To these people nearly as much more in wages is paid. According to *Science*, the American potter does not claim to be the peer of his foreign competitor in art productions, but he does claim to equal any foreign manufacturer in the class of china which he produces for the American people—both fine and common "crockery" for domestic uses. "To-day," says *Science*, "the English potter is copying American shapes, designs, and styles of decorations. How different is this state of affairs from that which existed a few years ago, when the American potter depended upon foreign ideas for his shapes and designs! With the development of the manufacturing process, talent for designing shapes and patterns or styles of decoration has likewise progressed, until we have made our own American shapes and designs, which foreigners have been compelled to copy and adopt in order to find a market for their wares in the United States."

In his "Butterflies of the Eastern United States," a part of which we lately reviewed, Mr. Scudder has an interesting paper —Excursus xxiii.—on mimicry and protective resemblance among butterflies. He points out that cases of mimicry are far more common in the tropics than in temperate regions, even relatively. The accounts of travellers in the tropics constantly mention the attacks of birds upon butterflies, while instances of butterflies being seen pursued by birds are vastly more rare in the temperate regions. Mr. Scudder himself has never seen one. In the tropics, moreover, there are many other insectivorous animals, such as lizards. "In our own country, therefore," says Mr. Scudder, "we should not look for many instances of mimicry of any decided type. The most striking is unquestionably that of *Basilarchia archippus*, which mimics *Anosia plexippus*, and the closely related case of *Basilarchia eros* and *Tasitia berenice*, the last two butterflies largely supplanting the first two on the peninsula of Florida. In both these instances the mimicry is enjoyed by both sexes. A third case is found in the less close but still striking mimicry of *Basilarchia astyanax* by the female of *Semnopsyche diana*, an instance the more remarkable as the mimicked species belongs to the same genus as our two other mimicking forms."

At a recent meeting of the Ceylon Branch of the Royal Asiatic Society, two papers were read by Dr. Trimen and Mr. A. P. Green, describing a visit paid by them to Ritigala. Dr. Trimen's paper was devoted to an account of the flora to be found on the mountain, and the difference between it and that of the surrounding country. Ritigala is the highest ground between the central mountains of Ceylon and the mountains of Southern India. It is only 2506 feet high; next to it in height in the central plain of Ceylon being Friar's Hood (2147 feet), Westminster Abbey (1829 feet), and Gunner's Quoin

(1736 feet). Ritigala is completely isolated, and its summit is frequently surrounded by mist, especially during the south-west monsoon—that is, when the plains are suffering severely from drought. The branches of the stunted trees on the mountain are covered with masses of *Meteorium* moss and lichens, like those on the high mountains. Unfortunately the long-continued drought had withered up much of the vegetation, and therefore the expedition was not as productive as it otherwise would have been.

CAPTAIN MOORE, of H.M.S. *Rambler*, has lately described—in a paper read before the China Branch of the Royal Asiatic Society—the appearance and effects of the remarkable "bore" which often occurs in Hangchow Bay. This dangerous visitor is the result of the struggle between the advancing tide in the great estuary and the current of the river. Captain Moore and his officers on several occasions observed the progress of the wave, and their investigations may be summarized as follows:—The rate at which the bore travels varies from ten to about thirteen miles per hour. The height of the bore rarely exceeds 12 or 14 feet, and broken water, in which no small boat could live, follows it for some distance. With the passing of the wave the tide rises many feet in a few seconds; in one instance, observed by Captain Moore, it rose from 9 feet 4 inches below to 4 feet 7 inches above mean level. The rush of the bore was so strong that the force of the waves breaking against the broadside of the *Rambler* sent the water into the mizzen chains and the spray on to the poop. The junks in that region are protected by platforms, with narrow steps cut in the sides. To the north of the estuary is a great sea-wall, built to protect the surrounding country from being flooded by these great tidal-waves. It is thirty-five miles long, and it is strengthened, where the bore strikes most strongly, by an elliptical stone buttress, 253 feet long by 63 feet wide. Behind this the junks are drawn up for shelter.

IN one of the recent American Consular Reports the preparation of Japanese *koji* (yeast) is described by Prof. Georgeson, of the Imperial Agricultural College at Tokio. *Koji* is made not only in special factories, but also in *saké* (rice-beer) breweries. The materials used are water, rice of the common starchy kind only, and *tane* (seed or leaven), the spores of a fungus (*Eurotium oryzeæ*, Ahlb.). It is this latter substance which, when germinating on the rice, changes a portion of the starch into dextrose and dextrin, and produces the fermentation. The rice is carefully washed, and the thin covering of the seed is always removed. It is then allowed to remain for some hours in water, and having been steamed, it is spread on mats to cool, and when the temperature has fallen to about 100° F., the *tane* is scattered uniformly on the mass and then thoroughly mixed with it. The whole is then allowed to remain for eighteen or twenty hours covered with mats, after which the rice is spread in a thin layer in a number of shallow, wooden trays, which are taken to the warmest room in the factory, and there left standing for four or five hours, when the contents are stirred by the hand, and again after an interval of four hours the operation is repeated. On the third day the fungus grows very rapidly, and great heat is generated. Care, however, is taken that the heat does not become too great. This is the usual mode, but there are many other methods.

THE additions to the Zoological Society's Gardens during the past week include a Guinea Baboon (*Cynocephalus sphinx*) from West Africa, presented by Mr. W. J. Vinton; a Valentyn's Phalanger (*Cuscus orientalis* ♂) from the Solomon Islands, presented by Mr. Chas. M. Woodford, C.M.Z.S; an Owen's Apteryx (*Apteryx oweni*), two Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, presented by Capt. C. A. Findlay, R.M.S. *Ruapehu*; two Nuthatches (*Sitta cæsia*), British, presented by Mr. J. Young, F.Z.S.; a Chimpanzee (*Anthropopithecus troglodytes* ♂) from West Africa, two Brown Cranes (*Grus canadensis*) from North America, three Black Swans (*Cygnus nigricollis*) from Australia, a Larger Hill-Mynah (*Gracula intermedia*) from India, deposited; a Hoffmann's Sloth (*Cholopus hoffmanni* ♀) from Panama, six Brent Geese (*Bernicla brenta*), European, purchased.

OUR ASTRONOMICAL COLUMN.

DISTRIBUTION OF SUN-SPOTS IN LATITUDE.—As remarked in the last issue of NATURE (p. 449), spots have been decidedly more numerous in the southern hemisphere of the sun during the last six years than in the northern. Since, however, the two hemispheres were about equally occupied in 1882, and the northern had the decided advantage in 1881 and 1880—whilst for nearly a quarter of a century previous there had been no long-continued difference between the two—it might be supposed that their present predominance in the southern hemisphere was one which would disappear in a series of observations spread over any great number of years. Prof. Spoerer, in a couple of valuable papers which he has just published—"Ueber die Periodicität der Sonnenflecken seit dem Jahre 1618," and "Sur les différences que présentent l'hémisphère nord et l'hémisphère sud du Soleil," the latter appearing in the *Bulletin Astronomique* for February—has given reason for believing that this is not the case, and that there have been at least two considerable periods, previous to the present one, in which the southern hemisphere was by far the more prolific in spots. The first of these was from 1621 to 1625, during which Scheiner's observations give us no spots in the northern hemisphere for 1621 and 1622, very few up to February 1625, and decidedly fewer than in the southern hemisphere until 1626, when a practical equality was established. The second period was in every way a more remarkable one, lasting for more than forty years. We have no record of any northern spots from 1672 to 1704; a few were seen in 1705, but their appearance there was looked upon as a most remarkable circumstance: Cassini and Maraldi recorded that they had never seen spots in the northern hemisphere before; and later, in 1714, on the occasion of the appearance of three northern spots, it was stated in a paper in the *Mémoires* of the Paris Academy that that hemisphere had been free from spots for forty years. This period, 1672–1713, seems to have been an exceptional one from several points of view. For much of the time there seem to have been scarcely any spots visible at all. Cassini, noting the third spot seen in 1676, remarks that in that year they had been more frequent than in the twenty preceding years. Flamsteed, in 1684, says that a spot he saw then was the first he had seen since 1676. Cassini, later on, states that the only spot seen since 1686 was that of May 1688, whilst in 1705 it is recorded ("Histoire de l'Académie," 1795, p. 128) that since Scheiner's observations, made sixty years before, two groups of spots had hardly ever been seen at the same time. Ten years later a similar remark is made; but, in 1716, spots became much more numerous, and as many as eight groups were seen at one time, from August 30 to September 3. The "law of zones," which Prof. Spoerer demonstrated for recent periods, the law that, from minimum to minimum, the spots show a tendency to seek lower and lower latitudes, breaking out afresh in high latitudes directly the next minimum is passed, seems to have been in abeyance during this remarkable period. The mean latitude seems to have been about 8° or 9°, but there was no regular drift downward made evident. The law, as Prof. Spoerer shows in the first of the above-named papers, appears to be unmistakably illustrated by Scheiner's observations at the time of the minimum of 1619, whilst the observations of 1643 and 1644 gave also a low mean latitude previous to the minimum of 1645. Observations at minima since the exceptional period of 1672–1713 supply many illustrations of the law of zones, as Prof. Spoerer takes occasion to demonstrate for the minima of 1755, 1775, 1784, 1833, and 1844.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *MARCH* 17–23.

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

At Greenwich on March 17

Sun rises, 6h. 11m.; souths, 12h. 8m. 23·5s.; sets, 18h. 7m.: right asc. on meridian, 23h. 49·4m.; decl. 1° 9′ S. Sidereal Time at Sunset, 5h. 48m.

Moon (Full on March 17, 12h.) rises, 18h. 10m.; souths, 0h. 42m.*; sets, 6h. 58m.*: right asc. on meridian, 12h. 24·6m.; decl. 2° 39′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	5 32	10 29	15 26	22 9·6	12 51 S.
Venus.....	6 55	14 44	22 33	2 25·1	19 21 N.
Mars.......	6 53	13 38	20 33	1 19·3	8 8 N.
Jupiter....	2 50	6 46	10 42	18 26·2	23 0 S.
Saturn....	13 47	21 25	5 3*	9 7·7	17 44 N.
Uranus...	20 14*	1 39	7 4	13 18·3	7 34 S.
Neptune..	8 27	16 11	23 55	3 52·3	18 32 N.

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

Mar.	h.	
18	21	Mercury at greatest distance from the Sun.
20	10	Sun in equator: day and night of equal length.

Variable Stars.

Star.	R.A.	Decl.		h. m.	
	h. m.	° ′			
ζ Geminorum	6 57·5	20 44 N.	Mar. 19,	21 10	*m*
R Canis Majoris	7 14·5	16 11 S.	,, 19,	18 49	*m*
			and at intervals of	27 16	
U Monocerotis	7 25·5	9 33 S.	Mar. 19,		*m*
V Cancri	8 15·4	17 38 N.	,, 23,		*M*
S Cancri	8 37·6	19 26 N.	,, 21,	6 16	*m*
R Leonis	9 41·6	11 57 N.	,, 23,		*M*
W Virginis	13 20·3	2 48 S.	,, 23,	23 0	*M*
U Coronæ	15 13·7	32 3 N.	,, 17,	22 15	*m*
β Lyræ	18 46·0	33 14 N.	,, 17,	20 0	*m*
			,, 21,	1 30	m_2
Y Cygni	20 47·6	34 14 N.	,, 17,	18 0	†*m*
			and at intervals of	36 0	
δ Cephei	22 25·0	57 51 N.	Mar. 17,	20 0	*M*
			,, 23,	5 0	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

† Y Cygni should be watched with especial care, as the time of minimum is subject to considerable uncertainty.

Meteor-Showers.

	R.A.	Decl.	
Near θ Ursæ Majoris	144	50 N.	March 20.
,, β Ursæ Majoris	162	57 N.	Slow.
,, ι Herculis	263	47 N.	

GEOGRAPHICAL NOTES.

THE paper on Monday night at the Royal Geographical Society was by the Hon. G. Curzon, M.P., and dealt with the Trans-Caspian Railway, over which Mr. Curzon recently travelled from the Caspian to Samarcand, a distance of 900 miles. Mr. Curzon described the structure of the railway, the engineering and other difficulties met with, the geographical features of the country traversed, and referred in some detail to the political, civilizing, and commercial results of the undertaking. The line is on a 5-foot gauge, which is uniform with the railway system of European Russia, but not with that of British India. The rails are of steel, from 19 to 22 feet long, and are laid upon wooden sleepers at the rate of 2000 sleepers to every mile, being simply spiked down without chairs or bolts. Every piece of timber, iron, and steel employed was brought from the forests or workshops of Russia, for the most part down the Volga and across the Caspian. The line is a single one from start to finish, except at the stations, where there are invariably sidings, and sometimes triangles, for an engine to reverse; it is laid upon a low earthwork or embankment thrown up with the soil scooped out of a shallow trench on either side. The permanent way is not metalled. It has been claimed that this railway is an astonishing engineering phenomenon, inasmuch as it traverses a country previously believed to be inaccessible to such a method of locomotion. But Mr. Curzon maintains, apart from the local lack of material due to the appalling dearth of the country, it is probably the easiest and simplest railway ever built. The region which it traverses is as flat as a billiard-table for almost the entire distance, the steepest gradient being 1 in 150. There are no tunnels, only a few insignificant cuttings, and but three bridges—across the Tejend, across the Murghab at Merv, and across the Amu-daria. The two main difficulties arose from scarcity of water and superabundance of sand. The former was not difficult to overcome, and the various means employed to check the destructive effects of sand will no doubt prove efficacious, though constant watchfulness along the whole line will be required. The really formidable sands are limited to three districts: (1) the first thirty miles from the Caspian; (2) the stretch between the Merv Oasis and the Oxus; and (3) the stretch between the Oxus and Bokhara. Here no vegetation is either visible or, with rare exceptions, possible; the sand, of the most brillant yellow hue, is piled in loose hillocks and mobile dunes, and is swept hither and thither by powerful winds. It has all the appearance of a sea of troubled waves, billow succeeding billow in melancholy succession, with the sand driving like spray from their summits, and great smooth-swept troughs lying between, on which the winds leave the imprint of their fingers in wavy indentations, just like an ebb tide on the sea-shore. Near the Caspian the permanent way was soaked with sea-water so as to give it consistency; in other parts it was covered with a sort of armour-plating of clay. Elsewhere, and in the more desolate regions, other plans were adopted. Light wooden palisades, 3 or 4 feet high, made of pine laths, were driven into the tops of the dunes and formed a barrier against which the winds might pile the sands with impunity. Nurseries for suitable desert plants were started in the Persian Mountains, and the product of these, tamarisk, wild oats, &c., were planted on the sand-hillocks contiguous to the line. Here, too, was planted that strange and interesting denizen of the wilderness the *Saxaoul* (*Haloxylon ammodendron*), which with a scanty and often ragged upper-growth, strikes its sturdy roots deep down into the sand, and somehow or other derives sustenance from that to which it gives stability and permanence. Fascines of the branches of this plant were also cut, laid at right angles to the rails along the edge of the earthwork or embankment, and covered over with a layer of sand. In spite of all these precautions, the sand must always constitute a serious danger to the line. In referring to Merv, and the miles and miles of ruins of the various old Mervs, Mr. Curzon gives the area of the oasis as 1600 square miles, with a population of not more than 100,000. The desert by which the oasis is surrounded is appalling. East and west, and north and south, stretches a troubled ocean of sand, each wave arrested, as it were, in mid career, when just curving to fall. Mr. Curzon never saw anything more melancholy than this wilderness with its sickle-shaped dome-like ridges of sand, succeeding each other with the regularity of infantry files. Each has the appearance of being cloven through the crown, the side facing towards the north-east, whence the prevailing winds blow, being uniform, convex, and smooth, while the southern face is vertical and abrupt. With regard to the famous bridge over the Oxus, Mr. Curzon states that its total length is 2000 yards, and that it rests on more than 3000 piles. The level of the rails is about 30 feet above low, but only 5 feet above high, water.

To the March number of the *Scottish Geographical Magazine*, an instructive paper on the Islands of Melanesia is contributed by Dr. R. H. Codrington. Mr. Ravenstein, in a paper accompanied by a map, on Lake Bangweolo, corrects the configuration of the lake based on a wrong interpretation of Livingstone's observations. Comparing them with subsequent results, Mr. Ravenstein shows that the length of the lake is north and south, and that the Luapula issues from its south-west corner. Mr. Ralph Richardson brings together a useful collection of data on the Edinburgh earthquake of January last.

THE Foreign Office has just published some extracts from a journal kept by Mr. W. J. Archer, British Vice-Consul at Chiengmai, or Zimmé, of a visit to Chiengtung, in May and June 1888. Mr. Archer traversed the little-known and mountainous region lying between the Rivers Salween and Cambodia, taking altogether thirty-six days for his journey both ways. From Chiengmai to Chienghai there are only a few towns, some of them inhabited by Luwas, the aborigines of the country. The hills, which abound in tigers, are uncultivated, but the valleys and low-lying grounds appeared well tilled, and bearing good crops of rice and paddy. North of Chienghai he saw the effects of attacks by the dacoits on the villagers. The former were Ngios from the Chiengtung territory, led by a Lao. Mr. Archer says that he was surprised to see what little traffic there was on the road, but this, perhaps, was due to some extent to the very heavy rains. The highest mountain-range of the whole region is a little south of Chiengtung, and is about 4400 feet high. After leaving Müang Hai, no signs of habitation were seen until Chiengtung was reached. As an instance of the insecurity of the country, he mentions that with one party he saw a young woman armed like the men. The plateau of Chiengtung is about 2700 feet above the sea-level, and is very bare and badly cultivated. The town itself is walled, and is situated in the southern corner of the plateau, which is inclosed by high mountains on the west, south, and east. The chief products are rice, cotton, and opium; tea is also cultivated on the hills. Cotton goods are imported from Moulmein; salt, silk, and other articles from Yünnan; cocoa-nuts and betel-nuts from Chienghai. The inhabitants of the district belong to several races; the majority are of the same people as the ruling family, but there are also numbers of Western Shans, of the hill tribes (the Kins, Kans, and Musös), and Lëms and Lüs. The houses of the people are as miserable as could be, and their great poverty is shown by the fact that, instead of their temples being made of brick, as is the case in Siam, they are wretched sheds, almost as miserable as the houses. Mr. Archer returned by a different road, and found whole districts uninhabited between Chiengtung and Muang Lën, which is close to the Cambodia River.

THE *Manchester Guardian* publishes some interesting information as to the movements of Dr. Lansdell. In October last he arrived at Khoten, whence he set out for Yarkand. He left Yarkand for Western Tibet and crossed the Kilian Pass at a height of 17,000 feet on December 2. On the 10th the Karakoram was passed without very much difficulty; but a few days later, when crossing the Saser, the party suffered very much from the intense cold. On November 14, Dr. Lansdell sighted at Changlung the first inhabited huts of the Tibetans. Crossing over Khardung Pass with great difficulty, he finally arrived at Leh, where he remained for a few days, setting out on December 5 to descend to Kashmir. To aid in this attempt, forty men were sent by the local authorities, and Zogiha Pass was crossed in safety, and Kashmir reached. In a few days Srinagur was reached, but Soultem Passes were blocked, and an attempt to penetrate them was abandoned. At Rawul Pindee, Dr. Lansdell's journey finished, he having travelled from Kuldja 2000 miles, crossing seven of the highest passes in the world.

THE Government Geologist who set out from Adelaide some months ago to explore Central Australia has just returned. He travelled as far north as Alice Springs—that is, to the Tropic of Capricorn—and spent nearly a fortnight there examining the ruby and gold fields. Between Anna Creek and Alice Springs the country is well watered, but at the diggings water is very scarce. In all, some 2400 miles were travelled, and, around the ruby fields, camels were employed. The heat was very great, on one occasion reaching as much as 124° F. in the shade.

THE DISCHARGE OF A LEYDEN JAR.[1]

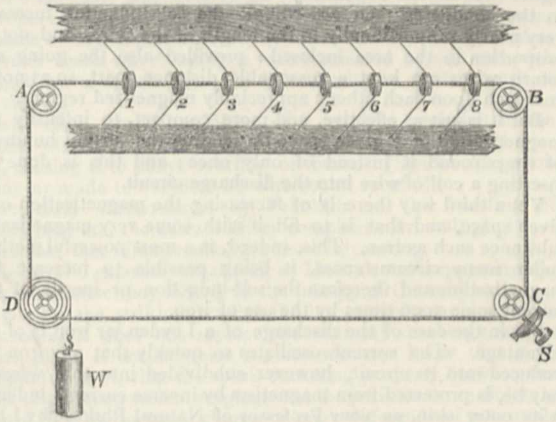
IT is one of the great generalizations established by Faraday, that all electrical charge and discharge is essentially the charge and discharge of a Leyden jar. It is impossible to charge one body alone. Whenever a body is charged positively, some other body is *ipso facto* charged negatively, and the two equal opposite charges are connected by lines of induction. The charges are, in fact, simply the ends of these lines, and it is as impossible to have one charge without its correlative as it is to have one end of a piece of string without there being somewhere, hidden it may be, split up into strands it may be, but somewhere existent, the other end of that string.

This I suppose familiar fact that all charge is virtually that of a Leyden jar being premised, our subject for this evening is at once seen to be a very wide one, ranging in fact over the whole domain of electricity. For the charge of a Leyden jar includes virtually the domain of electrostatics; while the discharge of a jar, since it constitutes a current, covers the ground of current electricity all except that portion which deals with phenomena peculiar to steady currents. And since a current of electricity necessarily magnetizes the space around it, whether it flow in a straight or in a curved path, whether it flow through wire or burst through air, the territory of magnetism is likewise invaded; and inasmuch as a Leyden jar discharge is oscillatory, and we now know the vibratory motion called light to be really an oscillating electric current, the domain of optics is seriously encroached upon.

[1] Friday evening discourse at the Royal Institution of Great Britain, on March 8, by Prof. Oliver J. Lodge, F.R.S.

But though the subject I have chosen would permit this wide range, and though it is highly desirable to keep before our minds the wide-reaching import of the most simple-seeming fact in connection with such a subject, yet to-night I do not intend to avail myself of any such latitude, but to keep as closely and distinctly as possible to the Leyden jar in its homely and well-known form, as constructed out of a glass bottle, two sheets of tinfoil, and some stickphast.

The act of charging such a jar I have permitted myself now for some time to illustrate by the mechanical analogy of an inextensible endless cord able to circulate over pulleys, and threading in some portion of its length a row of tightly-gripping beads which are connected to fixed beams by elastic threads.



Mechanical analogy of a circuit partly *dielectric*; for instance, of a charged condenser. A is its positive coat, B its negative.

The cord is to represent electricity; the beads represent successive strata in the thickness of the glass of the jar, or, if you like, atoms of dielectric or insulating matter. Extra tension in the cord represents negative potential, while a less tension (the nearest analogue to pressure adapted to the circumstances) represents positive potential. Forces applied to move the cord, such as winches or weights, are electromotive forces; a clamp or fixed obstruction represents a rheostat or contact-breaker; and an excess or defect of cord between two strata of matter represents a positive or a negative charge.

The act of charging a jar is now quite easily depicted as shown in the diagram.

To discharge the jar one must remove the charging E.M.F. and unclamp the screw, *i.e.* close the circuit. The stress in the elastic threads will then rapidly drive the cord back, the inertia of the beads will cause it to overshoot the mark, and for an instant the jar will possess an inverse charge. Back again the cord swings, however, and a charge of same sign as at first, but of rather less magnitude, would be found in the jar if the operation were now suspended. If it be allowed to go on, the oscillations gradually subside, and in a short time everything is quiescent, and the jar is completely discharged.

All this occurs in the Leyden jar, and the whole series of oscillations, accompanied by periodic reversal and re-reversal of the charges of the jar, is all accomplished in the incredibly short space of time occupied by a spark.

Consider now what the rate of oscillation depends on. Manifestly on the elasticity of the threads and on the inertia of the matter which is moved. Take the simplest mechanical analogy, that of the vibration of a loaded spring, like the reeds in a musical box. The stiffer the spring, and the less the load, the faster it vibrates. Give a mathematician these data, and he will calculate for you the time the spring takes to execute one complete vibration, the "period" of its swing. [Loaded lath in vice.]

The electrical problem and the electrical solution are precisely the same. That which corresponds to the flexibility of the spring is in electrical language called static capacity, or, by Mr. Heaviside, permittance. That which corresponds to the inertia of ordinary matter is called electro-magnetic inertia, or self-induction, or, by Mr. Heaviside, inductance.

Increase either of these, and the rate of oscillation is diminished. Increasing the static capacity corresponds to lengthening the spring; increasing the self-induction corresponds to loading it.

Now the static capacity is increased simply by using a larger jar, or by combining a number of jars into a battery in the very old-established way. Increase in the self-induction is attained by giving the discharge more space to magnetize, or by making it magnetize a given space more strongly. For electro-magnetic inertia is wholly due to the magnetization of the space surrounding a current, and this space may be increased or its magnetization intensified as much as we please.

To increase the space we have only to make the discharge take a long circuit instead of a short one. Thus we may send it by a wire all round the room, or by a telegraph wire all round a town, and all the space inside it and some of that outside will be more or less magnetized. More or less, I say, and it is a case of less rather than more. Practically very little effect is felt except close to the conductor, and accordingly the self-induction increases very nearly proportionally to the length of the wire, and not in proportion to the area inclosed: provided also the going and return wires are kept a reasonable distance apart, so as not to encroach upon each other's appreciably magnetized regions.

But it is just as effective, and more compact, to intensify the magnetization of a given space by sending the current hundreds of times round it instead of only once; and this is done by inserting a coil of wire into the discharge circuit.

Yet a third way there is of increasing the magnetization of a given space, and that is to fill it with some very magnetizable substance such as iron. This, indeed, is a most powerful method under many circumstances, it being possible to increase the magnetization and therefore the self-induction or inertia of the current some 5000 times by the use of iron.

But in the case of the discharge of a Leyden jar iron is of no advantage. The current oscillates so quickly that any iron introduced into its circuit, however subdivided into thin wires it may be, is protected from magnetism by inverse currents induced in its outer skin, as your Professor of Natural Philosophy [1] has shown, and accordingly it does not get magnetized; and so far from increasing the inductance of the discharge circuit it positively diminishes it by the reaction effect of these induced currents: it acts, in fact, much as a mass of copper might be expected to do.

The conditions determining rate of oscillation being understood, we have next to consider what regulates the damping out of the vibrations, *i.e.* the total duration of the discharge.

Resistance is one thing. To check the oscillations of a vibrating spring you apply to it friction, or make it move in a viscous medium, and its vibrations are speedily damped out. The friction may be made so great that oscillations are entirely prevented, the motion being a mere dead-beat return to the position of equilibrium; or, again, it may be greater still, and the motion may correspond to a mere leak or slow sliding back, taking hours or days for its accomplishment. With very large condensers, such as are used in telegraphy, this kind of discharge is frequent, but in the case of a Leyden jar discharge it is entirely exceptional. It can be caused by including in the circuit a wet string, or a capillary tube full of distilled water, or a slab of wood, or other atrociously bad conductor of that sort; but the conditions ordinarily associated with the discharge of a Leyden jar, whether it discharge through a long or a short wire, or simply through its tongs, or whether it overflow its edge or puncture its glass, are such as correspond to oscillations, and not to leak. [Discharge jar first through wire and next through wood.]

When the jar is made to leak through wood or water the discharge is found to be still not steady: it is not oscillatory indeed, but it is intermittent. It occurs in a series of little jerks, as when a thing is made to slide over a resined surface. The reason of this is that the terminals discharge faster than the circuit can supply the electricity, and so the flow is continually stopped and begun again.

Such a discharge as this, consisting really of a succession of small sparks, may readily appeal to the eye as a single flash, but it lacks the noise and violence of the ordinary discharge; and any kind of moving mirror will easily analyze it into its constituents and show it to be intermittent. [Shake a mirror, or waggle head or opera-glass.]

It is pretty safe to say, then, that whenever a jar discharge is not oscillatory it is intermittent, and when not intermittent is oscillatory. There is an intermediate case when it is really dead-beat, but it could only be hit upon with special care, while its occurrence by accident must be rare.

So far I have only mentioned resistance or friction as the cause of the dying out of the vibrations; but there is another cause, and that a most exciting one.

The vibrations of a reed are damped partly indeed by friction and imperfect elasticity, but partly also by the energy transferred to the surrounding medium and consumed in the production of sound. It is the formation and propagation of sound-waves which largely damp out the vibrations of any musical instrument. So it is also in electricity. The oscillatory discharge of a Leyden jar disturbs the medium surrounding it, carves it into waves which travel away from it into space: travel with a velocity of 185,000 miles a second: travel precisely with the velocity of light. [Tuning-fork.]

The second cause, then, which damps out the oscillations in a discharge circuit is *radiation: electrical* radiation if you like so to distinguish it, but it differs in no respect from ordinary radiation (or "radiant heat" as it has so often been called in this place); it differs in no respect from Light except in the physiological fact that the retinal mechanism, whatever it may be, responds only to waves of a particular, and that a very small, size, while radiation in general may have waves which range from 10,000 miles to a millionth of an inch in length.

The seeds of this great discovery of the nature of light were sown in this place: it is all the outcome of Faraday's magneto-electric and electrostatic induction: the development of them into a rich and full-blown theory was the greatest part of the life-work of Clerk-Maxwell: the harvest of experimental verification is now being reaped by a German. But by no ordinary German. Dr. Hertz, now Professor in the Polytechnicum of Karlsruhe, is a young investigator of the highest type. Trained in the school of Helmholtz, and endowed with both mathematical knowledge and great experimental skill, he has immortalized himself by a brilliant series of investigations which have cut right into the ripe corn of scientific opinion in these islands, and by the same strokes as have harvested the grain have opened up wide and many branching avenues to other investigators.

At one time I had thought of addressing you this evening on the subject of these researches of Hertz, but the experiments are not yet reproducible on a scale suited to a large audience, and I have been so closely occupied with some not wholly dissimilar, but independently conducted, researches of my own—researches led up to through the unlikely avenue of lightning-conductors—that I have had as yet no time to do more than verify some of them for my own edification.

In this work of repetition and verification Prof. Fitzgerald has, as related in a recent number of NATURE (February 21, p. 391), probably gone further; and if I may venture a suggestion to your Honorary Secretary, I feel sure that a discourse on Hertz's researches from Prof. Fitzgerald next year would be not only acceptable to you, but would be highly conducive to the progress of science.

I have wandered a little from my Leyden jar, and I must return to it and its oscillations. Let me very briefly run over the history of our knowledge of the oscillatory character of a Leyden jar discharge. It was first clearly realized and distinctly stated by that excellent experimentalist, Joseph Henry, of Washington, a man not wholly unlike Faraday in his mode of work, though doubtless possessing to a less degress that astonishing insight into intricate and obscure phenomena; wanting also in Faraday's circumstantial advantages.

This great man arrived at a conviction that the Leyden jar discharge was oscillatory by studying the singular phenomena attending the magnetization of steel needles by a Leyden jar discharge, first observed in 1824 by Savary. Fine needles, when taken out of the magnetizing helices, were found to be not always magnetized in the right direction, and the subject is referred to in German books as anomalous magnetization. It is not the magnetization which is anomalous, but the currents which have no simple direction; and we find in a memoir published by Henry in 1842, the following words:—

"This anomaly, which has remained so long unexplained, and which, at first sight, appears at variance with all our theoretical ideas of the connection of electricity and magnetism, was, after considerable study, satisfactorily referred by the author to an action of the discharge of the Leyden jar which had never before been recognized. The discharge, whatever may be its nature, is not correctly represented (employing for simplicity the theory of Franklin) by the single transfer of an imponderable fluid

[1] Lord Rayleigh.

from one side of the jar to the other; the phenomenon requires us to admit *the existence of a principal discharge in one direction and then several reflex actions backward and forward, each more feeble than the preceding, until the equilibrium is obtained.* All the facts are shown to be in accordance with this hypothesis, and a ready explanation is afforded by it of a number of phenomena, which are to be found in the older works on electricity, but which have until this time remained unexplained."[1]

The italics are Henry's. Now if this were an isolated passage it might be nothing more than a lucky guess. But it is not. The conclusion is one at which he arrives after a laborious repetition and serious study of the facts, and he keeps the idea constantly before him when once grasped, and uses it in all the rest of his researches on the subject. The facts studied by Henry do in my opinion support his conclusion, and if I am right in this it follows that he is the original discoverer of the oscillatory character of a spark, although he does not attempt to state its theory. That was first done, and completely done, in 1853, by Sir William Thomson; and the progress of experiment by Feddersen, Helmholtz, Schiller, and others has done nothing but substantiate it.

The writings of Henry have been only quite recently collected and published by the Smithsonian Institution of Washington in accessible form, and accordingly they have been far too much ignored. The two volumes contain a wealth of beautiful experiments clearly recorded, and well repay perusal.

The discovery of the oscillatory character of a Leyden jar discharge may seem a small matter, but it is not. One has only to recall the fact that the oscillators of Hertz are essentially Leyden jars—one has only to use the phrase "electro-magnetic theory of light"—to have some of the momentous issues of this discovery flash before one.

One more extract I must make from that same memoir by Henry,[2] and it is a most interesting one; it shows how near he was, or might have been, to obtaining some of the results of Hertz; though, if he had obtained them, neither he nor any other experimentalist could possibly have divined their real significance.

It is, after all, the genius of Maxwell and of a few other great theoretical physicists whose names are on everyone's lips[3] which endows the simple induction experiments of Hertz and others with such stupendous importance.

Here is the quotation:—

"In extending the researches relative to this part of the investigations, a remarkable result was obtained in regard to the distance at which induction effects are produced by a very small quantity of electricity; a single spark from the prime conductor of a machine, of about an inch long, thrown on to the end of a circuit of wire in an upper room, produced an induction sufficiently powerful to magnetize needles in a parallel circuit of iron placed in the cellar beneath, at a perpendicular distance of 30 feet, with two floors and ceilings, each 14 inches thick, intervening. The author is disposed to adopt the hypothesis of an electrical *plenum* [in other words, of an ether], and from the foregoing experiment it would appear that a single spark is sufficient to disturb perceptibly the electricity of space throughout at least a cube of 400,000 feet of capacity; and when it is considered that the magnetism of the needle is the result of the difference of two actions, it may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light."

Comparable it is, indeed, for we now know it to be the self-same process.

One immediate consequence and easy proof of the oscillatory character of a Leyden jar discharge is the occurrence of phenomena of sympathetic resonance.

Everyone knows that one tuning-fork can excite another at a reasonable distance if both are tuned to the same note. Everyone knows, also, that a fork can throw a stretched string attached to it into sympathetic vibration if the two are tuned to unison or to some simple harmonic. Both these facts have their electrical analogue. I have not time to go fully into the matter to-night, but I may just mention the two cases which I have myself specially noticed.

A Leyden jar discharge can so excite a similarly-timed neighbouring Leyden jar circuit as to cause the latter to burst its dielectric if thin and weak enough. The well-timed impulses accumulate in the neighbouring circuit till they break through a quite perceptible thickness of air.

Put the circuits out of unison by varying the capacity or by including a longer wire in one of them; then, although the added wire be a coil of several turns, well adapted to assist mutual induction as ordinarily understood, the effect will no longer occur.

That is one case, and it is the electrical analogue of one tuning-fork exciting another. It is too small at present to show here satisfactorily, for I only recently observed it, but it is exhibited in the library at the back.

The other case, analogous to the excitation of a stretched string of proper length by a tuning-fork, I published last year under the name of the experiment of the recoil kick, where a Leyden jar circuit sends waves along a wire connected by one end with it, which waves splash off at the far end with an electric brush or long spark.

I will show merely one phase of it to-night, and that is the reaction of the impulse accumulated in the wire upon the jar itself, causing it to either overflow or burst. [Sparks of gallon or pint jar made to overflow by wire round room.[1]]

The early observations by Franklin on the bursting of Leyden jars, and the extraordinary complexity or multiplicity of the fracture that often results, are most interesting.

His electric experiments as well as Henry's well repay perusal, though of course they belong to the infancy of the subject.

He notes the striking fact that the bursting of a jar is an extra occurrence, it does not replace the ordinary discharge in the proper place, it accompanies it; and we now know that it is precipitated by it, that the spark occurring properly between the knobs sets up such violent surgings that the jar is far more violently strained than by the static charge or mere difference of potentials between its coatings; and if the surgings are at all even roughly properly timed, the jar is bound to either overflow or burst.

Hence a jar should always be made without a lid, and with a lip protruding a carefully considered distance above its coatings: not so far as to fail to act as a safety valve, but far enough to prevent overflow under ordinary and easy circumstances.

And now we come to what is after all the main subject of my discourse this evening, viz. the optical and audible demonstration of the oscillations occurring in the Leyden jar spark. Such a demonstration has, so far as I know, never before been attempted, but if nothing goes wrong we shall easily accomplish it.

And first I will do it audibly. To this end the oscillations must be brought down from their extraordinary frequency of a million or hundred thousand a second to a rate within the limits of human audition. One does it exactly as in the case of the spring—one first increases the flexibility and then one loads it. [Spark from battery of jars and varying sound of same.]

Using the largest battery of jars at our disposal, I take the spark between these two knobs—not a long spark, ¼ inch will be quite sufficient. Notwithstanding the great capacity, the rate of vibration is still far above the limit of audibility, and nothing but the customary crack is heard. I next add inertia to the circuit by including a great coil of wire, and at once the spark changes character, becoming a very shrill but an unmistakable whistle, of a quality approximating to the cry of a bat. Add another coil, and down comes the pace once more, to something like 5000 per second, or about the highest note of a piano. Again and again I load the circuit with magnetizability, and at last the spark has only 500 vibrations a second, giving the octave, or perhaps the double octave, above the middle C.

[1] "Scientific Writings of Joseph Henry," vol. i. p. 201. Published by the Smithsonian Institution, Washington, 1886.

[2] *Loc. cit.*, p. 204.

[3] And of one whose name is not yet on everybody's lips, but whose profound researches into electro-magnetic waves have penetrated further than anybody yet understands into the depths of the subject, and whose papers have very likely contributed largely to the theoretical inspiration of Hertz—I mean that powerful mathematical physicist, Mr. Oliver Heaviside.

[1] During the course of this experiment, the gilt paper on the wall was observed by the audience to be sparkling, every gilt patch over a certain area discharging into the next, after the manner of a spangled jar. It was probably due to some kind of sympathetic resonance. Electricity splashes about in conductors in a surprising way everywhere in the neighbourhood of a discharge. For instance, a telescope in the hand of one of the audience was reported afterwards to be giving off little sparks at every discharge of the jar. Everything which happens to have a period of electric oscillation corresponding to some harmonic of the main oscillation of a discharge is liable to behave in this way. When light falls on an opaque surface it is quenched. What the audience saw was probably the result of waves of electrical radiation being quenched by the walls of the room, and generating electrical currents in the act. It is these electric surgings which render such severe caution necessary in the erection of lightning-conductors. This explanation is merely tentative. I have had no time to investigate the matter locally.

One sees clearly why one gets a musical note: the noise of the spark is due to a sudden heating of the air; now if the heat is oscillatory, the sound will be oscillatory too, but both will be an octave above the electric oscillation, if I may so express it, because two heat-pulses will accompany every complete electric vibration, the heat production being independent of direction of current.

Having thus got the frequency of oscillation down to so manageable a value, the optical analysis of it presents no difficulty: a simple looking-glass waggled in the hand will suffice to spread out the spark into a serrated band, just as can be done with a singing or a sensitive flame, a band too of very much the same appearance.

Using an ordinary four-square rotating mirror driven electromagnetically at the rate of some two or three revolutions per second, the band is at the lowest pitch seen to be quite coarsely serrated; and fine serrations can be seen with four revolutions per second in even the shrill whistling sparks.

The only difficulty in seeing these effects is to catch them at the right moment. They are only visible for a minute fraction of a revolution, though the band may appear drawn out to some length. The further away a spark is from the mirror, the more drawn out it is, but also the less chance there is of catching it.

With a single observer it is easy to arrange a contact maker on the axle of the mirror which shall bring on the discharge at the right place in the revolution, and the observer may then conveniently watch for the image in a telescope or opera-glass, though at the lower pitches nothing of the kind is necessary.

But to show it to a large audience various plans can be adopted. One is to arrange for several sparks instead of one; another is to multiply images of a single spark by suitably adjusted reflectors, which if they are concave will give magnified images; another is to use several rotating mirrors: and indeed I do use two, one adjusted so as to suit the spectators in the gallery.

But the best plan that has struck me is to combine an intermittent and an oscillatory discharge. Have the circuit in two branches, one of high resistance so as to give intermittences, the other of ordinary resistance so as to be oscillatory, and let the mirror analyze every constituent of the intermittent discharge into a serrated band. There will thus be not one spark, but a multitude of successive sparks, close enough together to sound almost like one, separate enough in the rotating mirror to be visible on all sides at once.

But to achieve it one must have great exciting power. In spite of the power of this magnificent Wimshurst machine, it takes some time to charge up our great Leyden battery, and it is tedious waiting for each spark. A Wimshurst does admirably for a single observer, but for a multitude one wants an instrument which shall charge the battery not once only but many times over, with overflows between, and all in the twinkling of an eye.

To get this I must abandon my friend Mr. Wimshurst, and return to Michael Faraday. In front of the table is a great induction coil; its secondary has the resistance needed to give an intermittent discharge. The quantity it supplies at a single spark will fill our jars to overflowing several times over. The discharge circuit and all its circumstances shall remain unchanged. [Excite jars by coil.]

Running over the gamut with this coil now used as our exciter instead of the Wimshurst machine—everything else remaining exactly as it was—you hear the sparks give the same notes as before, but with a slight rattle in addition, indicating intermittence as well as alternation. Rotate the mirror, and everyone should see one or other of the serrated bands of light at nearly every break of the primary current of the coil. [Rotating mirror to analyze sparks.]

The musical sparks which I have now shown you were obtained by me during a special digression[1] which I made while examining the effect of discharging a Leyden jar round heavy glass or bisulphide of carbon. The rotation of the plane of polarization of light by a steady current, or by a magnetic field of any kind properly disposed with respect to the rays of light, is a very familiar one in this place. Perhaps it is known also that it can be done by a Leyden jar current. But I do not think it is; and the fact seems to me very interesting. It is not exactly new—in fact, as things go now it may be almost called old, for it was investigated six or seven years ago by two most highly skilled French experimenters, Messrs. Bichat and Blondlot.

But it is exceedingly interesting as showing how short a time, how absolutely no time, is needed by heavy glass to throw itself into the suitable rotatory condition. Some observers have thought they had proved that heavy glass requires time to develop the effect, by spinning it between the poles of a magnet and seeing the effect decrease; but their conclusions cannot be right, for the polarized light follows every oscillation in a discharge, the plane of polarization being waved to and fro as often as 70,000 times a second in my own observation.

Very few persons in the world have seen the effect. In fact, I doubt if anyone had seen it a month ago except Messrs. Bichat and Blondlot. But I hope to make it visible to most persons here, though I hardly hope to make it visible to all.

Returning to the Wimshurst machine as exciter, I pass a discharge round the spiral of wire inclosing this long tube of CS_2, and the analyzing Nicol being turned to darkness, there may be seen a faint—by those close to not so faint, but a very momentary—restoration of light on the screen at every spark. [CS_2 tube experiment on screen.]

Now I say that this light restoration is also oscillatory. One way of proving this fact is to insert a biquartz between the Nicols. With a steady current it constitutes a sensitive detector of rotation, its sensitive tint turning green on one side and red on the other. But with this oscillatory current a biquartz does absolutely nothing. [Biquartz.]

That is one proof. Another is that rotating the analyzer either way weakens the extra brightening of the field, and weakens it equally either way.

But the most convincing proof is to reflect the light coming through the tube upon our rotating mirror, and to look now not at the spark, or not only at the spark, but at the faint band into which the last residue of light coming through polarizer and tube and analyzer is drawn out. [Analyze the light in rotating mirror.]

At every discharge this faint streak brightens in places into a beaded band: these are the oscillations of the polarized light: and when examined side by side they are as absolutely synchronous with the oscillations of the spark itself as can be perceived.

Out of a multitude of phenomena connected with the Leyden jar discharge I have selected a few only to present to you here this evening. Many more might have been shown, and great numbers more are not at present adapted for presentation to an audience, being only visible with difficulty and close to.

An old and trite subject is seen to have in the light of theory an unexpected charm and brilliancy. So it is with a great number of other old familiar facts at the present time.

The present is an epoch of astounding activity in physical science. Progress is a thing of months and weeks, almost of days. The long line of isolated ripples of past discovery seem blending into a mighty wave, on the crest of which one begins to discern some oncoming magnificent generalization. The suspense is becoming feverish, at times almost painful. One feels like a boy who has been long strumming on the silent keyboard of a deserted organ, into the chest of which an unseen power begins to blow a vivifying breath. Astonished, he now finds that the touch of a finger elicits a responsive note, and he hesitates, half delighted, half affrighted, lest he be deafened by the chords which it would seem he can now summon forth almost at will.

[1] Most likely it was a conversation which I had with Sir Wm. Thomson, at Christmas, which caused me to see the interest of getting slow oscillations. My attention has mainly been directed to getting them quick.

ON THE LIMIT OF THE SOLAR SPECTRUM, THE BLUE OF THE SKY, AND THE FLUORESCENCE OF OZONE.

THERE are two facts of particular interest which have been observed in connection with the light which we receive from the sun and the sky. First, though the ultra-violet spectrum of the sun is very well represented by the iron spectrum taken from the electric arc, yet its length is nothing like so great, and there is no fading away of feeble lines and a weakening of strong ones, which would be the case if the rays were affected by a turbid medium through which they were transmitted, but there is a sudden and sharp extinction which points to a very definite absorption. Secondly, light from the clearest sky, unaffected by aqueous vapour, is of a deep and beautiful blue colour, more of an indigo-violet tint than ordinary so-called sky-blue. There is nothing more beautiful in Nature than the blueness of the heavens.

The limitation of the solar spectrum has been the subject of elaborate investigation by M. Cornu.[1] He proved by direct experiment that the ultra-violet rays are absorbed with energy by the atmosphere, and showed that there is a variation in the amount of absorption corresponding with different altitudes, so that the absorbent matter is at each elevation proportional to the barometric pressure, and consequently in constant relation to the mass of the atmosphere. This fact alone is sufficient to exclude water-vapour from consideration as being the medium of absorption. Moreover, water-vapour, while it absorbs the red and infra-red rays, transmits the ultra-violet very completely.

Photographs taken in 1879 on the Riffelberg, at an altitude therefore of 8414 feet, reached to wave-length 2932; but at Viège, an altitude of 2165 feet, to only 2954.

In short, it was shown that within the limits of altitude at which Cornu's observations were made there was a difference of 10 tenth-metres of wave-length for every 984 feet of dry atmosphere, the shortening of the spectrum being due to the gaseous constituents. Notwithstanding this, it was stated by Prof. Liveing, in a lecture delivered at the Royal Institution on March 9, 1883, that we must suppose the absorbent substance, whatever it may be, is not in our atmosphere, because, when observations are made upon the summit of an elevation like the Riffelberg, the lengthening of the spectrum reaches only a very trifle beyond U. "The same reason will lead us to reject the notion that the absorption can be due to matter in interplanetary space, for it is not easy to suppose that the gases which pervade that space in extreme tenuity can differ much from those in our atmosphere, because the earth in its annual course must pick up whatever they are, and they must then diffuse into our atmosphere. The absorption is, therefore, probably neither in our atmosphere nor in interplanetary space, and we must look for it in the solar atmosphere."

It is then suggested that the blotting out of the sun's light beyond U is caused by something in the solar atmosphere higher up than the metallic vapours which give rise to Fraunhofer's lines, but at a lower temperature. It has been shown by Young that the Fraunhofer lines are bright lines, but appear black against the brilliant light of the photosphere. It never appeared to me that Prof. Liveing's objection to Cornu's explanation of the limitation of the solar spectrum by our atmosphere was valid, because it was proved beyond question that the atmosphere absorbs the ultra-violet rays, and also that on the same day and hour, at different elevations, the extent of rays absorbed was proportional to the barometric pressure—that is, to the quantity of air through which the rays passed. A considerable acquaintance with absorption-spectra in the ultra-violet region has proved to me that when an absorption-band has been blotted out by increasing the proportion of substance, or by increase of the thickness of the absorbent layer, a stage is soon reached at which any further increase only causes a trifling difference in absorbent action, and in fact that many substances attain a maximum of absorption beyond which there is no change unless we increase the density of the substance, and so probably alter its molecular structure. Under the same conditions of pressure, increased thickness of the absorbent layer only very slightly increases the absorbent action, and that in a degree which is by no means proportional to the layer of material.

There is a difficulty in accepting Prof. Liveing's views, because we know nothing, as he remarks, in the solar atmosphere capable of causing such absorption, and at the same time of transmitting the Fraunhofer lines of the less refrangible portions of the spectrum in the condition in which we observe them.

The matter was very fully considered by me two years previously—that is to say, in the year 1881. The absorption spectra of various gases were examined by photographing the ultra-violet rays which were transmitted by carefully measured quantities of gas at the atmospheric pressure, and one of these gases was ozone. It proved to be a substance with most extraordinary absorptive powers, so that even when very much diluted it exhibited an absorption-band of great intensity which was carefully investigated. By examining the spectrum transmitted by increasing quantities of ozone the band disappeared, and there was a complete and total absorption of rays extending to about wave-length 3160. Any further increase did not cause a corresponding shortening of the spectrum. The band was observed between wave-lengths about 2850 and 2320; but with increased proportions of ozone the rays transmitted were restricted to about 2920, and became more restricted in presence of greater quantities of gas.[1] Thus:—

Extreme ray transmitted by ozonized oxygen. λ.	Length of column of gas traversed by the rays. Centimetres.	Actual volume of ozone per square centimetre of sectional area of column. Cubic centimetres.
3035 ...	92 ...	1·012
3150 ...	196·5 ...	2·162
3160 ...	288·5 ...	3·175

It was found that a quantity of ozone proportional to the average quantity present in a vertical column of the atmosphere caused an absorption similar to that observed in the solar spectrum—that is to say, terminating about 2950. Largely increased quantities did not largely, but only in a trifling degree, increase the absorption. Furthermore, it was shown that the atmosphere contained ozone as a normal constituent, and that it was present in greater proportion in the upper regions than near the earth's surface. It was proved that all the other minute constituents of the atmosphere were either non-absorbent or exerted absorption in a manner different from that of ozone, and that the quantity of ozone commonly present in the atmosphere is quite sufficient to account for the limitation of the solar spectrum, without taking into account the possible absorption caused by the great thickness of oxygen and of nitrogen. The possibility of oxygen being the absorptive substance seemed very great, considering the small difference in constitution between the molecules of ozone and oxygen. It must be understood that the conclusions were arrived at by reasoning from strictly quantitative experiments, and seemed almost incontrovertible, and it may be stated that none of the facts alleged have ever been questioned. It was impossible to deal with the matter further without costly appliances for the compression of oxygen and nitrogen into tubes capable of holding a quantity comparable with the amount of oxygen and of nitrogen in a vertical column of the atmosphere, and for this reason the investigation fell into abeyance.

It was also considered that the problem might be attacked in another manner.

Messrs. Liveing and Dewar have recently made a very interesting and important communication to the *Chemical News* (vol. lviii. p. 163), on the absorption-spectrum of oxygen. In a tube 1·6 metre in length, filled with the gas at a pressure of 160 atmospheres, all rays were absorbed beyond wave-length 2665, but they began to diminish at 2705. With a tube 6 metres or 20 feet long, and with a pressure of 90 atmospheres, it seems that an absorption-band is to be traced at wave-length 3640 to 3600, and there is a complete absorption beyond 3360.

The gas seen in quantity corresponding to that in a vertical column of the atmosphere appears to have a faint blue tint. There can be no doubt whatever that the oxygen of the air exerts a powerful absorption on the rays of the sun, but it does not appear from these experiments that this absorption is exactly the cause of the limitation of the spectrum, as described by Cornu, since when observed in tubes it is carried into a region of longer wave-length than is observed at the level of the sea; thus at Dublin the limit in summer is usually about 3130.[2] It is no doubt the high density of the gas which causes the absorption to be stronger than that of the atmosphere. It should be noted that the oxygen in the 20-foot tube was the quantity in a vertical column of the atmosphere. It is probable that there are several substances in interplanetary space, or in the solar atmosphere, which, besides oxygen and ozone in the air, cause an absorption of the sun's rays and a limitation of the length of the spectrum, but as Messrs. Liveing and Dewar point out, our atmosphere places a limit to the observations we can make on the rays of other heavenly bodies.

Touching the colour of the sky, Prof. Tyndall has told us that four centuries ago it was believed that the floating particles in the atmosphere render it a turbid medium through which we

[1] "Sur l'Absorption Atmosphérique des Radiations ultra-violettes," *Journ. de Physique*, t. x. 1881.

[1] "On the Absorption-Spectrum of Ozone, and on the Absorption of Solar Rays by Atmospheric Ozone," Journ. Chem. Soc. 1881, xxxix., pp. 57, 111, 119.

[2] In the *Phil. Mag.*, September 1888, p. 288, Messrs. Liveing and Dewar refer only to my first paper on the absorption-spectrum of ozone, Journ. Chem. Soc., xxxix, p. 57, but not to the more complete paper on this spectrum at pp. 111–119, *loc. cit.*, which indicates the possible limits of the ozone in the atmosphere.

look at the darkness of space. The blue colour, according to his view, is supposed to be caused by reflection from minute particles, which can reflect chiefly the blue rays by reason of their small size. Experiments on highly attenuated vapours during condensation to cloudy matter were the basis of this reasoning. It always seemed to me that if a view be seen through a turbid medium which reflects chiefly the blue rays, it would not appear blue, but the complementary colour, yellow; and therefore this theory could not account for the blue of distance. In fact, when a light mist hangs over the surface of the earth, and the rays of the sun are transmitted in a direction approaching the horizontal, the result is that the sun and all objects lying in the direction looking towards it appear yellow, while the mist in the opposite direction appears blue, and only translucent, not transparent. The blue of the sky, if caused by such a similar action of floating particles, would not be seen when the sun was overhead, nor could it be seen by looking in the direction of the sun.

The blue would not be transparent and in character similar to the blue of a clear distance, in which the outlines of mountains and rocks are perfectly distinct and sharp, the shadows being of an intensely deep blue, and the most distant objects the deepest in colour. In 1880, Messrs. Hautefeuille and Chappuis liquefied ozone, and found that its colour was indigo blue (*Comptes rendus*, xcv. p. 522). On December 12, 1880, M. Chappuis presented the Academy of Sciences of Paris with a paper on the visible spectrum of ozone. He recognized the most easily visible of the absorption-bands of ozone in the solar spectrum, and in consequence he stated that a theory of the blue colour of the sky could not be established without taking into account the presence of ozone in the atmosphere, for the luminous rays which reach us will of necessity be coloured blue by their transmission through the ozone contained in the atmosphere. And since ozone is an important constituent of the upper atmosphere, its blue colour certainly plays an important part in the colour of the sky. In March 1881, quantitative experiments made by me were published to show how much of blueness could be communicated to layers of gas of different thicknesses when given volumes of ozone are present. I showed that ozone is a normal constituent in the upper atmosphere, that it is commonly present in fresh air, and I accounted for its abundance during the prevalence of westerly and south-westerly winds. It was likewise shown that it was impossible to pass rays of light through as much as 5 miles of air without the rays being coloured sky-blue by the ozone commonly present, and that the blue of objects viewed on a clear day at greater distances up to 35 or 50 miles must be almost entirely the blueness of ozone in the air. The quantity of ozone giving a full sky-blue tint in a tube only 2 feet in length is 2½ milligrammes in each square centimetre of sectional area of the tube. It is necessary to mention that a theory of the blue of the sky was propounded by M. Lallemand ("Sur la Polarisation et la Fluorescence de l'Atmosphère," *Comptes rendus*, lxxv. p. 707, 1872) after his observations had been found inconsistent with all previous explanations. If the coloration be due to reflection from minute particles of floating matter, or if it be due to white light being transmitted through a blue gas, the blue portion of the sky should be polarized quite as much as white light coming from the same direction in the heavens. But the experiments of M. Lallemand prove that this is not so. Upon these experiments he bases his theory that the blue colour of the atmosphere is due to a blue fluorescence like that seen in acid solutions of sulphate of quinine—that is to say, caused by a change of refrangibility in the ultra-violet rays.

Ångström first threw out the idea of fluorescence being a property of certain gases in the atmosphere. To possess this property the gas must be capable of absorbing either in part or entirely the ultra-violet and violet rays, and of emitting them with a lowered refrangibility and without being polarized. Ozone possesses the property of absorption in the highest degree in the ultra-violet region, and I have now to announce that strongly ozonized oxygen is highly fluorescent when seen in a glass bottle two inches in diameter illuminated by an electric spark passing between cadmium electrodes. The colour of the fluorescence is a beautiful steel blue. This fluorescence has not been observed in other gases, but it is in the highest degree probable that oxygen is fluorescent, though this has yet to be proved. There can be, however, little doubt that the colour of the sky is caused in part by the fluorescence of ozone, and also to some extent by the transmission of rays through the blue gas. The blue of distance is doubtless to be attributed more to transmission than the blue of the sky, though it is quite conceivable that fluorescence also here comes into play. Whatever other cause concurs in the production of the blue of the heavens, it has certainly been established by M. Chappuis that the properties of ozone participate in its production.

In August 1884, a very short note was sent by me to NATURE concerning the red solar halo seen at Zermatt and on the Riffelberg with great distinctness. I recorded the occurrence of a dark band in the spectrum, slightly more refrangible than D, which was seen to vary in intensity; a second band a little less refrangible than D was also observed. On account of the altitude at which the observations were made, viz. 9000 feet, and the state of the weather at the time, these bands were considered to be due to some constituent of dry air.

The subject of the telluric rays has become of increased interest since M. Cornu has studied the dark lines in the neighbourhood of D, but unfortunately the rays absorbed to which I refer are both a little more and a little less refrangible than those figured on his map of this region. If we accept the number 5890 tenth-metrets as approximately representing the mean value of the lines D^1 D^2, the narrow bands observed by me have wave-lengths about (1) 5950 and (2) 5770 at their darkest parts, as far as one can ascertain with a hand spectroscope giving excellent definition but small dispersion. They are very variable, being dependent on the state of the weather, and are more distinct and broader when viewed with the sun on the horizon. In London during the dry calm weather of June and July 1884, they were very strong, but variable in different parts of the sky.

The less refrangible band, or broad line as it usually appears, below D, is generally over-lapped by a band belonging to water vapour, the chief "rain-band." On this account observations at an elevation of 10,000 feet or so during perfectly dry weather were considered of interest. The bands were observed against the blue sky on several occasions, but they were also at other times entirely absent or barely visible. There is some liability to a group of iron, barium, and other solar lines being mistaken for the more refrangible band when it is not decidedly strong. Chappuis observed bands in the blue sky coincident with ozone bands, and I have on that account always expected to find some indication of the spectrum of ozone in the upper atmosphere, but the reason why there must always be a difficulty in obtaining evidence of any absorption due to this substance arises from the strongest visible band of ozone, with wave-length 6095 to 5935, being masked by the band of water vapour; and secondly, because the total amount of white light so preponderates as to overpower the effect of absorption—that is to say, the rays absorbed are only a small fraction of those transmitted, so that the bands are faint and the colour due to absorption is either not seen or seen only with difficulty. Owing to this fact we cannot distinguish the blue colour of the clouds when the sunlight is bright; but when the sky is completely over-clouded with cumuli a faintly bluish tint is given to the cloud-shadows, even at the zenith. Near the horizon not only are the bright parts of the clouds blue, but their shadows have a rich blue tint.[1] The blueness varies somewhat: at times it may be seen to shift about in the sky; it has been observed, for instance, to pass over from south-west to north-east. The second but less conspicuous band of ozone absorbs rays with wave-length 5770 to 5600. Both bands have been observed in a dry atmosphere at elevations varying from 6000 to 10,000 feet, both in the blue of the sky and against white clouds. The measurements, very imperfectly made under difficulties, showed them to have wave-lengths about (1) 5950, (2) 5770, in the centre of the dark portion, while, according to Chappuis, the bands of ozone are—

(1) 6095 to 5935	...	mean, 6010
(2) 5770 to 5600	...	mean, 5680.

On Ångström's chart, a dark band, diminishing in depth towards the east, extends from 5785 to 5680, which is classed among the *raies atmosphériques*; this is similar to the band observed by me when viewing the sun or bright clouds near the horizon, and is similar to the second ozone band.

The work of Prof. Piazzi-Smyth, "Madeira Spectroscopic," does not give that portion of the spectrum which would serve for comparison. The "low-sun band," δ, comes very near to band (2), wave-length 5770, while the "rain-band" comes very near (1), 6095. On several subsequent occasions the two bands were

[1] Prof. Pickering has proved that sunlight as it reaches us is blue, which must be the case if it has passed through a blue medium (Proc. American Academy of Sciences, 1880, p. 236).

observed at lower levels than Zermatt and the Riffel, but with even less intensity. On the last occasion (November 10, 1884) that such an observation was recorded, there was only a trace of these faintly seen. No doubt a clear atmosphere, free from the turbidity so easily created by condensing moisture, is essential to their visibility. Schœne has observed bands in a bright sky before sunrise and after sunset, during an intense frost in Central Russia. The measurements taken identified them with ozone bands, and leave scarcely any doubt whatever of the presence of ozone in the atmosphere, and if it can be so recognized, it must communicate its characteristic blue colour to the air (Journ. Chem. Soc. Abstracts, vol. xlviii. part 2, p. 713). The remarkable crepuscular phenomena seen at the close of 1883 proved highly favourable to such investigations.

In order to continue a series of observations on the solar spectrum near D it would be best to employ a fairly good dispersion and large lenses with long focus admitting a large amount of light to the eye, or, better still, to specially prepared photographic plates highly sensitive to the yellow rays.

The very extensive absorption of the ultra-violet rays by oxygen leads us to expect it to be fluorescent. All such absorbents are fluorescent more or less, and generally strongly, but when the absorbed rays are of very short wave-length the fluorescence is not always visible. Thus there are many substances which do not appear fluorescent by lime-light nor by dull daylight, but are strongly so when seen by electric light, especially if it has passed through no glass or other medium than a quartz lens and a short column of air. Some substances are not fluorescent when seen in glass vessels, because the glass has absorbed those rays of which the refrangibility would have been lowered by the fluorescent substance. In air, and by the light of an electric spark rich in ultra-violet rays, such as that from cadmium electrodes, almost everything is fluorescent. The whole range of the cadmium spectrum has been viewed by me, owing to the fluorescence of the purest white blotting-paper. The light, of course, is feeble, and the eye has to be trained to make observations in total darkness.

Pure water, however, never appears fluorescent. Some solutions in water, which transmit all the ultra-violet rays as far as 2304, are fluorescent, though whether this is caused by impurities or not has not been decided.

It cannot any longer be doubted (1) that the extreme limit of the solar spectrum observed by Cornu is caused by the gases in the atmosphere, probably both by oxygen and ozone; (2) that the blue of the sky is a phenomenon caused by the fluorescence of the gaseous constituents of the atmosphere, and probably ozone and oxygen are the chief fluorescent substances; (3) that ozone is generally present in the air in sufficient quantity to render its characteristic absorption-spectrum visible, and that therefore it gives a blue colour to the atmosphere by absorption, through which blue medium we observe distant views; (4) that water vapour does not participate in the coloration of the atmosphere under like conditions and in the same manner as ozone. W. N. HARTLEY.

Royal College of Science, Dublin.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Vice-Chancellor of Cambridge University publishes the following:—

"*Ferndene, Gateshead, March* 4, 1889.

"SIR,—You may be aware that some years ago I erected here a refracting telescope, 25-inch aperture and 30 feet focal length.

"Owing to unfavourable atmospheric conditions and other reasons, the instrument has done no systematic work in its present position. I should much like to place it where it would work under capable direction.

"I contemplate offering my telescope and the dome and instruments connected with it to the University of Cambridge. The part that the University has taken of late years in the advancement of science induces me to hope that the possession of an instrument especially adapted to the study of stellar physics may give impetus to the development of the subject.

"I beg you therefore to give the matter consideration, and let me know what proposals can be made to insure proper use and maintenance of the instruments, and publication of yearly reports, in case my offer is acceptable to the University.

"I have the honour to be, Sir,

"Your obedient Servant,

"R. S. NEWALL.

"The Vice-Chancellor of the University of Cambridge."

At the Congregation to-day, at 2 p.m., the following Grace, having received the sanction of the Council, is to be offered to the Senate:—

"That the Vice-Chancellor, Dr. Routh, Dr. Glaisher, Prof. Adams, Prof. Liveing, Prof. Darwin, and Prof. Thomson, be appointed a Syndicate to consider Mr. R. S. Newall's munificent proposal to present his telescope to the University, and the arrangements and expenditure which would be necessary to maintain and utilize it for astronomical and physical research, and to report to the Senate."

SCIENTIFIC SERIALS.

IN the *Journal of Botany* for February and March, Mr. A. Fryer continues his notes on pond-weeds, the present instalment being characterized by two plates and the description of a new species, *Potamogeton falcatus*, from Huntingdonshire. Another addition to the British flora is recorded by the Rev. E. S. Marshall, in *Festuca heterophylla*, from Witley, in Surrey; and Mr. F. J. Hanbury describes no less than four species or sub-species of the difficult genus, *Hieracium*, new to science—all from the extreme north of Scotland. Messrs. G. Murray and L. A. Boodle commence a monograph of *Avrainvillea*, a genus of siphonocladaceous Algæ, which they consider most nearly allied to *Penicillus* and *Udotea*.

THE *Botanical Gazette* for January contains a short description and history, illustrated by five plates, of the new Botanical Laboratory at the University of Philadelphia. The occurrence of a new phosphorescent Fungus is noted, *Agaricus* (*Clitocybe*) *illudens*, in which the phosphorescence appears to reside in the hymenium. In the number for February, Dr. Henrietta E. Hooker gives a highly interesting description of the structure and mode of life of the common dodder of Massachusetts, *Cuscuta Gronovii*. It is stated to have a habit of entirely withdrawing its roots from the soil by the contraction of the coils of its twining stem as soon as it commences to lead a parasitic life on its host. Miss Emily L. Gregory continues her paper on the "Development of cork-wings on certain trees."

Rivista Scientifico-Industriale, January 31.—On the oxalate of lime in plants, by Prof. Aser Poli. In these remarks, which are made in connection with C. Acqua's recent contribution to the study of the crystals of the oxalate of lime in plants, it is argued that even on Acqua's own showing, the presence of these crystals cannot be regarded as necessary to the life of the plant. In some they are not found at all, and where they do exist they seem to be rather an inevitable consequence of the production of oxalic acid in the presence of the salts of lime.—Prof. Ercole Fossati continues, without concluding, his elaborate monograph on the thermic and electric properties of iron subjected to magnetic influences.

Bulletin de la Société des Naturalistes de Moscou, 1888, No. 3.—On the peculiarities of the skulls of the horned cattle of the Kalmucks, by P. Kuleschoff (in German), with photographs of skulls. The great likeness between the skulls of the Kalmuck race of horned cattle and those of *Bos sondaicus* and the zebra brings the author to the conclusion that the ancestors of the European cattle must be searched for in India.—On the Orthopteres of Crimea, by O. Retowski (with three plates).—On the structure of the pseudo-scorpions, or *Chernetidæ*, by A. Croneberg (both in German). The differences in the structure of this group and that of the scorpions proper are pointed out, and Thorell's views on the affinities of the *Chernetidæ* are confirmed.—Review of the generative organs of the *Pompilidæ*, by General Radoszkowski (in French, with four plates).—Supplementary note on the great comet 1887 I., by Th. Bredichin (in French). It belongs to the author's third class of comets—that is, it consists of heavy elements; and more accurate calculations convince the author that it could consist only of elements having a great molecular weight, such as gold, mercury, and lead.—Some additions to the flora of Moscow, by S. Milutin.

SOCIETIES AND ACADEMIES.

London.

Royal Society, February 21.—"The Innervation of the Pulmonary Vessels." By J. Rose Bradford, M.B., D.Sc., George Henry Lewes Student, and H. Percy Dean, M.B., B.S., B.Sc. Communicated by E. A. Schäfer, F.R.S. (From the Physiological Laboratory of University College, London.)

Hitherto no direct experimental proof of the existence of vasomotor fibres for the vessels of the mammalian lung has been obtained. The method used by us consisted in exciting the roots of the upper dorsal nerves, and recording simultaneously the effects produced on the aortic and pulmonary blood-pressure. The aortic pressure was measured in the usual way, *i.e.* a mercurial manometer was connected with the carotid artery. A second manometer was then connected with the branch of the pulmonary artery distributed to the lower lobe of the left lung. All the observations were made on dogs.

Before considering the results obtained by excitation of the upper dorsal nerves, it is necessary to know what effect a given rise of aortic pressure will have on the pulmonary pressure. This was determined by the three following methods:—

I. Excitation of the peripheral end of a divided splanchnic.

II. Excitation of the lower end of the spinal cord divided in the middle of the dorsal region.

III. Compression of the thoracic aorta.

By all these methods an enormous rise in the systemic blood-pressure is obtained, but the simultaneous pulmonary rise is very small. Thus the aortic pressure may be doubled or even quadrupled. The rise of the pulmonary pressure is always small and sometimes absent. The rise, as a rule, is about one-fifth of the total pulmonary pressure, *i.e.* the rise is then very small compared with the doubling or quadrupling of the systemic pressure. The relative ratio of the pulmonary rise to the carotid rise is that the former is about one-twentieth of the latter. Hence an enormous increase of systemic pressure is required in order to cause even a small rise of pressure in the pulmonary vessels.

The same occurs when the aorta is compressed, no rise of pulmonary pressure occurs unless the aortic rise is not only very marked but also of some duration, *e.g.* 30 seconds.

Excitation of the central end of the sciatic causes but a very slight pulmonary rise. The central end of the vagus gives larger effects, due to reflex contraction of the pulmonary arterioles, and the central end of a posterior root of one of the upper dorsal nerves gives still greater effects.

If the vasomotor centre in the medulla be excited, a great rise of both pulmonary and systemic pressure is produced. The same excitation of the medulla after division of the cord in the mid-dorsal region produces as great a pulmonary rise, but the systemic effect is now quite small.

Excitation of the upper dorsal roots, *i.e.* from the second to the seventh, produces rises of pulmonary pressure as marked as any of the preceding, but accompanied either by a small aortic rise or by no aortic effect. With the third nerve a marked rise of pulmonary pressure accompanied by a fall of aortic pressure is seen. Hence these nerves contain the vaso-constrictor fibres for the pulmonary vessels, since the effects produced on the pulmonary pressure must be due to constriction of the pulmonary arterioles, inasmuch as the simultaneous carotid effects are quite incompetent to cause them.

Finally, although it is undoubted from the results of the research that the mammalian pulmonary vessels receive vasomotor fibres, yet it is probable that this vasomotor mechanism is but poorly developed when compared to that regulating the systemic vessels.

Geological Society, February 20.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The President announced that a special general meeting, for the consideration of the by-laws, would be called for Friday, March 15, at 4.30 p.m.—The following communications were read:—On the Cotteswold, Midford, and Yeovil Sands, and the division between Lias and Oolite, by S. S. Buckman. After giving a short sketch of the work and opinions of other writers, the author proceeded with the evidence on which his own views are based. He described a series of sections of the typical exposures of "sands" and contiguous strata, commencing near Stroud and terminating on the Dorset coast. Dividing the series into *seven* horizons, characterized by their distinctive Ammonites, viz. *Amm. communis, variabilis, striatulus, dispansus*, the genus *Dumortieria, Amm. Moorei*, and *opalinus*, and taking the *striatulus* beds as a fixed starting-point, the author demonstrated how the strata varied in regard to that horizon. The Cotteswold Sands, containing the *variabilis* and part of the *communis* horizons, were below the *striatulus* beds; the Midford Sands, containing the *dispansus* horizon, were above, *Gramm. striatulum* occupying a thin bed at the base; the Yeovil Sands, containing the *Moorei* and *Dumortieria* horizons, overlay a bed containing Ammonites of the *dispansus* horizon, and were consequently still later deposits. Since the different sands were deposited not on a horizontal plane, in point of time, but, as it were, obliquely, the deposit of Cotteswold Sands having ceased before that of Yeovil Sands commenced, it was incorrect to lump all the "sands" from the Cotteswolds to the Dorset coast under the single local name "Midford Sands," thereby implying a contemporaneity which did not exist, while the use of the present restricted local names was defended. The Ammonites were apparently uninfluenced by changes in the character of the deposit, since the same species are found in Limestone in the Cotteswolds, in Sands at Midford, and in argillaceous Marl at Ilminster. The change from argillaceous to arenaceous or calcareous deposits has been looked upon as so distinct a feature, that it has been utilized as a great argument in favour of drawing the line between Lias and Oolite at that point; but if this be done, the line is always drawn at different horizons in different districts. If lithology furnishes no reason for a dividing-line at this point, it was shown that neither did palæontology. It was also shown that the Ammonite family Hildoceratidæ dominated the period from the *falcifer* to the *concavus* zones, and that with the close of the latter zone they died out with singular abruptness, and that, furthermore, there exists, both in England and upon the Continent, a marked hiatus at the same point due to the absence of a zone or a number of zones. On account of these facts the proposal was put forward that d'Orbigny's term "Toarcien" should be employed to designate the strata fron the *falcifer* zone to the *concavus* zone inclusive, that this term should not be used in the sense of merely an extended "Upper Lias," but to mark an entirely distinct transition-formation—a definite part of the Jurassic period—separating the typical Lias from the mass of thoroughly Oolitic strata. The reading of the paper was followed by a discussion, in which the President, Mr. H. B. Woodward, Prof. Blake, Rev. H. H. Winwood, and Mr. Hudleston, took part.—On some nodular felstones of the Lleyn Peninsula, by Miss Catherine A. Raisin. Communicated by Prof. T. G. Bonney. Some remarks on the paper were made by Mr. Cole, Dr. Hicks, and Prof. Bonney.—On the action of pure water, and of water saturated with carbonic acid gas, on the minerals of the mica family, by Alexander Johnstone.

Paris.

Academy of Sciences, March 4.—M. Des Cloizeaux, President, in the chair.—Remarks accompanying the presentation of a work entitled "Introduction à l'étude de la Chimie des anciens et du moyen age," by M. Berthelot. This work forms a sequel to the author's "Origines de l'Alchimie" and "Collection des anciens Alchimistes grecs," thus completing a series of historical researches which fully establish the true character of the old philosophic doctrines, methods, and practices, which were hitherto supposed to be mainly absurd and fanciful, but which must henceforth enter into the scheme of historical evolution of the positive sciences. Here M. Berthelot gives a full description and translation of the Leyden papyrus of Egyptian origin, the oldest extant treatise on chemistry. The signs, notations, and appliances of the ancient alchemists are also described and reproduced by the photogravure process.—On the artificial reproduction of halos and parhelic circles, by M. A. Cornu. The author obtains the halos more effectively than by Brewster's method, by depositing on a sheet of glass a crystalline powder of potash alum, which is obtained from a heated saturated solution shaken while cooling. The phenomenon is sufficiently bright to be projected and rendered visible to an audience. This remark applies also to the parhelic circles, which are obtained by an extremely simple process.—On the chondroid plaques in the tendons of birds, by M. L. Ranvier. By employing a solution of osmic acid, the author shows that the chondroid plaques in the tendons of the feet of finches and other small birds contain cellules filled with a fatty substance. By means of other reagents the presence is also revealed of glycogen and cartilage.—On the great storm of March 11, 12, and 13, 1888, in the United States, by M. H. Faye. Lieut. George Dyer's monograph on this terrific hurricane with its accompanying blizzard describes it as of an exceptional character, to which is inapplicable the law based on the circular theory—that is, the eight-point rule. Instead of the usual barometric pressure in form of a basin, it is stated to have presented the aspect of an immense linear depression, a trough of low barometer moving eastwards. This view is incompatible with the well known hypothesis advocated by M. Faye, who accordingly endeavours here to account for the recorded phenomena without having recourse to the "highly improbable theory of a vast trough of low temperature."—On the complete rectification of the sextant, by M. Gruey. Two methods are proposed, dealing with the rectification of the axis of rotation, R, of the large mirror M, with the rectification of the large and small mirrors M and M′, and with that of the axes U and S.—On the separation of zinc and cobalt, by M. H. Baubigny. In a previous paper (*Comptes rendus*, cviii. p. 236) the author described the process by which he has succeeded in completely separating zinc and nickel. The further experiments here carried out tend to show that, even in the state of sulphates, zinc and cobalt cannot be totally separated by sulphuretted hydrogen in presence of a small excess of free sulphuric acid unless the quantity of cobalt be relatively slight. The separation may also be effected with sufficient completeness if the quantity of zinc be slight.—Observations on saccharification by diastase, by M. L. Lindet. During saccharification the decomposition of the starch into maltose and dextrines is always accompanied by a secondary reaction in which the diastase attacks the dextrines and transforms them to maltose. This reaction is arrested by the presence of a certain quantity of maltose, which, however, may be removed by alcoholic fermentation, as maintained by Payen, although denied by O'Sullivan and others. Here M. Lindet confirms Payen's theory by another process, employing phenylhydrazine as a reagent for precipitating the maltose in the state of insoluble phenylmaltosazone.—On some new neutral and acid ethers of the camphols, by M. A. Haller. These ethers are formed under the same conditions as the succinates already described in the *Comptes rendus* of February 25.—Influence of mineral substances on the structure of plants, by M. Henri Jumelle. The experiments here carried out with lupins cultivated under like conditions, but some with distilled water, some with Knop's mineral solution, show that the presence of mineral substances is followed by a greater development of sap, and diminished formation of supporting elements; further, that the absence of salts considerably modifies the structure of plants, the modifications, however, being largely due less to the absence of the salts themselves than to the consequent diminution of water attracted and retained by the salts.—Papers were contributed by M. E. Goursat, on isogonal transformations in mechanics; by M. S. Arloing, on the general effects of the substances produced by *Bacillus heminecrobiophilus* under natural and artificial culture; by M. Stanislas Meunier, on the Carboniferous rocks containing *Bacillarites*, Stur; and by M. de Rouville, on the genus *Amphion* (*Pander*) in the Cabrières district, Hérault.

Berlin.

Physiological Society, February 1.—Prof. du Bois Reymond, President, in the chair.—Prof. Moebius spoke on the movements of the flying-fish through the air. He first described, from personal observation, the way in which the fish shoot out of the water from both bows of the ship, and then propel themselves horizontally for a distance of several ship's-lengths with their pectoral and abdominal fins stretched out flat, skimming along without moving their fins, always in the direction of the wind, but either with or against the same. When they meet the crest of a wave they raise themselves slightly in the air, falling again to the same extent in the succeeding trough of the sea. Occasionally a slight buzzing of the fins may be observed, similar to that of the movements of the wings in many insects. At night they frequently fall on the deck of the ship. As the result of a detailed investigation, the speaker had proved that these fish do not fly, since the anatomical arrangements of their fins and muscles are not adapted to this purpose. What really occurs is that when frightened by the approach of a ship or any enemy they shoot up out of the water, as do so many other fish, and are then carried along by the wind, which strikes on the under surface of their outstretched and evenly-balanced fins. Notwithstanding the general acceptance which was accorded to the above investigation, it was urged by many that the buzzing of the fins, the rising over the crest of a wave, and the falling overboard after having landed on the deck of a ship, were evidences that this fish really executes movements which result in flight. In reply to this, the speaker pointed out that the buzzing of the fins takes place when a strong current of air is directed against the outspread fins of a dead flying-fish by means of a bellows, and further, that the rising over the crest of a wave or the bulwarks of a ship may be explained by the ascending currents of air which are always produced whenever a strong horizontal wind strikes against any elevated object such as a wave or part of a ship. Thus, finally, with the exception of the movements involved in its oblique sudden exit from the sea, all the motions of a flying-fish when in the air are really passive.—Dr. Posner spoke on the conversion of mucous membrane into cuticular tissue. It has long been known that ectodermal tissue can become converted into that which is characteristic of the alimentary tract; thus, for instance, when pieces of skin are transplanted into the cavity of the mouth, they become completely converted into mucous membrane, and the epidermis becomes an epithelium. On the other hand, no observations existed as to whether the reverse conversion of mucous into epidermal tissue is possible. No conclusive evidence could be drawn from the cuticular conversions which occur in mucous membranes which are derived from ingrowths of the ectoderm, and equally inconclusive were the cases of cuticular conversion which are observed in strictures of the urethra, and in the bladder, and in cases of papilloma and pachydermia of the mucous membrane of the mouth, larynx, and œsophagus. The speaker had found in Leidig a hint that in those animals which do not chew their food a conversion of the gastric mucous membrane occurs, which often amounts to a real cuticular formation. It is true that in birds the gizzard is possessed of tubular glands which pour out a secretion which at once sets into a mass as hard as bone, and provides a means of comminuting the food. But, on the other hand, cuticular growths are observed in the stomachs of Edentata, which consist of a true conversion of epithelium into epidermis. The speaker had been able to study this conversion in the stomach of Manis, and found by chemical and microscopical investigation that the whole stomach, even down to the region of the pylorus, is lined with a true epidermis, and that typical papillæ are developed underneath the same. The conversion of endodermal into ectodermal structures is hereby clearly proved, a fact which is not devoid of significance in pathology.

February 15.—Prof. du Bois Reymond, President, in the chair.—Prof. F. E. Schulze spoke on the organization and mode of living of Sponges. The simplest form of Sponge consists of a sac, which, being composed of three layers, is equivalent to the embryonic form of the more complicated types. This sac is attached by its base, and has at its apex an opening—the osculum; its wall is composed of an outer layer of epithelial cells (ectoderm), a middle layer of connective-tissue with migratory cells (mesoderm), and an inner layer of collared flagellated cells (endoderm), and is perforated with round and regularly distributed apertures. The more complex forms arise by a thickening and folding of the wall, the thickening being still more marked in the most complicated forms. In these, a system of branched canals takes the place of the simple apertures in the wall, communicating with the exterior by round openings, and leading internally into cavities lined with collared flagellated cells (choanocytes); from these cavities a further system of branched canals leads into the now limited internal cavity with its osculum. The skeleton of the Sponge is composed of chalk, siliceous earth, spongin, or foreign substances. In the simplest Sponges, consisting of a simple sac, the skeleton is made up of the simplest star-shaped spicules, with three rays, so as to give support to a membrane which is perforated with regularly alternating apertures; in the more complicated forms, these spicules possess four or six rays, as supplying the most convenient supporting structure, while a collar of simple rays is developed round the osculum. According to the material of which the skeleton is chiefly composed, Sponges may be classified as chalky, siliceous, horny, or sandy. The Hexactinellidæ, with six-rayed spicules, inhabit deep seas. In all Sponges, a continuous stream of water is observed entering the openings on the surface of the body, and emerging at the osculum, so that the Sponges filter the water in which they live. The movement of the water is brought about by the flagella; the contraction and retraction of the osculum is produced by elongated protoplasmic cells, but these cannot be regarded as muscle-cells until it has been proved to a certainty that they are connected with nerves, and receive their impulse from these nerves. Only one observer has as yet described a nervous system in Sponges, the majority not yet having seen it. Nothing has as yet been definitely ascertained as to the mode of nutrition of Sponges: from among the several hypotheses, some suppose that the solid organic particles which are suspended in the water are taken up by the ectoderm-cells and digested in the body; or else that these particles are taken up by the flagellated cells—that is to say, are passed from within outwards; or that the digestion is cellular, inasmuch as the amœboid migratory cells take up the food-particles, digest them, and pass on the digestive products to the rest of the body; or, finally, that Sponges, like plants, only absorb food-stuffs in solution. The reproduction of Sponges is both asexual and sexual, the first resulting from natural or artificial fission: natural fission, consisting of a simple separation, by constriction, of a portion of the body-substance, occurs in fresh-water Sponges. A further asexual mode of reproduction is by means of buds and gemmules. In the sexual mode of reproduction, the females develop eggs all over their surface, and the males spermatozoa, the latter consisting of a head and tail. The egg, after impregnation, goes through the various stages of segmentation met with in the higher animals, and then develops into a sac-like embryo.—Dr. Uhthoff gave an account of his researches on the dependence of visual acuteness in spectral colours upon the intensity and wave-length of the light; these have been recently reported to the Physical Society by Dr. Kœnig (NATURE, February 21, p. 408).

Physical Society, February 8.—Prof. Kundt, President, in the chair.—Dr. Michelson spoke on the normal rate of combustion of explosive mixtures of gases. When such a mixture is ignited at one point, and the temperature of combustion is propagated only by conduction from this point, then the surface at which combustion is taking place separates the burnt from the still unburnt portion of the mixture; the temperature of this unburnt portion is then raised by conduction to that at which it ignites, and it burns. Mallard and Le Chatelier have determined the rate at which this ignition is propagated, by observing the onward movement of the flame in a cylindrical vessel filled with the mixture of gases; Bunsen, on the other hand, allowed the explosive mixture to stream out of a burner with a known velocity, and took this as being equal to the rate of propagation of the combustion in the case where the flame was just on the point of striking back into the tube to which the burner was attached. An objection which may be urged against the first method is that the velocity with which the flame is propagated increases very rapidly the further it travels, so that it is uniform only at the beginning of the explosion. The objection to the second method is, that the flame is continually wavering in and out of the edge of the burner. Dr. Michelson made use, in his experiments, of the dark cone in the centre of the flame, in which the gases are still unburnt, and whose luminous envelope forms the limit of the commencing combustion. When the rate of supply of the combustible gases is uniform, this cone is very steady, and the rate at which the gases stream out from its surface is exactly equal to the rate of combustion of these gases. The volumes of the gases consumed were measured in accurate meters, and the size of the luminous envelope to the centre cone of the flame was determined from photographs of the flame. The mixtures examined were those of coal-gas and air, hydrogen and air, carbonic oxide and oxygen, hydrogen and oxygen, carbonic oxide and air, and of methane and air. With coal-gas and air the rate of combustion increased as the mixture contained more of the coal-gas, reaching a maximum with 18 per cent. of this gas, and then gradually became less; the maximum rate of propagation of the combustion was 70 centimetres per second. With hydrogen and air, the maximum rate was observed with 40 per cent. of hydrogen in the mixture, being then 270 centimetres per second, and then becoming less. The curve representing the rate of combustion of carbonic oxide and oxygen presented a very different appearance. The maximum rate was only obtained with 75 per cent. of the carbonic oxide, and was about equal to the maximum for coal-gas. For the other three mixtures, no curves could be drawn. With a mixture of hydrogen and oxygen, the speaker estimated the maximum rate of combustion as being about 10 metres per second, but no actual measurement was possible, since the mixture could not be expelled at this rate from the burner, which consisted of a glass tube 1 metre long.—Prof. Preyer spoke on combination-tones. He endeavoured to prove that difference- and summation-tones have no objective existence. The first of these are the outcome of a co-vibration in the inner ear. When two different tuning-forks are made to vibrate for a long time, and are then damped, and a third tuning-fork, whose vibration-frequency is only slightly greater than that of the difference in frequency of the other two, is applied to the head, the experimenter hears the corresponding beats. Persons with a defective tympanic membrane in one ear, and a normal membrane in the other, were unable to appreciate difference-tones with the former which they could perfectly well do with the latter. According to the speaker, the summation-tones are really difference-tones, due to the fundamental tone and over-tones of the vibrating forks.—Prof. H. W. Vogel exhibited a complete spectrum of cyanogen, which he had obtained by photographing the spectrum of an arc-light produced by a concave grating; the spectrum extended in the red beyond the line A. Most remarkable was the abundance of lines which were fixed on the photograph, and made visible. The speaker discussed briefly the respective advantages of a concave grating for laboratory experiments on spectra, and of the ordinary prism for practical purposes, especially for observations during solar eclipses.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Scottish Moors and Indian Jungles: Captain J. T. Newall (Hurst and Blackett).—The Scientific Works of C. William Siemens, 3 vols.: edited by E. F. Bamber (Murray).—The Collected Mathematical Papers of Arthur Cayley, vol. i. (Cambridge Press).—A Catalogue of the Newton Manuscripts, Portsmouth Collection (Cambridge Press).—The Gamekeeper's Manual: A. Porter (Douglas).—Catalogue of the Fossil Cephalopoda in the British Museum (Natural History), part 1: A. H. Foord (London).—Catalogue of the Chelonians, Rhynchocephalians, and Crocodiles, in the British Museum (Natural History), new edition: G. A. Boulenger (London).—A Hand-book of Cryptogamic Botany: A. W. Bennett and G. Murray (Longmans).—Blackie's Modern Cyclopedia, vol. i.: edited by C. Annandale (Blackie).—A Treatise on Chemistry, vol. ii., part 2, new edition: Sir H. E. Roscoe and C. Schorlemmer (Macmillan)—Henri Sainte-Claire Deville; sa Vie et des Travaux: J. Gay (Paris, Gauthier-Villars).—The House that Jack Built in Diversified Consideration: F. J. Wilson (Reeves)—Encyklopædie der Naturwissenschaften Erste Abthg., 59 Liefg.; Zweite Abthg., 51 and 52 Liefg. (Breslau, Trewendt).—Our Earth and its Story: edited by P. Brown (Cassell).—Revision of North American Umbelliferæ: J. M. Coulter and J. N. Rose.—Notes from the Leyden Museum, vol. x. Nos. 1-4, vol. xi. No. 1 (Leyden).—Folk-Lore Journal, vol. vii. part 1 (E. Stock).

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