THURSDAY, MARCH 23, 1893.

COLLIERS AND COLLIERY EXPLOSIONS.

Coal Pits and Pitmen. A Short History of the Coal Trade, and the Legislation affecting it. By R. Nelson Boyd, M.Inst.C.E. (London: Whittaker and Co.)

AS the author remarks in his preface, his present work is a re-cast of a book published for him in 1879 by W. H. Allen and Co., under the title of "Coal Mines' Inspection." A casual examination of both books shows that they are alike in their main features; only, the latter work has been extended so as to include some of the events of later years. The subject is divided into twelve chapters, to which are added four short appendices and a good index. The text extends to 239 pages 8vo of good readable print, and there are a few good illustrations of ancient mechanical arrangements, including the steel mill.

Mr. Boyd begins by giving a very short historical account of the situation before Parliament began to interfere in the relations between masters and men. He then describes the circumstances which led to the appointment of successive Royal Commissions, charged to inquire into various matters relating to mines and miners, and he sketches briefly the leading features of the reports presented by these Commissions, together with the chief points of interest contained in the legislative enactments which were founded upon some of them. Our author also pauses from time to time to recount in considerable detail the events of more than passing interest, such as explosions, inundations, and other accidents which happened during the period with which he is dealing; and lastly, in his appendices, he gives the titles of the Acts of Parliament affecting coal mines and miners, both English and Scotch, a list of serious colliery explosions previous to and since 1850, and a table showing the production of coal at different times, commencing in 1660, and brought down to 1891.

Among other more or less important provisions of the Acts of Parliament our author gives prominence to:—The exclusion of women from mines, the appointment of Government inspectors, the limitations of the ages at which boys can be employed, the restrictions under which explosives may be used, the requirement that each mine should have two distinct shafts, that each mine manager must have a certificate similar to that of a sea captain, that payment must be made by weight and not by measure, the conditions under which safety lamps are to be used, and the method of dealing with coal-dust when it is present.

He also reviews such questions as the payment of royalties and wayleaves to landlords, the employers' liability, the wasteful consumption of fuel, the duration of the coal supplies, and old-age pensions to miners.

Contrasting the present with the past he says:—"The workmen of the present day have attained a distinct social position, have representation in the House of Commons, and trade unions, societies, and powerful combinations," whereas formerly the Scotch colliers were *adscriptæ glebæ*, that is, were bought and sold with the land: an Act of the Scotch Parliament of 1660 prohibited them from leaving their employment without a written attestation from their masters under pain of punishment in their bodies, and any person employing them was ordered to return them within twenty-four hours or pay a fine of one hundred pounds Scots. The colliers of the North of England were little better, being hired by the year under a system of binding or bonding; those of the Bristol coal-field were described by contemporary writers as being as brutal and ignorant as savages. Colliers lived apart from the rest of the community, were looked down upon with contempt by their fellow-men, and diverted themselves with bull-baiting, drinking, and debauchery.

The state of serfdom was removed by various Acts of Parliament passed during the latter half of last century, apparently more with the object of increasing the numbers of the colliers by drawing other classes of labourers into the mines than from any specially humanitarian motives. But at length a day dawned when higher principles began to prevail. From 1842, when Lord Ashley's Act was passed for the exclusion of women from the mines, onwards to 1887, one Act was passed after another, each having the same object in view, namely, the amelioration of the lot of the miner.

The frequent occurrence of disastrous colliery explosions and the great destruction of life and property which accompanied them, had done more than anything else to draw the attention of the public to a consideration of mining affairs, and had likewise been the principal incentive to the appointment of Royal Commissions and to the passing of Acts of Parliament to regulate the supervision and the working of mines.

Notwithstanding all that had been done previously, more lives were lost in explosions during the five years ending 1870 than during any preceding five years, the aggregate number lost in fifteen explosions, each of which involved the loss of ten lives and upwards, having amounted to 923.

The Coal Mines' Regulation Act of 1872 was drawn up with great care by Mr. Bruce (now Lord Aberdare), the Home Secretary. It embraced the experience of the Inspectors of Mines, as well as the combined wisdom of mine owners, engineers, managers of mines, and delegates of the colliers. But, firedamp having been hitherto regarded as the sole cause of colliery explosions, the stringent provisions of that Act were directed exclusively towards the detection and removal of that element of danger.

In 1845 Messrs. Lyell and Faraday, who had examined the scene of the Haswell colliery explosion, which they attributed to large accumulations of firedamp in the empty spaces (goaves) from which pillars had been removed, remarked that firedamp had not been its only fuel, but that doubtless the coal-dust which was raised and swept along *by the firedamp flame* would be decomposed by the heat of that flame, and would therefore add to the force of the explosion.

Between the years 1860 and 1875 several French mining engineers were impressed with the idea that coal-dust had played a part in certain explosions which took place in France. Some experiments were made by a committee of the Société de l'Industrie Minérale, and by M.

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Vital, one of the Ingénieurs des Mines, with the object of ascertaining the likelihood or otherwise of this hypothesis, but no definite conclusions were arrived at.

In December, 1875, the present writer examined into all the circumstances attending a colliery explosion in South Wales, and gave a minute description of it before a coroner's jury. He insisted that coal-dust had been the principal agent in that explosion, and that firedamp had only played a subordinate part. At the same time he referred to the results of experiments he had made which showed conclusively that when fine dry coal-dust is added to a mixture of air and firedamp, in which the firedamp is present in so small a proportion as to escape detection by the means employed for this purpose in mines, the mixture is inflammable at ordinary pressure and temperature, and when ignited burns like a jet of inflammable gas. In March, 1876, he read a paper before the Royal Society, in which he described these experiments, as well as the apparatus with which they had been carried out. In this paper, also, he claimed that when an explosion is once begun in a dry and dusty mine it becomes self-propagating, and, provided the continuity of the deposit of coal-dust is unbroken, it extends to the utmost limits of the workings. This became known afterwards as the "Coal-dust Theory of Great Colliery Explosions."

In May, 1876, Mr. Hall, one of the Inspectors of Mines, read his first paper on the subject before a meeting of the North of England Institute of Mining and Mechanical Engineers held in London.

During the year 1878 the present writer published a series of papers on "Coal-dust Explosions" in "Iron"; and while they were appearing Messrs. Marrecco and Morrison read their first paper on the subject.

Numerous societies of mining men, and individuals more or less connected with mining, now began to take an interest in the subject, and to make experiments with coal-dust. About this time, also, Commissions were appointed by the Governments of England (Royal Commission on Accidents in Mines 1879), France (Commission du Grisou), Prussia and Austria, to inquire into the causes of mining accidents, and amongst other things to investigate the probable influence of coal-dust in colliery explosions.

This sudden activity was no doubt quickened by the events of the ten years ending with 1880, during which the loss of life from explosions was twice as great as it had been during any previous decade. Taking into account only those explosions in which ten lives and upwards were lost, we find that there were thirty-five explosions, involving the lives of 2014 persons, of which 1411 were attributable to the second half of the decade.

In 1880 Prof. (now Sir Frederick) Abel, one of the English Commissioners, was instructed by the Home Secretary to investigate and make a special report upon the Seaham colliery explosion (September 8, 1880). During the course of these investigations Abel repeated our experiments of 1875-76 with a similar apparatus with practically the same results as far as coal-dust is concerned; but he claimed in addition to have discovered that any very finely divided incombustible dust would render a mixture of air with 3 or 4 per cent. of firedamp inflammable. His apparatus was not, however, provided with any special means of mixing the gas and air such as had been used in our own apparatus, of which it was otherwise a copy.

The German Commissioners erected and made experiments with an apparatus similar to one that we had described to the Royal Society in 1881, but on a somewhat larger scale, and they obtained similar results. Unfortunately, however, they passed away from the main question, viz. whether an explosion that *has taken place* in a dry and dusty mine under the circumstances that would have been formerly described as mysterious, can be attributed to the influence of the coal-dust in the supposed absence of firedamp?

The French Commissioners arrived at conclusions adverse to the coal-dust theory. They made no special experiments with coal-dust on an important scale and they did not, so far as can be gathered from their reports, examine the workings of any mine immediately after an explosion.

The Austrian Commissioners arrived at the conclusion which we had previously stated in this country, namely, that the relative fineness of a dust has far more to do with its relative inflammability than its chemical composition.

The English Commissioners expressed an oracular opinion. They denied on the one hand that coal-dust could be the principal agent in great colliery explosions, for, "If that were the case," said they, "an explosion would happen every day, nay every hour." But, on the other hand, they endeavoured to point out that coal-dust may be an element of the gravest danger under certain circumstances which they proceeded to define in a very precise manner. The Act of 1887 embodies their recommendations regarding safety lamps, explosives, and coal-dust.

From the end of 1875 onwards attention had been more and more directed to the coal-dust question. It had been observed that a great explosion never by any chance took place in a damp or wet mine, that when such an explosion took place in a dry and dusty mine, its progress was always arrested by dampness or wetness or by the absence of coal-dust, that it always passed through the dry intake airways, which contain pure air, and comparatively clean coal-dust, that it frequently avoided the return airways, which contain all the firedamp produced in the workings, but impure coal-dust or only stone-dust, and, lastly, that it spread into all the districts of the workings ventilated by separate and distinct intake and return airways, quite irrespective of the force or direction of the ventilating currents, and dependent only upon the one simple but indispensable condition that the train of dry coal-dust continued unbroken, or was interrupted only for short distances here and there. These facts were proved to demonstration by the researches of a number of independent observers in the mines themselves, immediately after the occurrence of explosions. In vain have the opponents of the coal-dust theory, who were at one time very numerous, urged that the intake airways might have contained firedamp, that the coal-dust cloud raised and ignited by a local disturbance, such as the firing of a blasting shot, probably acted as a connecting link which carried the flame from one accumulation of firedamp to another, that if coal-dust was as dangerous as it was represented to be, an explosion would take place every day, nay, every hour; that certain kinds of coal-dust were perhaps less inflammable than others, and so on.

Comparatively few have had the advantage of carefully studying the coal-dust flame as well as the opportunity of investigating the minutest details of a series of great colliery explosions in the mines, immediately after their occurrence. The foregoing arguments are therefore perhaps to some extent excusable; but they are none the less the outcome of the imagination of their authors. They are being pressed more and more feebly as time goes on, and they are likely, we think, before many years have passed, to vanish as absolutely as the so-called "outburst of gas" theory which for more than a generation was invariably quoted as the only possible means of accounting for the kind of explosions to which we have been drawing attention.

The late Home Secretary, Mr. Matthews, was so much impressed by the occurrence of great explosions one after the other in dry and dusty mines, that he appointed a Royal Commission on Coal-Dust in 1890. That Commission has not yet issued its report, but the volume of evidence taken before it, which has been lately published, shows to what small proportions the opposition has shrunk since the theory was first started. It is also satisfactory to observe that the number of lives lost in great explosions during the last ten years is only about one half of the number lost during the previous ten years.

W. G.

REVERIES OF A NATURALIST.

Idle Days in Patagonia. By W. H. Hudson, C.M.Z.S., Author of "The Naturalist in La Plata." (London: Chapman and Hall, 1893.)

THE title of this book well describes its contents; but Mr. Hudson has established so high a standard by his previous work that the present volume has something of the character of an anti-climax. In literary style, in picturesque description, and in suggestive ideas and reflections there is no falling off; but we miss the wealth of original observation and ingenious speculation which made "The Naturalist in La Plata" a masterpiece.

Mr. Hudson was wrecked on the shores of Patagonia, and had a weary tramp over the desert, of some thirty miles, to reach the settlement on the Rio Negro. There, and at some farms higher up the valley, he appears to have spent a year or more, doing nothing but wandering about on foot or on horseback, observing the habits and peculiarities of the scanty fauna and flora, noting the varied aspects of nature, and apparently thoroughly enjoying day after day of dreamy idleness. He spent some months at a house about seventy miles up the valley, which was here about five miles wide; and every morning he rode away to the terrace or plateau, covered with grey thorny scrub, and there found himself as completely alone as if he were five hundred instead of only five miles from civilisation. He says:—

"Not once, nor twice, nor thrice, but day after day I returned to this solitude, going to it in the morning as if to attend a festival, and leaving it only when hunger and thirst and the westering sun compelled me. And yet I had no object in going—no motive which could be put into words; for although I carried a gun, there was nothing to shoot—the shooting was all left behind in the valley. Sometimes a dolichotis, starting up at my approach, flashed for one moment on my sight, to vanish the next moment in the continuous thicket; or a covey of tinamous sprang rocket-like into the air, and fled away with long wailing notes and loud whirr of wings; or, on some distant hillside a bright patch of yellow, of a deer that was watching me, appeared and remained motionless for two or three minutes. But the animals were few, and sometimes I would pass an entire day without seeing one mammal, and perhaps not more than a dozen birds of any size."

There was nothing beautiful or even pleasing to be seen in this dreary monotonous solitude, yet he felt a great delight and satisfaction in it, which he imputes to the ancestral savage nature that still exists in all of us, though repressed and overlaid by civilisation and society.

"It was elation of this kind, the feeling experienced on going back to a mental condition we have outgrown, which I had in the Patagonian solitude; for I had undoubtedly *gone back;* and that state of intense watchfulness, or alertness rather, with suspension of the higher intellectual faculties, represented the mental state of the pure savage."

In the second chapter—"How I became an Idler"—we are told of a still more disagreeable adventure than the shipwreck. Mr. Hudson was going with a friend to a farm eighty miles up the valley. On the way they stayed a night at a deserted hut, and here he had the misfortune accidentally to discharge a revolver bullet into his knee, rendering it necessary for him to return to the settlement to be cured, perhaps to save his life. His friend tied up the wound as well as he could, and left him to get a cart from the nearest house a good distance off. He was absent a whole day, Mr. Hudson lying on his back on the ground all the time. When his companion at length returned with the cart, and lifted him up to put him into it, a large and very poisonous snake moved from under his cloak, where it had been lying close to his feet for many hours. It glided away into a hole under the wall, and Mr. Hudson rejoiced "that the secret deadly creature, after lying all night with me, warming its chilly blood with my warmth, went back unbruised to its den."

This accident kept the author for some months in bed, and for other months a convalescent unable to walk far; and thus the finest summer weather was wasted, and he acquired those habits of the country and the people that made him an idler, and prevented him from learning as much of the animal and vegetable life of the country as, under more favourable circumstances, he might have done. Yet he gives us many interesting facts and discussions, and the chapter on "The War with Nature" is one of these. This war begins when man introduces domestic cattle, cultivates the soil, and destroys the larger wild animals for food or sport. In doing this he provides food of an attractive kind for many wild creatures, and the war begins. Pumas devour his cattle; locusts destroy his grass or crops; coots, ducks, geese, or pigeons devour the grain as soon as sown, or feed upon the young shoots, or upon the ripe wheat ready for the harvest; and thus the farmer is kept in a constant state of activity and watchfulness, which really gives him a beneficial excitement in what would otherwise be a most monotonous and unattractive existence. In one of his glowing passages Mr. Hudson thus describes and personifies the war between nature and man.

"He scatters the seed, and when he looks for the green heads to appear, the earth opens, and lo! an army of long-faced yellow grasshoppers come forth! She too, walking invisible at his side, had scattered her miraculous seed along with his. He will not be beaten by her, he slays her striped and spotted creatures; he dries up her marshes; he consumes her forests and prairies with fire, and her wild things perish in myriads; he covers her plains with herds of cattle, and waving fields of corn, and orchards of fruit-bearing trees. She hides her bitter wrath in her heart, secretly she goes out at dawn of day and blows her trumpet on the hills summoning her innumerable children to her aid. Nor are they slow to hear. From north and south, from east and west, they come in armies of creeping things, and in clouds that darken the air. Mice and crickets swarm in the fields; a thousand insolent birds pull his scarecrows to pieces, and carry off the straw stuffing to build their nests; every green thing is devoured; the trees, stripped of their bark stand like great white skeletons in the bare desolate fields, cracked and scorched by the pitiless sun. When he is in despair deliverance comes; famine falls on the mighty host of his enemies; they devour each other and perish utterly. Still he lives to lament his loss; to strive still unsubdued and resolute. And she, too, is unsubdued; she has found a new weapon it will take him long to wrest from her hands. Out of the many little humble plants she fashions the mighty noxious weeds; they spring up in his footsteps, following him everywhere, and possess his fields like parasites, sucking up their moisture and killing their fertility. Everywhere as if by a miracle, is spread the mantle of rich, green, noisome leaves, and the corn is smothered in beautiful flowers that yield only bitter seed and poison fruit. With her beloved weeds she will wear out his spirit and break his heart; she will sit still at a distance while he grows weary of the hopeless struggle; and at last, when he is ready to faint, she will go forth once more, and blow her trumpet on the hills and call her innumerable children to fall on him and destroy him utterly."

This, the author tells us, is no fancy picture, but one painted from nature in true colours. If so it is not encouraging for emigrants; but then, the climate is superb, and it is a proverb that "once in a hundred years a man dies in Patagonia." Then, again, the bird music is unsurpassed; there are numerous exquisite songsters; and of one of them—the mocking bird, he declares that the song is so varied and beautiful that all the music of our song-thrush might be taken out of it and not be much missed. Azara declared that there were as many and as good songsters in Paraguay and La Plata as in Europe, and Mr. Hudson agrees with him. The reason why Darwin and other travellers thought otherwise is, because most of the South American songsters are shy wood-birds which rarely approach man's dwellings, and are therefore only heard by those who seek them; whereas in Europe they are mostly species which haunt gardens and orchards, and cultivated fields, and are thus more or less familiar to every one.

In a chapter on "Sight in Savages" it is maintained that they have no superiority in this respect to civilised man; and that what often seems like better sight is merely trained observation of objects which it is essential for them to know. There is an amusing story of a middle-aged Gaucho, who laughed and jeered at an Englishman for wearing spectacles, and would not believe that bits of glass over his eyes could possibly make him see better. The gentleman persuaded the man to try them, and they happened exactly to suit his sight, which had gradually grown imperfect without his knowing it. He stared round, utterly amazed, and then shouted:—"Angels of heaven, what is this I see! What makes the trees so green—they were never so green before! I can count their leaves! And the cart over there—why it is red as blood." And he went up to it to be sure it had not been fresh painted. There is also a chapter—"Concerning Eyes"—dealing with their characteristic colours, their scintillation under excitement, and the uses of these peculiarities, a subject to which Mr. Hudson has given much attention. Many old Indian burial places and village sites were found, with abundance of arrow-heads, flint knives, scrapers, mortars and pestles, stone anvils, pottery, and other objects. There were two kinds of arrow-heads, some large and very rude, others smaller and exquisitely finished, the former found mostly on the plateau, the latter in the valley. One of the village sites, where the greatest number of objects was found, had been buried seven or eight feet, and was exposed by heavy rains, which had washed away great masses of gravel and sand. Many of the smaller arrow-heads were of crystal, agate, green, yellow, or horn-coloured flint, and were perfect gems of colour and workmanship, and these were all found at one spot. Unfortunately, most of the finest specimens, which had been packed separately for security, were lost on his homeward journey—"a severe blow," Mr. Hudson says, "which hurt me more than the wound I had received on the knee."

Although this volume cannot have the same absorbing interest for the naturalist as the author's previous work, it is yet full of suggestive observations and reflections, and gives us a vivid picture of both animate and inanimate nature in one of the least known portions of the southern hemisphere. The volume is nicely got up, and is illustrated with a number of landscapes and figures of men and animals in the same style as in the author's former work.

ALFRED R. WALLACE.

OUR BOOK SHELF.

Ueber das Verhalten des Pollens und die Befruchtungsvorgänge bei den Gymnospermen. Von Eduard Strasburger. (Jena: Gustav Fischer, 1892.)

THIS forms the fourth part of Prof. Strasburger's "Histologische Beiträge," and it is largely taken up with an examination of segmentation in pollen-grains of the gymnosperms, and the contents of, and processes in, the pollen-tubes. Recent discoveries had led Strasburger to doubt the correctness of his former interpretation of the contents of the pollen-tubes, and his further researches have "confirmed in a surprising manner" the results obtained by Belajeff in his paper on *Taxus baccata*, entitled "Zur Lehre von den Pollenschläuchen der Gymnospermen." Strasburger is also essentially in accord with Belajeff's generalisations therefrom. Two double plates illustrate division in the pollen-grain, the development of the pollen-tube, and the further processes of fertilisation in various gymnosperms, including *Taxus*, *Pinus*, *Ginkgo*, and *Welwitschia*. An unusual condition is shown of cell-division in a pollen-grain of *Ginkgo*. Usually two or three "prothallium cells" are formed, and in part disappear before the protrusion of the pollen-tube and the division of the "generative cell"; but occasionally they persist somewhat longer, and Strasburger figures a pollen-grain in which the three prothallium cells are intact, and the first of them has a partition at right-angles to the walls of the other cells. In this work Strasburger also gives the results of some experiments on the colour-reactions of the male and female nuclei. Rosen discovered that, as in animals, the male nucleus of phanerogams is kyanophilous and the female nucleus erythrophilous. Strasburger found that the small nuclei of the cells formed in the pollen grains of gymnosperms were kyanophilous, whether the cells were vegetative or destined for generation; but the nucleus of the pollen-tube was more or less decidedly erythrophilous. The second and larger portion of this "Beitrag" is devoted to swarmspores, gametes, vegetable spermatozoids, and the nature of fertilisation. W. B. H.

Autres Mondes. By Amédée Guillemin. (Paris: Georges Carré, 1892.)

WHETHER the author of this small volume thought that the sequence of the subjects dealt with was really quite unimportant, or whether no order at all was intended, puzzled me considerably when glancing through these pages for the first time. To be suddenly led off without a word of warning into "L'infini dans le temps et dans l'espace," and then to be as suddenly pulled back again to a second chapter dealing with Sirius seems rather a large oscillation to commence with. The same remarks might apply to the next chapters, for they treat consecutively of "The Cluster in Hercules," "Structure of the Visible Universe," "Movement in the Universe," and "The Nebula of Orion," followed up by chapters on "The Age of Stars," and "The End of the Solar System."

That the work is written by M. Guillemin is quite sufficient guarantee that strict accuracy is throughout adhered to. The book is one that can be picked up at odd moments and a chapter or two read with delight. The illustrations are excellent copies of lunar and stellar photographs taken by the brothers Henry at the Paris Observatory. W. J. L.

Some Lectures by the late Sir George E. Paget, K.C.B., F.R.S. Edited, with Memoir, by Charles E. Paget. (Cambridge: Macmillan and Bowes, 1893.)

THIS volume will be cordially welcomed by the late Sir George Paget's friends; and members of the medical profession, whether they knew him personally or not, will find in it much that cannot fail to interest them. The lectures deal with three subjects—the ætiology of typhoid fever, alcohol as a cause of disease, and mental causes of bodily disease. In dealing with each of these topics, the author presents the results of prolonged and most careful observation; and it is impossible not to admire the directness, lucidity, and vigour with which his facts and conclusions are set forth. The memoir, by the editor, is a short and attractive record of Sir George Paget's distinguished career, and its value is increased by the fact that it is accompanied by an excellent portrait.

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.*]

Origin of Lake Basins.

WE may all thank Mr. Alfred Wallace for putting together so concisely the main arguments on which the glacial theory of the origin of all lake basins has had a wide acceptance. My time is just now so occupied with "earth movements" of another kind that I am unable to marshal all the arguments on the other side. But I shall try to put the main points as clearly as I can.

I accept Mr. Wallace's correction of the word "grinding" as the best word to describe the action of glaciers. It is better than either "digging up" or "scooping." Many men who account for marine gravels on such places as Moel Trefan mountain-top by the action of glaciers, *must* conceive of glaciers as capable of digging out and lifting up. But I agree with Mr. Wallace that "grinding" down is the best expression for true glacier action. This is the *mode* of action; but what of the *cause* of the motion which effects the grinding? Are we agreed on this? Mr. Wallace does not explain his view on this point. I hold that the only cause of true glacier action is gravitation, and that masses of ice will not move at all, or exert any grinding action, except when impelled by gravity down gradients more or less steep. Even if they do mount up some slopes, it is only when they are violently pushed by other masses moving down slopes from behind them. If this be true, then glaciers will not tend to dig holes out of the flat bottoms of valleys. Mr. Wallace says they will, if they are exceptionally thick. This is very doubtful: and still more is it doubtful that they can dig holes of a very peculiar character, such as is now proved to be the character of Como and other lakes, with steep and sharp outlines, or with barriers left untouched. One single fact of this kind, if well ascertained, is quite enough to upset a great theory, because it may be sufficient to prove that at least *some* lake basins *cannot* have been made by glaciers. And if some have not, it is not certain that any have been made by glaciers alone.

The constant association of lake basins with glaciated countries is Mr. Wallace's grand argument. But it is explicable in the theory of earth movements quite as easily as on the theory of glacial action. Glaciated countries are generally hilly, or mountainous. If Mr. Wallace believes that all hills and valleys are due to superficial *sculpturing alone*, of course his argument is facilitated. But if hills and valleys are even in any measure due to earth movements—crumplings of the surface—then the formation of lake basins is an inevitable necessity. Every hollow must become a lake basin which has no natural outlet except at a higher level than at its own bottom. Yet if there be such a thing as earth movements at all, it is in the highest degree improbable that they should have failed in numerous cases to occasion hollows in which water would accumulate.

Mr. Wallace's unbelief that any earth movements have taken place so lately in geological time as the glacial age—say 100,000 years ago—is a declaration that does indeed astonish me. I can understand great doubt and difficulty as to the extent of these movements. But that they have taken place to some extent very lately indeed is, in my opinion, demonstrable in the country in which I now write. There is one old sea beach on the Island of Jura where the stones as left by the surf are as bare of vegetation and as unaltered in forms which show surf action, as if the ocean had beat upon it last year. And this sea beach extends for miles at elevations varying from 120 to (I believe) 160 feet. If I am not mistaken, recent surveys of the great Canadian and American lakes have proved that they lie in hollows of crumpled and distorted land surfaces. The whole of Mr. Wallace's theory on this subject seems to me to be out of date. The distribution of boulders in the Highlands can, in my opinion, be accounted for in no other way than the transport of masses of stone on floating ice. But putting aside altogether this larger question, if a "great submergence," as one of the latest events in the glacial epoch, smaller elevations of the land are among the most certain of geological facts. But if so, we have lake-basins in all hilly countries easily explained. Very often the elevation of land to a very small extent indeed, if unequal, as it is sure to be more or less, would immediately cause lakes wherever a pre-existing valley had its lower end more tilted than its upper end. The 120 feet which is represented on the coast of Jura in this county is an elevation which would fill half of our glens all over the county with lakes unless it was an elevation perfectly equal along the whole of pre-existing contours. The co-existence of lake-basins with hilly and glaciated countries, therefore, admits of an easy explanation without attributing to ice a kind of action which has never been proved to exist at all. Hilly countries are *crumpled* countries, and slight increases or decreases of the same action must of necessity produce lakes.

Glaciers have, however, without doubt caused lakes in cases where they have dammed up the mouth of glens with detrital matter. The enormous masses of such matter which dam up the waters of the northern Italian lakes are most impressive. But it does not follow that the glaciers which left those great masses also scooped out the deep bed and rocky walls of the Lake of Como.

My own belief is that the great recency of large earth movements is one of the facts of geological science which has yet to be accepted; and that the slowness with which it has made progress, or has even been overborne, is entirely due to very natural preconceptions and general assumptions about the stability of the earth surfaces, such as those which find expression in Mr. Wallace's very interesting and significant paper. ARGYLL.

Inveraray, Argyllshire, March 11.

P.S.—Recent calculations in America seem to bring down the possible date of the close of the glacial epoch there to little more than 10,000 years.

The Cause of the Sexual Differences of Colour in Eclectus.

Mr. F. E. Beddard says in his suggestive work on "Animal Coloration" (1892, p. 3):—

"Sometimes differently coloured animals have in reality the same skin pigments. The attention of the reader will be directed in a later chapter to the remarkable difference in colour between the males and females of certain parrots. In *Eclectus polychlorus* this sexual dimorphism is extremely marked. It would be an exceedingly anomalous fact if the same species of bird were to possess different pigments in the two sexes; and as a matter of fact it is not so in this parrot, different in colour though the two sexes are. The same pigments are present, *but the structure of the feathers is different, and thus the resulting colour as seen by the eye is different.*"

The last sentence (the italics are mine) is not consistent with late Dr. Krukenberg's investigations on the colours of feathers. The case is not one of structural difference in the feathers, for the differences in colour between male and female of *Eclectus* are occasioned by the presence or absence of the pigment itself. The green colour of the male results from a yellow pigment (*psittacofulvin*) lying over a blackish brown one (*fuscin*), but the blue colour of the female (*E. linnei*, auct.) simply results from the absence of the yellow pigment. The dark pigment (*fuscin*) is present and the incident rays of light are reflected from it, passing through a zone without pigment, which zone absorbs the rays of the red extremity of the spectrum. Here the same conditions occur which effect the blue colour of the sky. The blue is an optical colour, as is the green, but a different structure of the feathers does not come into question. The red colour both in male and female is effected by a red pigment, which is the same in both sexes, the differences in shade (as also the violet in *E. grandis, e.g.*) depend on the quantity of this colouring substance and in the absence or presence in different quantities of the underlying *fuscin*. The pigment of the yellow feathers in the female of *E. grandis* is the same as the yellow pigment in the green males. Dr. Krukenberg supposes that these different pigments are derived from one and the same ground substance, a supposition which appears to be very plausible.

Why the yellow pigment of the male is not developed in the blue parts of the female we do not know, nor why the different pigments in *Eclectus* are disposed just as they are, since we are in general quite ignorant about the causes of the disposition of colours in bird feathers; but in the case under discussion a "different structure" of the feathers would not give as sufficient an explanation of the facts as does the above. Touching the *causa movens* of the different colours in the sexes of *Eclectus*, we can only say that it is sexuality, but this, of course, is no mechanical explanation, *i.e.* no true explanation at all. We can only say that in most birds the male offers an *overplus* of colour as compared with the female, which *overplus* no doubt has a relation to the more vigorous biological processes or superabundant vitality in the male during certain periods, and this also holds good in *Eclectus*, as we see that the female wants the yellow pigment which the male possesses. But we must bear in mind that in *Eclectus* the young ones from the egg display already these sexual differences of colour, a fact which is as remarkable as it is rare.

For reference see C. Fr. W. Krukenberg, "Die Farbstoffe der Federn," four papers in *Vergl. phys. Studien*, 1881 sq., and my papers, *Mitth. orn. Ver. Vienna*, 1881, p. 83, and *Sitz. ber. Akad. Wiss. Berlin*, 1882, p. 517 sq.

Dresden, March 8. A. B. MEYER.

Blind Animals in Caves.

Mr. Cunningham's notion as to what constitutes "a fact" would appear from his letter published in your issue of March 9 to be peculiar. It is of course only through inadvertence that he declares a mere supposition to be a fact, and states that I have "overlooked" it. His words are "he (Prof. Lankester) has overlooked the fact that blind cave-animals are born or hatched at the present day with well developed eyes." Further on he proceeds to state that no such fact is known or recorded, but that he is "quite confident" that the young of blind cave-animals have well developed eyes.

I am quite aware that an important test of the truth of my theory of the origin of blind cave-animals would be found in the details of their embryonic development, but cannot think that Mr. Cunningham is justified either in his confidence as to the result of a hitherto unattempted embryological research or in asserting what is at variance with his own subsequent avowal, viz. that there are facts ascertained as to the condition in which blind cave-animals are born, which I have ignored.

E. RAY LANKESTER.

Lunar "Volcanoes" and Lava Lakes.

I have waited some time to see what replies might be made to Mr. J. B. Hannay's suggestion, that lunar walled plains may have been due to tides in the molten nucleus during crust formation (Nature, vol. xlvii. p. 7).

There seem to be at least two objections to the "volcanic" theory of lunar surfacing. First, that there must have been during the earlier, and indeed later, stages of it a vast gaseous and vaporous envelope, which, as secular temperature slowly declined, would be condensed to form seas, giving rise to a long era of erosion, and extensive denudation, and formation of sedimentary strata, as on our earth. There are no traces of this on our moon, the surfacing of which is conspicuously destitute of evidences of drainage phenomena. Secondly, there is an entire absence of distinct local colour in the detail, which should be easily seen in volcanic deposits unencumbered by vegetation and weathering.

I leave it to geologists and physicists to say if they think it at all likely or possible for any globe like our moon to pass from the semi-incandescent, lava-crusted stage, with huge vaporous envelope, to the cold, airless, and waterless condition of our satellite without passing through a very prolonged era of erosion, which, as in our case, would obliterate all traces of the former era.

Judging by our vast series of stratified rocks, we are led to conclude that an exceedingly long temperate era of erosion must, in the very nature of things, supervene on the heated lava stage in all planetary development, quite obliterating the relics of the volcanic era and relaying a sedimentary surfacing.

Taking up the second objection, *in re* the marked absence of colour, I would point out the abnormal brightness, or even brilliancy, of the lunar cliffs and steep inclines all over the surface. It is precisely at such places that astronomers expect to see the nature of the surface and degradation due to the effect of gravitation, *i.e.* where (exposed to unmitigated solar heat in the day, and a cold probably below $-100°$ C. at night) the cliff-falls would be most frequent, and the true colour of the strata most visible.

Proctor in his "Moon" (pp. 301-2) says:—"In each lunation the moon's surface undergoes changes of temperature which should suffice to disintegrate large portions of her surface, and, with time, to crumble her loftiest mountains into shapeless heaps. In the long lunar night of fourteen days a cold far exceeding the intensest ever produced in terrestrial experiments must exist over the whole unilluminated hemisphere."

Neison, on page 113 of his "Moon," also says:—"That physical changes of various characters must be still occurring upon the moon is rendered certain by . . . the alternate heating and cooling of the lunar strata; from the nature of the expansion and contraction thus brought into play must, through numerous fractures of the resulting general disintegration, gradually ruin all the lunar formations." Thus "considerable changes must slowly be effected in the condition of the surface through earth-falls and landslips" . . "until all the more striking and abrupt irregularities have disappeared from their action."

Now it is precisely at cliff faces and steep slopes that we should best see the real colour, if any, of the superficial strata, and what do we find? Wherever we turn, from pole to pole, there is an entire absence of colour; they are white and at times as brilliant as "new fallen snow." If we scan the vast cliffs of the "Apennines," say at sunset, for hundreds of miles, rising to 8000 or 10,000 feet, with peaks up to 20,000, they are white, seen in sunshine.

If we examine the cliffs of the Sinus Iridium highlands, the huge array round Mare Crisium, or indeed anywhere else, it is the same, and without a doubt it demonstrates to us that the outer strata on the moon are of *the same white material all over the globe.* Precisely where degradation is most certain, and where the true colour of the strata would be distinctly visible, there we find the most extraordinary and invariable whiteness for a thickness of at least two or three miles.

A remarkable feature of the case is that, as a rule, all cliffs are much whiter than the general surface around them. In the raised ramparts of craters and walled plains, it is well known that the outer, and more gradual slopes, are invariably darker than the steep inner cliffs facing the enclosure. In Aristarchus, Theophilus, and such like rings, at sunrise, this is very conspicuous, especially in photographs, and it is not easy to account for this peculiar feature (evidently the result of disintegration and removal of the surface by gravitation) except by the supposition that the outer surface all over (and excepting rays and nimbi) is snow stained by meteoric dust. "It is well known that the fall of meteoric dust on our earth is very considerable, and estimated by Dr. Kleiberg, of St. Petersburg, at about 11,435 tons per annum. It has been found on all our ocean bottoms, and on our polar snows, where it is soon overlaid or removed by winds. On the moon, however (where there is no wind and now no snowfall), it could accumulate for many thousands of years, at least on levels, and so stain them very perceptibly."

Undoubtedly we see the true colour of the surface layers at the cliff faces, but unless the outer surface were stained in some way their bright *contrast* would be impossible.

Hence I take it that the outer layer of the surface all over, for at least one or two miles in thickness, is formed of snow, stained outside by a deposit of meteoric dust, the accumulation of many thousands of years, the removal of which, by gravitation, at cliffs causes their brightness, and this would explain the perennial enigma of where all the water has gone.

At low temperature neither ice nor snow vaporise, even *in vacuo*, and also that at low temperature ice is a non-viscous solid (like glass) has been experimentally demonstrated by Mr. T. Andrews, F.R.S., and the results laid before the Royal Society (see NATURE, vol. xlii. p. 214). The prevailing whiteness, therefore, of the lunar cliffs and steep inclines would seem to be a powerful argument against a "volcanic" surfacing to our satellite, and a good one in favour of glaciation.

The question of maximum surface temperature under fourteen days' solar heat has undergone a startling change since Lord Rosse's classical experiment. The possibility of snow existing on the moon is now admitted by leading astronomers, since the researches of Profs. S. P. Langley and F. W. Very, of the Allegheny Observatory, have demonstrated that the maximum may be so low that the *mean* temperature may possibly be below -100°C.

The old volcanic "selenology" is dying; there is no hope of any more *progress* in it (and that is the great sign of life in all branches of science nowadays); it is fossilised. That a "new selenology" is badly wanted is pretty obvious to all who look into the question. The surfacing of our own satellite, one of the most conspicuous and easily seen objects in the heavens, is still the standing enigma. S. E. PEAL.

Sibsagar, Asam, February 8.

THE CROONIAN LECTURE.

MUCH interest has been excited not only among men of science but among the general public by Prof. Virchow's visit to England. From the moment when it was announced that he had agreed to deliver the Croonian Lecture, it was universally felt that it would not do to lose so good an opportunity of doing honour to an illustrious investigator. Prof. Virchow is known, of course, chiefly as a pathologist. He is the founder of the science of pathology in the sense in which it is now everywhere understood and taught; and it would be difficult to form too high an estimate of the value of this part of his labours. But Prof. Virchow is one of those men of genius who never find in any one department of research a sufficient outlet for their energies. In archæology, anthropology, and ethnology he has been for many years one of the foremost workers of the age, and he has brilliantly represented science in the political life of Prussia and the German Empire and in the municipal life of Berlin. As a teacher in the Berlin University, of which he is now Rector Magnificus, he has done much to foster a genuinely scientific spirit among the pupils who have flocked to his class-room; and as a writer he has command of so pure and attractive a style that he has been able to exercise a wholesome and stimulating influence on the intellectual life even of classes to whom science does not usually make a very strong appeal. Altogether, Prof. Virchow's career is one of which Germany has good reason to be proud. In him she possesses one of those rare and potent thinkers who touch no subject without giving it fresh significance, and who have the secret of awakening in other minds something of their own enthusiasm, independence, and vigour.

There was so great a demand for tickets that arrangements had to be made for the delivery of the Croonian Lecture in the theatre of the University of London; and here—on Thursday, March 16—a crowded audience listened with the deepest interest to what Prof. Virchow had to say about the great subject in the development of which his researches have marked so splendid an era. In the evening a public dinner was given in his honour at the Hôtel Métropole. Lord Kelvin presided, while the Presidents of the Royal Colleges of Physicians and Surgeons acted as vice-chairmen. In proposing the toast of the evening, Lord Kelvin said he was one of those who had listened with rapt attention that day to the lecture delivered by Prof. Virchow. The mystery he dealt with remained a mystery, but they were conscious of no feeling of disappointment. Though it was not for any man to tell them what life was, they had been brought nearer than ever to the solution of that fascinating problem by the researches of Prof. Virchow. Mr. Huxley, Sir James Paget, and Sir Andrew Clark also spoke of Prof. Virchow's magnificent discoveries. Prof. Virchow, in responding to the toast, expressed the pleasure he felt in being welcomed by "so large and so illustrious an assembly of the learned men of England." "Abroad," he said, "he had never seen anything like it." English men of science do not often indulge in demonstrations of this kind, and it is satisfactory to know that when they do try to show what they think of a great investigator their achievement does not fall short of anything done with a like intention in Paris or Berlin.

This week Prof. Virchow has been in Cambridge, where he has been received with as much enthusiasm as in London. On Tuesday the University marked its sense of the importance of his labours by conferring on him the honorary degree of Doctor in Science. The following is the speech delivered by the Public Orator, Dr. Sandys, in presenting Prof. Virchow for the degree:—

Dignissime domine, domine Procancellarie, et tota Academia:—

Universitatis Berolinensis Rector Magnificus, vir non modo de medicina et salute publica, sed etiam de anthropologia, de ethnologia, de archaeologia praeclare meritus, vir et sexagesimo et septuagesimo exacto aetatis anno honoribus amplissimis cumulatus, satis magnum hodie praebet dicendi argumentum. Ipse laude nostra maior, laudes tamen suas (qua est modestia) invitus audiet; atqui laudes illas non verba nostra qualiacumque, sed ipsius opera insignia, ipsius discipuli illustres, ipsius denique orationes disertissimae, etiam ipso invito, satis clare loquuntur.

Legistis fortasse orationem illam in qua, Rectoris munus nuper auspicatus, studiorum Academicorum orbe universo lustrato, partium liberalium dux et signifer olim insignis dixit veram Academiae libertatem esse libertatem docendi, libertatem discendi; ostenditque, qua potissimum mentis disciplina iuventus Academica discendi amore vere liberali imbui posse videretur. Legistis certe, fortasse etiam audivistis, orationem alteram in qua nuperrime inter scientias biologicas locum pathologiae proprium vindicavit, et, studiorum suorum origines repetens, non modo HARVEII nostri merita immortalia denuo commemoravit, sed etiam GLISSONII nostri gloriam prope intermortuam ab integro renovavit. HARVEII quidem in doctrina, *omne vivum ex ovo* nasci, lacunam magnam relictam esse constat; laetamur lacunam tantam ab eo magna ex parte esse expletam qui primus omnium re vera probavit *omnem cellulam e cellula* generari.

Ergo rerum naturae investigator tantus, tot illustrium praesertim medicorum in Academia, titulo nostro honorifico iure optimo decoratur. Etenim ubicumque florent medicinae studia cum rerum naturae observatione exquisita feliciter coniuncta, talium virorum nomina in honore maximo non immerito habentur. Talium certe virorum per labores verba illa vetera vera facta sunt, quae Salutis in templo supra portam inscripta esse debent:—*sine rerum naturae cognitione trunca et debilis est medicina.*

Duco ad vos Regiae Societatis Londinensis unum e sociis extraneis, virum gentis Teutonicae inter decora numeratum, RUDOLFUM VIRCHOW.

The following is Prof. Virchow's Croonian Lecture:—

It is now nearly ten years since this illustrious Society conferred on me the unexpected honour of electing me one of its Foreign Members. Not this only, but last autumn it held me worthy of a further honour, in awarding me the Copley medal —a sign of the highest recognition of my work, the significance of which far exceeds the distinctions which the changing favour of political powers is accustomed to bestow. Nevertheless, deeply as I appreciated this mark of its constant and increasing esteem, still I was not in a position to offer my thanks personally to the society. Numerous duties, official and private, the weight of which has increased with each year, compelled me to continuous work at home, and even during the vacations the freedom of my movements has been for some time past restricted by international engagements, which yearly become more numerous and more pressing.

With great indulgence, which I fully know how to appreciate, the Council ha allowed me to postpone the date of my appearance in your midst. Hence, you see me only to-day among you, and I may tell you in person how very grateful I am to this Society, and how great an incentive to new efforts your recognition has become to me.

Who of us is not in need of friendly encouragement in the changing events of life? True! happiness is not based on the appreciation of others, but on the consciousness of one's own honest labour. How otherwise should we hold our ground in the midst of the turmoil of the day? How should we preserve the hope of progress and of final victory against the attacks of opponents and the insults which are spared to no one who comes before the public? He who during a long and busy life is exposed to public opinion, certainly learns to bear unjust criticism with equanimity, but this comes only through the confidence that our cause is the best, and that some day it must triumph. Such is our hope in our wrestlings for progress in science and art. Such is our hope in our struggles for civil and religious liberty, and in this hope we gradually become hardened against malicious attacks. It is a kind of immunisation which, I acknowledge, has also great drawbacks, for this hardening against unjust attacks leads very easily to a similar indifference towards just attacks, and, owing to the tendency to contradiction rooted in the nature of human thought, it finally leads also to indifference to praise and recognition. One withdraws again and again into oneself discontented with the world and with oneself also; but who can so completely retire within himself that the consciousness of the insufficiency of human thought, and that the criticisms of opponents are justified, cannot break through the crust of even the most hardened self-consciousness? Happy is he who has courage enough to keep up or regain his connections with other men, and to take part in the common work! Thrice happy he who does not lack in this work the flattering commendation of esteemed colleagues!

Such were the thoughts which filled my mind, as, looking forward to the present occasion, I reviewed my own life and the history of science, or, to use another expression, the fortunes of our predecessors. How often have I found myself in a state of despondency, with a feeling of depression! And the history of science—what long periods of stagnation and numerous interruptions has it not experienced owing to the victory of erroneous doctrines! What has saved me is the habit of work, which has not forsaken me even in the days of outward misfortune—that habit of scientific work which has always appeared to me as a recreation, even after wearying and useless efforts in political, social, and religious matters.

That which has saved science is identically the same; it only appears to be different, because the co-operation of many is necessary to secure the advance of science; hence, the exalting and consoling thought that one nation after the other comes to the front to take its share in the work. When the star of science becomes dim in one nation, it rises sooner or later to yet brighter glory in another, and thus one nation after another becomes the teacher of the world.

No science, more often than medicine, has gone through these waxings and wanings of brilliancy; for medicine alone, of all the sciences, has, for more than 2000 years, found ever new homes in the course of a progress which, though often disturbed, has never been wholly arrested.

It would lead us too far to illustrate this with examples drawn from the entire past. It is enough for my present purpose to take the outlines of modern medicine as the object of our consideration. Such a sketch, cursory as it must be, ought at the same time to throw some light on the intellectual relations of both nations, English and German, for these have taken a prominent part in establishing the principles of modern medicine.

The downfall of the old medicine, the so-called humoral pathology was brought about in the beginning of the 16th century. We, in Germany, are inclined to attribute to our nation a decisive *rôle* in this memorable struggle.

It was a man of our race, Andreas Vesalius, or from Wesel, who transformed anatomy into an exact science, and who thus, at one stroke, created for medicine a solid foundation, which it has retained ever since and, let us hope, will never again lose.

But the principal blow to the old medicine was struck by his somewhat elder contemporary Paracelsus, that charlatan, yet gifted physician, who removed from among the beliefs of mankind the doctrine of the four humores, which, *quasi*-chemical in its construction, formed the basis of the old pathology. Strangely enough he accomplished this with weapons borrowed from the armoury of the Arabs, the successors of the Greeks, and the chief representatives of the mediæval humoral pathology. From them, also, he borrowed alchemy, and, at the same time, the fantastic spiritualism of the East, which found a clear expression in his doctrine of the "Archæus," as the determining force in all living beings.

In this way, the new medicine, at its very birth, absorbed the germs of that ruinous contradiction, which, even up to this present century, has kept up the embittered strife of the schools.

To Vesalius is due the exact tendency, which starts from the observation of actual conditions, and which, without going further, we may call the anatomical.

Paracelsus, who pronounced the anatomy of the dead body to be useless, and sought for the basis of life as the highest goal of knowledge, demanded "contemplation" before all else; and, just as he himself arrived in this way at the metaphysical construction of the archæi, so he unchained among his followers a wild and absolutely fruitless mysticism.

Nevertheless there lay hidden in that "contemplation" of his a healthy kernel, which would not allow the intellectual activity which it had stirred up to come to rest. It was the idea of *life* which formed the ultimate problem for all future research. Strangely enough, this idea, which always existed in the popular mind, and which is in an unmistakable form present even amongst primitive nations, had been driven far into the background in scholastic medicine. Ever since the time of Hippocrates it had been the custom to use, instead of life, the obscure expression φύσις, *natura;* but in vain does one seek for a more exact definition of the term. To Paracelsus nature was living, and the basis of his life was that very "archæus," a force differing from matter, and separable from it, or, as he himself expressed it, in the sense of the Arabs, a spirit, "spiritus." In the compound organism of man, the mikrokosmos, each part, according to him, had its own "archæus," but the whole was ruled by the "archæus maximus," the "spiritus rector." From this premiss has proceeded the long succession of vitalistic schools, which, in ever-changing forms, and with ever new nomenclature, introduced into the notions of physicians this idea of a fundamental principle of life.

If the sagacious Georg Ernest Stahl, whose services to the development of chemistry are now acknowledged everywhere, substituted the soul for the "spiritus rector," and so created a system of animism, the last vestiges of which have disappeared from the school of Montpellier within our own time only, so also in turn did the pure vitalists build up on the dogma of specific dynamic energies, maintained so stoutly by the physicists, that notion of the vital force, the half spiritualistic and half physical character of which has contributed so much, even in our day, to puzzle and mislead men's minds.

The doctrine of the vital force found its strongest support in the "Natur-philosophie," especially in that which, on German ground, soon obtained universal sovereignty.

This summary exposition of mine has greatly anticipated the historical progress of the evolution of medicine. It is now time to pay proper homage to the great investigator who made the more exact method the ruling one, and at the same time to award to this country, which brought him forth, its important share in determining the new direction of our science.

Nearly 100 years had passed since Vesalius and Paracelsus had begun their work when William Harvey published his "Exercitatio anatomica de motu cordis et sanguinis in animalibus." Here, for the first time, the anatomical examination of living parts was carried through, in an exemplary way, according to experimental methods. All the objections that anatomy concerned itself with dead parts only were thus at once set aside; living action became the object of immediate observation, and this was done on one of the most important organs, one absolutely necessary to life, the varying activity of which constantly calls for the attention of the practical physician. Not only so, however, but a new mode of observation—the experimental method—was thus brought into use for research; a method through which a new branch of medical science, physiology, has been laboriously built up.

The influence of this one wonderful discovery of Harvey's on the ideas of men of his time, and of his successors, was memorable.

Among the men of his time the last support of Galenism disappeared with the proof of circulation; among his successors the comprehension of the causation of local processes dawned for the first time. Very ancient and highly difficult problems, such as inflammation, could now be attacked; a goodly piece of life also became intelligible, since one of the vital organs themselves could now be subjected to experiment, and, to the astonishment of all, the action of this organ showed itself to be an absolutely mechanical one. The revulsion of thought was so complete that it has become since a difficulty hardly to be overcome to enter even in imagination into the ideas of the older physicians, to whom the circulation of the blood was unknown.

Nevertheless, in spite of such striking results, the craving of man for more complete understanding remained unsatisfied. One saw the action of the living heart, but how did it live? What was this life, the action of which one saw before one? In the heart itself the essence of life could not be recognised.

Harvey turned his attention to another object; he tried to observe the very beginnings of life in the incubated egg of the fowl and in the embryos of mammalian animals. He thereby soon arrived at the question of the significance of the egg in general, and enunciated the celebrated dictum, "Omne vivum ex ovo." Owing to the more extensive researches of modern investigators, this dictum, as is well known, proved too narrow for the whole animal kingdom, and is no longer exact when applied to plant life. Its validity for the higher animals, on the other hand, cannot be questioned, and it has formed one of the firm standpoints on which researches on sexuality and on the propagation of life have been based. But Harvey, on account of the deficiency of his optical instruments, was unable to see that which he was labouring to discover, namely, the process of organisation as such, just as he had been unable in former times to see the continuity of the capillary flow. This imperfection lasted for a long time afterwards; and thus it happened that even Albrecht von Haller and John Hunter considered the formation of the area vasculosa in the incubated egg of the fowl as the commencement of organisation, and indeed, as the type of organisation itself.

I will return to this point later on; but for the present I should like first to draw your attention to a man whose importance for the further development of the doctrine of life has always appeared to me to have been uncommonly great and highly significant, but who, nevertheless, has sunk into unmerited oblivion, not only among posterity in general, but also, I think I may be allowed to say, even in the memory of his countrymen. I mean Francis Glisson, who was a contemporary of Harvey, and whose works appeared almost simultaneously with those of his more celebrated colleague; but the brilliancy of Harvey's discoveries was so great that the light which shone from Glisson's work-table almost disappeared. I rejoice that on so auspicious an occasion I may recall the memory of the modest investigator, and may offer him the tribute of gratitude which science has to award him.

When, thirty-five years ago, I published my little essay on "Irritation and Irritability" (*Archiv für Pathologische Anatomie und Physiologie*, 1858, vol. xiv. p. 1), I did not know much more about Glisson than what every student of medicine learns, namely, that there is in the liver a "capsula communis Glissonii," and what was even less known, that this anatomist had written a small work on "Rachitis," which, indeed, was the first of its kind. In my own paper on this disease (*ibid.* 1853, vol. v. p. 410) I had tried to demonstrate the circumspection and accuracy which are noticeable in this book, and which make it a typical model for all collective investigations; but even at that time I overlooked the fact that this was only the smallest merit of this wonderful man. It was only in the further course of my studies on the history of the doctrine of irritation and irritability that I made the discovery, an astonishing one to me, that the idea of irritability did not, as is generally thought, originate with Haller, but that the father of modern physiology, and the Leyden school in which he had been brought up, had borrowed this idea from Glisson. I then stumbled on a series of almost forgotten publications of this original scholar, especially his "Tractatus de natura substantiæ energeticæ seu de vita naturæ ejusque tribus primis facultatibus, perceptiva, appetitiva et motiva," which appeared in London in 1672, and in which the ideas were further worked out, the outlines of which had already been brought forward in his "Anatomia hepatis," published in 1654. In this work (p. 400) the newly-coined word "irritabilitas" appears, so far as I can find out, for the first time in literature. It may be noticed, by the way, that the expression "irritatio" is much older. I find it already in Celsus, but with an exclusively pathological signification. It appears, also, occasionally in later writers, and to this day it has not, speaking accurately, lost this original signification. It is otherwise with Glisson; to him, irritability is a physiological property, and irritation merely a process of life dependent on the natural faculties of living matter.

Thus he was led, through a process of "contemplation," to maintain the existence of the "biarchia," the "principium vitæ," or the "biusia," the "vita substantialis vel vitæ substantia." And in order to allow of no misunderstanding as to the scource of his "contemplation," he adds distinctly that this is the "archæus," of Van Helmont—the "vis plastica" of plants and animals.

In the further course of his philosophical discussions, he nevertheless is led into the same by-path, which has misled, even in the most recent times, so many learned men and even excellent observers. This is the by-path of unlimited generalisation. The human mind is only too prone to render intelligible what is unintelligible in particular phenomena, by generalising them. Just as even in recent times an attempt has been made to render consciousness intelligible by representing it merely as a general property of matter; so Glisson thought he might attribute to the active principle ("principium energeticum") which according to him is contained in all matter the three faculties of living matter which he considered as fundamental, namely, the facultas perceptiva, appetitiva et motiva. All matter was sensitive, was thus stimulated to develop impulses, and moved itself as a consequence of these impulses.

It is not necessary for the purpose of our present inquiry to carry these quotations further, since they are quite, in the Paracelsian sense, contemplative in their nature; and especially as, in their generalisation, they do not appear to be important for the history of advancing knowledge.

That which is full of significance for us is concerned with actual life only, in the narrower sense of analytic science. It was not the "principium energeticum" set up by Glisson, which stimulated his successors again to take np the thread of his observations, but rather this process of irritation described by him, and the fundamental faculties of living matter on which it depended. In this way he has really led up to a more exact study of the actions of life and the properties of living matter.

Unfortunately, there intervened a mistaken conception, which led his followers again into a series of most serious errors. Glisson, following on this point also the example of Van Helmont, was convinced that nerves contracted when irritated. He joined to this the idea that, through the contraction of the nerves, or even of the brain, the fluid contained in them was propelled towards the periphery.

This notion, shared by Willis and many other physicians of that time, furnishes the reason why irritability was identified with contractility. Even the great master Hermann Boerhaave, and after him his pupil Gaubius, the first special writer on general pathology, considered sensation and motion as common properties of, at all events, all the solid parts of the body. The former thought it proved that hardly a single particle of the body existed which was not sensitive and did not move; and thus it becomes comprehensible how Haller himself carried this idea that irritability had the same significance as contractility from his school days in Leyden to his professorship in Göttingen. It was in this sense that he understood the irritability of the muscles, and in the same sense he denied this property to the nerves.

This dispute about the irritability of muscles has continued far into the present century; its long duration becomes intelligible only when we bear in mind that, without the most exact knowledge of its historical development, even the very statement of the question is liable to be misunderstood.

As a matter of fact, so far as we know, the nerves are not contractile, like the muscles; on the other hand, the muscles are not only contractile, but are also irritable. Irritability and contractility are not identical, even when they occur in the same part. The nerve current, on the other hand, cannot be compared with the blood stream; it does not consist in the movement of a fluid, but is of electrical nature, and hence there is no need for its production of a contraction of the nerve-tubes.

It was also an erroneous conclusion that every irritated part contracted. Instead of contraction, secretion, or, under certain circumstances, a more vigorous nutrition, may occur as the final result of irritation. Hence we use a more comprehensive term in order to express this final result, and call all forms of it "actions." While Glisson defined all "actio propria sic dicta" as "motus activus," we distinguish different kinds according to the nature of the effects, or, expressed otherwise, according to the direction of the activity (nutrition, formation, and function): but we agree with the above thinker in the opinion that no vital energy is ever set free without stimulus: that, therefore, every action is of an irritative nature. In this irritation, according to my idea, consists the "principium dividendi," according to which we must distinguish between active and passive processes of life, and in this way we gain also a basis for the fundamental division of pathological elementary processes. How much work has been necessary in order to render this conception possible! And how great, even now, is the number of our colleagues who have not fully accepted it! The reason for this difficulty is twofold.

Most of the vital actions of life, whenever they manifest themselves by visible events, are of a compound nature. As a rule very various, at times wholly unlike parts, each with its specific energy, combine to produce them. Not unfrequently it thereby happens that in the visible sum of final effects one part behaves in an active the other in a passive manner. It is only the most minute analysis of the phenomenon, tracing it right back to the elementary parts, which allows the total result to be resolved into its components; such an analysis cannot, for the most part, be expressed in current language, except at great length. No language in the world is rich enough to possess special expressions for each such combination. Only too often we help ourselves out of the difficulty by regarding the compound phenomenon as a simple one, and by expressing its character according to some chief trait, which stands out in a commanding manner from the general picture. This is the practical difficulty.

With it, however, a theoretical difficulty is very often combined. The human mind, owing to a natural impulse, seeks in the phenomena indications of their determining cause. The more complex the phenomenon the more busy is the imagination, in order to convert it into a simple one, and to find a unitarian cause for it. So has it happened in respect to life, so in respect to disease. The course of thought followed by Glisson is opposed to such an explanation. He had no scruple in dividing the unit of life into a large number of individual lives. Although the knowledge we now possess of the arrangements of the body was absolutely foreign to him, yet he arrived quite logically at the "vita propria," the proper elementary life, of the several parts. To be sure, this expression, as far as I can see, is not to be found in his works, and occurs first in those of Gaubius; but Glisson says distinctly ("Anatomia hepatis," "Ad lectorem," N. 17): "Quod vivit per se vivit vitam a nulla creatura præter se ipsum dependentem. Hoc enim verba vivere per se sonant."

The unitarian efforts of the following period relentlessly passed over the tendency of which I have just spoken. Some returned to the old Mosaic dictum, "the life of the body is in his blood"; others gave the nervous system, and the brain especially, the first place in their consideration. Thus once more was renewed the old struggle, which for thousands of years had divided the schools of medicine into humoral and solidar pathology. Even when we ourselves entered on scientific work, hæmato-pathologists stood in hostile attitude to neuro-pathologists.

In England, humoral pathology found a strong support in the great and legitimate authority of John Hunter. Although this distinguished practitioner never shared the one-sidedness of the later pathologists, but rather attributed to the solid parts the living principle the existence of which he assumed, yet, in his investigations, the blood took precedence over all other parts as the chief vehicle of life.

One must, however, recall to mind that Hunter laid special stress on the fact that life and organisation are not bound to each other, since animal substances which are not organised can possess life. He started, as has already been noticed, from the erroneous conception that eggs are not organised, and that it was not till after incubation that the first act of organisation, namely, the formation of vessels, took place. He considered his "diffuse matter" ("materia vitæ diffusa") as the actual carrier of life; and this was to be met with not only in the solid parts, but in the blood also. This matter, according to him, existed in the brain in a remarkable degree of concentration, but its presence was quite independent of all nervous structures, as is shown by the example of the lower animals which possess no nerves. In the posthumous writings of Hunter, which Owen has collected, the very striking expression "simple life" is met with, a state most clearly to be recognised in plants and the lowest animals. This simple life was in Hunter's view the ultimate source of all living actions, pathological as well as physiological.

Hunter was out and out a vitalist, but his materialistic vitalism, so to speak, differed *toto cælo* from the dynamic vitalism of the German schools. If living matter existed independently of all organisation, such living matter was beyond the scope of anatomical investigation; but, on the other hand, if it were present in non-organised parts, such as an egg, it was in itself the ultimate source of the organisation which subsequently makes its appearance in these parts. It must, therefore, to adopt a later mode of expression, be of a plastic nature. Here Hunter's notion fell in with that of the plastic lymph, as developed by Hewson; and it was only logical that Schultzenstein applied it to the blood at last, and designated as "plasma" the material of life present in the blood. In this way the formative and nutritive matter necessary to physiological life as well as the plastic exudations occurring in diseased conditions could be attributed to the same material—a highly satisfactory result in appearance, and one providing a most convenient basis for interpretations. The exponents of this notion had no scruples in going one step further, and in providing this material of life with a technical name. They called it "fibrin." Evidently this did not quite correspond with Hunter's ideas, for we know of no such matter, either in the egg or in the plants or the lower animals, as that to which he attributed simple life; but the necessities of pathology overcame all such scruples, and the plastic exudations were received as undoubted evidence that fibrin possessed the power of becoming organised. They formed, in the *crasis* doctrine of the Vienna school, the bright spot of this newest kind of hæmato-pathology.

Wherever fibrin failed, blastemata were brought to the fore. Ever since Schwann had given the name of cytoblastema to the organising material of the egg, the way had been open for assuming in other places the existence of material with this ambiguous name.

But of course through these steps the one simple matter of life predicated by Hunter was replaced by many "matters of life," and thus the entire advantage gained by the exposition of a unitary theory of life was at once lost.

Even when, finally, the cell-contents were designated as protoplasm, and thus the one requisite of Hunter, namely, that the material of life must also be contained in the individual parts, appeared to be fulfilled, yet no single specific material was thereby arrived at. No one dreamed of regarding protoplasm as fibrin, and least of all did any one consider it a simple chemical body.

By the conception of the blastema, however, there had been reawakened a thought which had occupied the minds of man from the earliest times. If a plastic matter capable of being organised really existed in the body, then the organisation of the same must present the first reliable example of epigenesis. The problem of the "generatio æquivoca," which had been fought over for so long a time, now appeared to be solved. What Harvey had taught concerning the continuous descent from the egg became temporarily obliterated, when the theory of descent through exudation made its appearance. Several generations of young medical men have been educated in this belief. I myself remember my "epigenetic" youth, with no little regret, and I have had hard work to force my way through to the recognition of the sober truth.

Meanwhile, the attention of other bodies of inquirers had been directed to the tissues of the body. Among these, in view of their importance, the nervous tissues, and especially the mass of nervous tissues in the brain and spinal cord, rank highest.

Hunter also had acknowledged the importance of the brain, and hence called it the "materia vitæ coacervata." It was easily seen that it contained no fibrin, but experimental research showed also that neither the brain nor the spinal cord was of the same value throughout all its parts. The more accurate the experiments the smaller became the region which, in the strictest sense, is the vital part, until Flourens limited it to one single spot, the knot of life ("nœud vital"). Was the unity of life found in this way? By no means. The brain is no more and no less vital than the heart; for life is present in the egg long before the brain and heart are formed, and all plants, together with an immense number of animals, possess neither the one nor the other. In the highly compound organism of man, the brain and spinal cord have a certain determining action on other parts necessary to life. Their disturbance may immediately be followed by the disturbance of other vital organs, and sudden death may ensue.

But the collective death of a compound animal no more implies the immediate local death of all its special parts than the local death of some of the latter is incompatible with the continued collective life of the animal. As has been well said, at the death of a compound organism there is a "primum moriens," one part which first ceases to live; then follow, at long intervals sometimes, the other organs, one after the other, up to the "ultimum moriens." Hours and days may pass between the total death of the individual and the local death of the parts. The fewer nerves a part contains the more slowly usually does it die; I therefore consider the process of dying in the compound organism as the best illustration of the individual life of the several constituent parts, which is in its turn the first axioma necessary for the study and for the understanding of life.

A long time, however, elapsed before it was possible to return to this starting point, and to obtain a considerable number of supporters for the doctrine of the "vita propria." The attention of many observers was drawn to a totally different side of the question. In the last decade of the past century, about the same time that John Hunter, starting from careful anatomical investigations and exact observations of surgical practice, worked out his idea of the material of life, a new system of medicine was founded in Scotland, the so-called Brownian system, which was based on quite different premisses. Brown also was a vitalist; he, too, constructed, not merely a pathological and therapeutic system of vitalism, but a physiological one, though this doctrine was dynamic in its character. There is but little to be noticed therein of the material anatomical foundation of exact medicine. It is concerned principally with contemplations of the forces of the living organism. One can understand to some extent how this happened, if one keeps in view the histo.y of the development of this extraordinary personality; I cannot go into this here, but anyhow the remarkable fact remains that the two contemporaries, Brown and Hunter, worked near each other without its appearing from their writings that they were acquainted with one another. Brown struck out his own line, and stuck to it, without troubling himself about the rest of the medical world. And yet even his first work "Elementa Medicinæ," had the effect of an earthquake; the whole European continent was shaken by it, and even the physicians of the recently opened New World bent under the yoke of revolutionary ideas; and in a few years the aspect of the whole field of medicine was entirely changed. True! the triumph was but short; the Brownian system disappeared as it had come, a meteor in the starry heaven of science. There would be no reason to go into it more fully, had not the impulse which he had given instigated other men, and be permanently applied by them to the true service of science. This impulse was founded on the fact that irritability, or, as Brown called it, "incitability," was thus reinstated as the starting point of the theory; but, along with this, the stimuli which set living substances in action, the "potestates incitantes," were brought to the fore. In so far that stimuli produce a state of irritation ("incitatio"), or, as Brown called it later, excitement, they came to be viewed not only as the cause of health and disease, but even of life itself; for excitement, so he said, is the true cause of life. But, as excitement stands in a certain relation to the strength of the stimulus, a state of good health was only possible with a normal degree of stimulus, whilst an excess or a lack of stimulus brought diseased conditions in its wake. Of course excitement is dependent also on irritability, with a certain quantity of which, in the form of energy, every living being is endowed at the beginning of its life.

The division of diseases, according to the amount of vital force visible in them, into sthenic and asthenic, has never been abandoned since, though acknowledged perhaps in a less precise manner; it has sometimes been brought more prominently forward, and sometimes thrown into the background. In Germany, Schönlein was the one of all others who took this doctrine as the foundation of his opinion on special cases of disease, and for his choice of treatment.

But the application of the Brownian principles to physiology has been of far greater importance. If life itself were dependent on external stimuli, the notion of the spontaneity of vital actions, a notion still in force, must lose all significance. Certain stimuli would in that case prove to be necessary conditions of vital activity, without which life could at best be carried on in a latent form only. Certainly even for this latent life the question remained open: How does it come to pass, and in what does it practically consist? Brown avoided this ticklish question, not without great skill, by drawing the whole attention to active life and to the stimuli which call forth action. To speak openly, science has since then deflected little, or not at all, from this guiding notion. Even now, we cannot say what latent life is. We simply know that through external stimuli it may be converted into active life, and hence irritability is considered by us as the surest sign of life, not of course of the general life of all matter in the sense of Glisson, but of the real and individual life of special living organisms. Brown remarked, with reason, that through irritability the living substance is differentiated from the same substance in its dead condition, or from any other lifeless matter. Nevertheless, neither irritability nor incitability, neither irritation nor incitation, explains the essence of the living substance, and therefore neither explains the essence of life.

In Germany the physiologists especially took up this question. Among the first was Alexander von Humboldt, who in his various writings, especially in his celebrated treatise on the irritated muscle and nerve fibre, entered into the question. In the end he held fast to the assumption of a vital force. The majority of pathologists and physicians followed in his footsteps, and long and fierce controversies were necessary before, nearly half a century later, the belief in a vital force was destroyed. When du Bois-Reymond had demonstrated the electrical current in muscle and nerve in all its characters, and, at the end of his work, had also disclosed the inadmissibility of vital force, then the venerable Humboldt formally and expressly renounced the dream of his youth, with the masterly submission of the true naturalist to the recognised natural law.

The hypotheses of a particular force of life had, however, in regard to Brown's theory neither a positive nor a negative value. Johannes Müller rescued for general physiology, in which it has ever since kept its place, that which was valuable in Brown's system, the doctrine of the integrating life stimuli. The occasional stimuli which produce disease have found their place in etiology; their significance has become more and more sharply defined, the more accurately we have learnt to distinguish between the causes and the essences of disease, a distinction which became more difficult as the "causæ vivæ" of diseases became known in ever-increasing numbers. And now a new task has arisen, namely, to draw into our sphere of observation the life of the causative agents themselves.

The way in which pathology has tried to approach the desired goal, to fathom the living substance in its diseased conditions, has led us a great step forward. Pathological anatomy, especially, has opened this road. The more numerous its observations, and the more it penetrated into the details of the lesions, the smaller became the field of so-called general diseases. The first steps of mediæval anatomists had the effect of drawing the attention to local diseases. In the first and longest period, which one may define as that of Regionism, the pathological anatomists sought the cause of disease in one of the larger regions or cavities of the body—in the head, chest, or abdomen. In the second period, ushered in by the immortal work of Morgagni, shortly before the time of which I last spoke—the time of Brown and Hunter—they endeavoured to find in a certain region the actual organ which might be considered as the seat of disease. On this foundation arose the Parisian school of Organicism, which, until late in this century, held a dominant position in pathology. In this school, already, they recognised that not the organ, nor even a portion of it, could be the ultimate object of research. Xavier Bichat divided the organs into tissues, and showed that in the same organ sometimes one and sometimes another tissue might be the seat of disease.

From that time forward the eye of the pathological anatomist was directed chiefly to the changes in the tissues, but it soon became apparent that even the tissues are not simple substances. Since the third decade of this century, the microscope has disclosed the existence of cells, first in plants, and very soon afterwards in animals. Only living beings contain cells, and vegetable and animal cells show so much similarity of structure that one can demonstrate in them the actual product of organisation. This conviction has become general, since through our embryologists, especially through Schwann, proof has been afforded that the construction of embryonic tissues was derived from cells also in the highest animals and in man himself.

In the fourth decade of this century the science of pathological anatomy had already begun to be directed towards cells. These researches very soon struck on great difficulties. Many tissues, even in their developed state, appeared to contain neither cells nor their equivalents; nevertheless, I have been able to demonstrate their existence in those tissues in which their presence appeared to be most doubtful, viz. in bone and connective tissues. At the present time we are so far advanced as to be able to say that every living tissue contains cellular elements. We go a step further even, for we require that no tissue should be called living in which the constant occurrence of cells cannot be shown.

A still greater difficulty then appeared, namely, to discover in what way new cells originated. The answer to this question had been very heavily prejudiced by the so-called cell-theory of Schwann. Inasmuch as this very trustworthy investigator asserted that new cells originated from unformed matter, from "cyto-blastema," there was opened up a wide road to the old doctrine of the "generatio æquivoca," which afforded all partisans of plastic materials an easy way of reviving their dogma. The discovery of cells of connective and allied tissues gave me the first possibility of finding a cellular matrix for many new growths. One observation followed another, and I was soon in a position to give utterance to the dictum, "Omnis cellula a cellula."

And so at last the great gap was closed which Harvey's ovistic theory had left in the history of new growth, or, to speak more generally, in the history of animal organisation. The begetting of a new cell from a previous cell supplements the reproduction of one individual from another, of the child from the mother. The law of the continuity of animal development is therefore identical with the law of heredity, and this I now was able to apply to the whole field of pathological new formation. I blocked for ever the last loophole of the opponents, the doctrine of specific pathological cells, by showing that even diseased life produced no cells for which types and ancestors were not forthcoming in normal life.

These are the fundamental principles of cellular pathology. In proportion as they have become more certain, and more generally recognised, they have in turn become the basis of physiological thought. The cell is not only the seat and vehicle of disease, but also the seat and carrier of individual life; in it resides the "vita propria." It possesses the property of irritability, and the changes in its substance, provided these do not destroy life, produce local disease.

Disease presupposes life; should the cell die, its disease also comes to an end. Certainly, as a consequence, the neighbouring and even far distant cells may become diseased, but as regards the cell itself the susceptibility to disease is extinguished with life.

Since the cellular constitution of plants and animals has been proved, and since cells have become recognised as the essentially living elements, the new science of biology has sprung up. It has not brought us the solution of the ultimate riddle of life, but it has provided concrete, material, anatomical objects for investigation, the structures and active and passive properties of which we can analyse. It has put an end to the wild confusion of fantastic and arbitrary notions such as I have just mentioned; it has placed in a strong light the immeasurable importance of anatomy, even in the most delicate conditions of the body; and lastly, it has made us aware of the close similarity of life in the highest and lowest organisms, and has thus afforded us invaluable means for comparative investigation.

Pathology has also its place, and one certainly not without honour, in this science of biology, for to pathology we are indebted for the knowledge that the opposition between healthy and diseased life is not to be sought in a fundamental difference of the two lives, not in an alteration of the essence, but only in an alteration of the conditions.

Pathology has been released from the anomalous and isolated position which it had occupied for thousands of years. By applying its revelations not only to diseases of man, but also to those of animals, even the smallest and lowest, and to those of plants, it in the best manner helps to strengthen biological knowledge, and to narrow still more that region of the unknown which still surrounds the intimate structure of living matter. It is no longer merely applied physiology; it has become physiology itself.

Nothing has more contributed thereto than the constant scientific union which has endured for more than 300 years between English and German investigators, and to which we to-day add yet another link. May this union never be broken!

APPLIED NATURAL HISTORY.

THE so-called experimental sciences—chemistry and physics—in their various branches, have hitherto been more extensively "applied" to the service of man, than the observational sciences of botany and zoology.

The various industries in which civilised man has naturally become engaged have induced a scientific study of the fundamental principles, and an eager search for such information as can lead, with the assistance of art, to a further advance towards the goal of perfection.

It is true, however, that the practice of medicine has much dependence on the science of botany.

Zoology, on the other hand, has never been considered as possessed of qualities serviceable to any bread-winning occupation, and although included, like botany, in all ordinary courses of medical study, has not until recently been considered of importance for the advancement of any industry.

Now, when the nineteenth century is in its last decade, we in this country are beginning to realise that a knowledge of the life-histories and habits of sea-fishes and other food-products of the deep is of paramount importance in regulating and bettering the fisheries around our coasts.

A few years ago the scientific aspects of this industry received but scant attention. Many outcries have indeed been always heard as to injurious methods of fishing, the wilful destruction of fish suitable for food, and the general depletion of certain fisheries, but in spite of Royal Commissions and Courts of Inquiry, we have been slow to grasp the truth that for want of proper knowledge with which to control our laws and regulations we have been timidly procrastinating, and allowing our chance of ready resuscitation to diminish. We have about 400,000 men dependent on our fisheries, and yet are at the present day lagging behind other and younger countries in our State Aid. In Scotland the proportion between fishermen and the rest of the population is 1 in every 76; in Ireland 1 in every 216; in England and Wales 1 in every 612. In a recent report of the Board of Trade it is also stated that "the sea fisheries of the United Kingdom appear to be of greater value than those of any other country in which fishery records are kept." The value of the fish landed annually in the United Kingdom is about six million pounds, and yet a large proportion of our fishermen eke out a miserable existence, and see the industry in which they are engaged becoming more and more unremunerative every year. In Scotland, where most is done for our fisheries, there is a Government Board where appeal can at all times be made by any persons desiring alterations in the existing state of circumstances. A Board which not only collects all statistics, but which has power and capabilities to inquire into all methods of fishing, whether from a biological or commercial standpoint, as well as to construct by-laws if necessary. In England the absence of such a body is much felt. Conference after conference is held, but although promoted under the most favourable auspices, the resolutions agreed upon can hardly be made to impress the House of Commons, because of this want of a proper channel. It would be quite out of place in an article such as the present to speculate as to the constitution of a Fishery Board for England, but without any doubt it should have not only a representative of biology, but a small staff of investigators.

The unfortunate antithesis which at present exists between so-called practical people and men of science results largely from the unknown altitude from which the latter choose somewhat exclusively to illuminate the world. Without desiring in any way to discount the pursuit of knowledge for its own sake, it seems apparent that the benefits to be derived for our fisheries are not to be obtained from the lovers of pure science, but rather from those who, having had the proper scientific training, are willing to occupy a position in which they will be intimately acquainted with the requirements of practice as their object, and yet be able to focus the theoretic rays of the specialists on the different sections of their work.

The history of the various Royal Commissions has thrown considerable light on the particular nature of the information needed. It has also shown how widely the investigations yet to be carried on must extend.

Take, for example, the old vexed question of beam-trawling in Scotland. Fishermen practising the time-honoured art of long-lineing appeared as witnesses before the Commission of 1883, and being keenly antagonistic to the trawler, described how this species of robber descended upon their old haunts, scraped and harrowed the bottom to the utter destruction of all spawn and fry, scooped up tons of fish (which should have lived to have been caught by hook and line in the proper manner), and glutted the market with what was quite unfit for human food.

It is often extremely difficult to separate political interests from fishery reports, but the fact remains that evidence of this kind, being inserted in the public press, led to much misunderstanding, and inclined people to support the line fishermen at the expense of the trawler. But the late Lord Dalhousie, as chairman of the Commission, was fortunate in having as one of his colleagues a naturalist who had for many years given special attention to fisheries. The statements, therefore, as to destruction of spawn and young fish were tried and found wanting. The evidence as to the natural history of fishes being most wild and conjectural, though given by men who had spent their lives at sea and were masters of their craft, was met by scientific accuracy and fell to the ground. We find in the official report of the Commission, published in 1886, very decided statements indicating that in the opinion of the Commissioners the injury done by the use of the beam-trawl is insignificant.

Much information has now been gained as to the eggs and embryology of sea fishes, and important observations published on such matters; for instance, as to the proportional numbers and sizes of the sexes, and the sizes at which the various food fishes become sexually mature.

Observations made on the last-named inquiry show that on different coasts where the conditions of life vary as to temperature, food, or ocean currents, the sizes at which any individual members of a species of fish spawn are distinctly different, and that the rate of growth is different. This is a matter of some importance to those who would prevent capture of fish till after some progeny has been allowed to remain. Fulton's experiments on the proportional numbers of the sexes show that out of 12,666 fish of twenty-one species examined, 3,858 were males and 8,808 were females—a ratio of 228 females to 100 males.

The flounder and the brill were, however, found to be exceptions, while the greatest inequality was found in the case of the long rough dab (*Hippoglossoides limandoides*), where the ratio was 842 females to 100 males, or nearly seventeen females to every two males. As regards the proportional size, the observations show that "Among all the flat fishes without exception, the female is longer than the male, the ratio varying with the species."

Mr. Holt, who has worked most extensively at the sexual maturation of fishes, in order to determine if possible a method of protecting fishes which have never spawned, discards the male sex altogether, and considers only the sizes of the females, since the males, being both smaller and less numerous, would be more highly protected than the females by any measures drawn up with a view to prohibiting the capture or sale of flat fishes under certain sizes. Others who have worked at the same subject pursue the same course.

These inquiries have been instituted not for their own sakes, but because, from studying the fisheries of the country, it has become obvious that knowledge of this kind is essential. The constant clamour kept up by fishermen who daily see their returns becoming smaller does not reach the ears of those who are busily occupied in commerce, or in science; it is appreciated only when special attention is paid to the history and present condition of some of the most important areas. Take the great industry of the Dogger Banks, which for other reasons has come before the notice of the public of late years. In 1828 the North Sea was practically an unknown fishing region. Boats of no very great size were in that year just beginning operations from Harwich. Before this date trawling was confined to the south coast, having commenced at Brixham about the year 1764. The Dogger Bank was found to be teeming with fish; there was plenty for every one, and an almost endless scope for fresh ventures. The "Silver Pits" were discovered in 1837, the name being significant of the value to the discoverer and his followers. So things went on, more and larger boats were built, heavier gear used, boats banded together in fleets, and remained out on the grounds for weeks at a time, steam was introduced, and the east-side of the North Sea visited. It was a "roaring trade," and many were made wealthy by it. Now things are changed, and every one cries out that the balance has been overturned, that the fish are being caught faster than the stock is being kept up: this, in spite of what was once said as to the amount of fish which could be taken from one acre of sea-bottom. It is possible to fix close times during which salmon and trout must not be taken from certain rivers, and to hatch fry which will remain in the one district. It is another matter to apply close seasons, or fix standard sizes for areas of the open sea. From what we know of life at the sea-bottom it is pretty certain that if one of the conditions necessary for keeping up a true balance of nature is removed or greatly lessened, the proportional arrangement of the remaining fauna is also interfered with, for since marine animals prey largely upon each other it follows that if one class of devourers is removed, the devoured become more numerous, which again seriously affects other classes.

For this reason an over-fished oyster or mussel bed if left to itself, or not properly regulated, will probably never regain its former condition, a fact brought out with great clearness in the course of the evidence taken before Lord Balfour of Burleigh, at the Board of Trade Conference last June. With free swimming round fish the condition is somewhat analogous, although more knowledge is required concerning their migratory movements. If the natural balance is interfered with, the result, although at first it may be only to increase certain other forms which are also of advantage to man, will eventually appear when useless or unprofitable fishes remain in the majority, or when the appearance of a once common and useful species is no longer present in the market.

If human interference can so alter the marketable productivity of the sea, and materially lessen the incomes of a large portion of a nation, surely it becomes a duty to study the application of such sciences as deal directly with the animals concerned. If by continual fishing the only available grounds became depleted, it is by a thorough study of the actual cause and effect, and the application of the principles of natural history involved, that the only true remedy is to be found.

W. L. CALDERWOOD.

THE SOUTH KENSINGTON LABORATORIES AND RAILWAY.

THE friends of science throughout the country may be congratulated upon the fact that work in the laboratories of the Royal College of Science and of the City and Guilds Institute is not to be rendered impossible by the building of a railway along Exhibition Road. Sir John Kennaway, the chairman, and the members of the House of Commons Committee deserve the best thanks of the community for their unanimous rejection of the scheme even if only partly on scientific grounds. When the evidence given before the committee comes to be published there will be some curious reading. Lord Kelvin, the President of the Royal Society, informed the committee of what was at stake, and gave his opinion as to the question both of mechanical and electrical disturbance. The paid "scientific experts" in their pleading on the side of the company promoters may be said to have almost eclipsed the usual "emphasis" of statement. We may refer to this evidence later, but in the meantime the following quotation from a leader in the *Times* indicates the general opinion as to the importance of the result which has been achieved:—

"What makes the history of this Bill novel and interesting is the second line of attack adopted by its opponents. On either side of Exhibition Road stand two of the most important scientific institutions in London. One of these—the Royal College of Science—is supported by the State; the other was founded by the City and Guilds of London for the promotion of advanced technical education. The former of these institutions, and the great collection of scientific instruments which is being formed at South Kensington, make an organised whole. This collection, which includes the earlier and the latest instruments, is invaluable both historically and practically; and is in close proximity to the lecture-halls and laboratories where use can be made of the instruments. The collection and the laboratories are used not only by many other students, but by the large number of national scholars and exhibitioners who, after the annual May examination of the Science and Art Department, are brought up from all parts of the country, chiefly at the public expense. These students, and the deserving lads who work at the City and Guilds Institute, form an important element in the situation; for to them the advent of an electrical railway was a serious peril. It was shown, and admitted, that the magnetic disturbances in the neighbourhood of the South London Railway are so great that no accurate magnetic work can be done within some hundreds of yards of it. Now the proposed Paddington and Clapham Railway would run, not some hundreds of yards from the South Kensington laboratories, but within forty feet of some of them; and there was a genuine fear on the part of the Professors that at such small distances it would be impossible not only to accurately neutralise the conflicting forces, but to prevent the astronomical instruments being affected by the earth-tremors caused by the passage of trains. This view was urged by Lord Kelvin, perhaps the greatest living authority on such matters, and by Profs. Norman Lockyer, Ayrton, Rücker, and Boys; and after a contest which lasted three days their view prevailed, and the committee found the preamble of the Bill 'Not proved.' The men of science are to be congratulated on the result. A year or more ago they successfully defended their South Kensington preserve against the invasion of Art; and it would be pitiful indeed if Science were now to be put in jeopardy by a practical application of herself. It appears that electricity cannot be studied in the neighbourhood of an electric railway; naturally, then, we cannot have an electric railway close to the great central institution where electrical science is taught at the public expense."

NOTES.

THE annual general meeting of the Institution of Naval Architects is being held this week in the rooms of the Society of Arts, which have been lent for the purpose. The proceedings began yesterday (Wednesday) morning, and will conclude to-morrow evening. The meeting is one of more than usual importance in the history of the Institution from the fact that the president, the Earl of Ravensworth, is resigning the position which he has so well filled for a period of fourteen years Lord Ravensworth is the second president the Institution has had, he having succeeded to the chair on the death of Lord Hampton, who first occupied the position. The new president is Lord Brassey, whose great interest in all maritime questions well qualifies him for the post. Lord Ravensworth will not sever his connection with the Institution, as he will accept the position of a vice-president. The following is the programme of the present meeting:—Wednesday, March 22.—Morning meeting, at twelve o'clock: Annual report of Council; address by the president (the Earl of Ravensworth); on the present position of the cruiser in warfare, by Rear-Admiral S. Long; on approximate curves of stability, by W. Hök. Thursday, March 23.—Morning meeting, at twelve o'clock: Some considerations relating to the strength of bulkheads, by Dr. F. Elgar; on the measurement of wake currents, by George A. Calvert; on the new Afonasieff's formulæ for solving approximately various problems connected with the propulsion of ships, by Captain E. E. Goulaeff. Evening meeting, at seven o'clock: Some experiments on the transmission of heat through tube-plates, by A. J. Durston; some notes on the testing of boilers, by J. T. Milton. Friday, March 24.—Morning meeting, at twelve o'clock: On an apparatus for measuring and registering the vibrations of steamers, by Herr E. Otto Schlick; on the re-pairs of injuries to the hulls of vessels by collisions, stranding, and explosions, by Captain J. Kiddle. Evening meeting, at seven o'clock: Some experiments with the engines of the s.s. *Iveagh*, by John Inglis; on the cyclogram, or clock-face diagram, of the sequence of pressures in multi-cylinder engines, by F. Edwards; presentation of an address from the Institution to the Right Hon. the Earl of Ravensworth, on his retirement from the office of president. In addition to the above there is a paper by Lord Brassey on merchant ships as cruisers. The annual dinner was held at the Holborn Restaurant yesterday evening. In summer the Institution will meet at Cardiff.

ON Friday a deputation will wait upon Mr. Campbell Bannerman to make some representations as to the position of those Woolwich cadets who have taken up science at the entrance examination. The existing system at the Royal Military Academy, as we have repeatedly taken occasion to point out, is very unfavourable to cadets of the scientific type, and it is hoped that the approaching interview may lead to the adoption of more reasonable methods. Among the members of the deputation will be Sir Henry Roscoe, Sir Henry Howorth, and the head masters of Rugby, Cheltenham, and Clifton.

MR. W. L. CALDERWOOD has resigned the post of director of the Laboratory of the Marine Biological Association at Plymouth. He vacates the residence early in April.

WE are privately informed of the death, on the 7th instant, of Dr. G. Vasey, the chief of the botanical section of the United States Department of Agriculture at Washington. He was a native of Yorkshire, we believe, and emigrated to America many years ago. The grasses of North America were his special study, and he published several important works on this family. The "Grasses of the Pacific Slope" and the "Grasses of the South-west," fully illustrated, are his latest works; but the former is not yet completed. Dr. Vasey wrote also on the agricultural value of the grasses of the United States. Last year he visited England, and made many friends through his amiable disposition.

WE learn with regret, from the daily papers, that the Rev. W. Woolls, of Burwood, near Sydney, New South Wales, has lately died. It is stated that he emigrated from England as long ago as 1831, and he certainly did much to promote science in the country of his adoption. Botany was his favourite study, and he made several important contributions to botanical literature, chiefly on the botany of New South Wales. He was president of the "Cumberland Mutual Improvement Society," and in that capacity delivered a number of carefully compiled instructive lectures on the vegetable products and resources of the colony, and other branches of botany. One of the most interesting of his published lectures is on the progress of botanical discovery in Australia, which is indeed a concise and correct history of the subject. It was he who wrote the appreciative reviews of the volumes of Bentham's "Flora Australiensis" that appeared in the *Sydney Morning Herald*, and he himself published separate accounts of the plants of the neighbourhood of Sydney, of the Paramatta district, and of the colony of New South Wales.

THE *Botanisches Centralblatt* announces the death of Dr. Karl Prantl, Professor of Botany in the University of Breslau, and director of the Botanic Garden there. For some years past Dr. Prantl has edited *Hedwigia*, a journal devoted to cryptogamic botany; but it was chiefly as a teacher that he was known. An English edition of his "Lehrbuch der Botanik" was edited by Dr. S. H. Vines in 1880.

THE following are among the lecture arrangements at the Royal Institution after Easter:—Mr. John Macdonell, three lectures on symbolism in ceremonies, customs, and art; Prof. Dewar, five lectures on the atmosphere; Mr. R. Bowdler Sharpe, four lectures on the geographical distribution of birds; Mr. James Swinburne, three lectures on some applications of electricity to chemistry (the Tyndall lectures). The Friday evening meetings will be resumed on April 14, when a discourse will be given by Sir William H. Flower, on seals; succeeding discourses will probably be given by Prof. A. B. W. Kennedy, Prof. Francis Gotch, Mr. Shelford Bidwell, the Right Hon. Lord Kelvin, Mr. Alfred Austin, Mr. Beerbohm Tree, Prof. Osborne Reynolds, Prof. T. E. Thorpe, and other gentlemen.

DR. H. WOODWARD, F.R.S., is the president of the Malacological Society which was founded lately at a meeting held at 67, Chancery Lane. The Society will meet at the same place on Friday, April 14, at 8 p.m., and again on the second Fridays in May and June, after which there will be no meeting till November.

ANY one who may desire to learn all that is best worth knowing about the progress and prospects of technical education should read an admirable lecture on the subject delivered by Sir Philip Magnus last week before the Society of Arts, and printed in the current number of the Society's Journal. Sir Philip is of opinion that what is now wanted is the co-ordination of our resources and the simplification of our machinery. The Technical Instruction Committees, with the help of their able secretaries, are doing good and useful work, although much of it is necessarily impeded by the restrictions of the Acts of Parliament under which they work. Between these bodies and the School Boards, Sir Philip urges, there should be earnest cooperation. To them, acting together, and strengthened by the representatives of other educational interests, should be ultimately submitted the duty of making that further provision for secondary education, the need of which is generally admitted.

A MEETING was held at the First Avenue Hotel on Saturday last for the purpose of forming a Cage-bird Club. Dr. Martin, chairman of the Norton Ornithological Society, and vice-president of the London Cage-bird Association, occupied the chair; and a paper was read by Mr. W. H. Betts, who explained that the object of the club was the enrolment among its members of ladies and gentlemen who, from the fact that the majority of cage-bird clubs were held at public-houses, were debarred from membership thereof. He said the club would endeavour to train novices in the management of cage-birds, would give encouragement and assistance to ornithological societies generally, would circulate literature with the object of improving the moral tone of the cage-bird fancy, and would endeavour to prevent cheating at shows and to put an end to brutality. On the motion of the Rev. W. K. Suart, president of the Cage-Bird Association, seconded by Mr. George Crabb, president of the London and Provincial Ornithological Society, it was determined that the club should be founded. Mr. Betts was appointed honorary treasurer, and Miss E. A. Darbyshire honorary secretary.

DR. JAMES RORIE, writing from Westgreen House, Dundee, sends us the following note on a brilliant meteor:—"A very brilliant meteor, or fire-ball, was seen here about 6.23 p.m. on Saturday evening, the 18th inst. When first observed it was about 70° above the horizon south-south-west from the asylum, and moving in a direction from east-south-east to west-north-west. It was visible for about five seconds, and appeared like a large pale blue ball of fire throwing off jets of red-coloured flames, and leaving behind it a pale white silvery streak, marking its course across the sky like a very thin line of vapour, but at the point near the horizon where the meteor disappeared leaving a shining electric blue colour. This streak was in all probability composed of dust particles thrown off by the meteor during the passage in a state of ignition through the atmosphere, as it remained visible for nearly three-quarters of an hour, first as a straight line, and then, evidently caught by the westerly wind, becoming gradually contorted, and, slowly expanding and disappearing, it passed overhead like a long thin twisted cloud of pale blue smoke."

DURING the latter part of last week the high pressure over France gave way, and several shallow secondary depressions passed across our islands, accompanied by northwesterly winds, snow and hail showers. Sharp frosts occurred in places at night, the shade minima varying from 20° to 23°, while the grass temperatures were much lower, the thermometer on Saturday night falling as low as 12° to 16° in the Midland counties and in London; but during the bright intervals of the day-time the maxima reached 50° and upwards. Towards the close of the week an anti-cyclone which previously lay off our south-west coasts spread over the United Kingdom, and extended eastwards over the continent. The weather during the next few days became fine and bright generally, with the exception of fog in the neighbourhood of London and the south-east of England. The maximum day temperatures exceeded 60° at several stations, but the nights continued exceptionally cold, the ground being thickly coated with hoar frost. Such severe frosts as those experienced on several nights during the past week rarely occur so late in the season. The *Weekly Weather Report* for the week ending the 18th instant shows that, notwithstanding the very low minimum temperatures, the averages for the week were rather above the mean in England and the south of Ireland. Rainfall was considerably in excess of the average in the north of Scotland, but less in all other parts. The greatest amount of bright sunshine was recorded in the north-east of England, where there was 52 per cent. of the possible amount; the lowest average amount was 18 per cent. in the north of Scotland.

WITH the view of enabling masters of vessels to know what weather to expect at sea in the far East, and to choose the best routes, all the observations recorded in the archives of the Hong Kong Observatory made between 0° and 45°, and between Singapore and 180° E. Gr. are being tabulated, and will serve for the construction of maps, which will ultimately make it possible to issue pilot charts for the China Seas. Dr. Doberck invites all persons having old log-books in their possession to send them to him on loan. There are log-books of our large lines which, if forwarded to the proper quarters, might help to make passages shorter, pleasanter, and safer.

THE Societies forming the Scientific Alliance of New York have held their first joint meeting, the object being to present the needs of science in that city, and the plans and purposes of the Council of the Alliance. The addresses delivered on this occasion have now been published as a pamphlet. We may note that the membership of the societies is over 650, and is said to include the names of nearly all persons in New York who are interested in pure science.

COLUMBIA COLLEGE, New York, has received from Mr. Loubat an endowment which is to be used for the encouragement of the study of (1) The history, geography, and numismatics, (2) the archæology, ethnology, and philology of North America. It will permit an award at least every five years alternately in these two groups of subjects. This year two prizes of 1000 dollars and 400 dollars will be given for the best works published in English on the subjects in question. The author need not be a citizen of the United States. The works must have been published since January 1, 1888, and must be based on original research. Copies must be sent, not later than June 1 of the present year, to the president of Columbia College, whose secretary will furnish copies of the regulations adopted.

MR. THOMAS STEEL, of Victoria, has been visiting several zoological gardens in Great Britain and America; and in the February number of the *Victorian Naturalist* he gives an interesting account of some of his experiences. In the London Zoological Gardens he was naturally attracted especially by animals and birds from Australia. The kangaroos seemed to him to have very small quarters compared with those set apart for kangaroos in the Melbourne gardens. Nevertheless, he thought them "fairly healthy and sleek." Mr. Steel was much pleased with a pair of Australian brush turkeys, who were evidently "quite at home in their enclosure." The laughing jackass, however, was the animal which interested him most strongly. He had "quite a thrill of pleasure" when he recognised its "well-remembered voice." Of the collection of animals in the Central Park, New York, Mr. Steel formed no very high opinion. He was much surprised that so mighty a city should be "so far behind in a matter of this kind." Of the "dejected-looking lions" in the Central Park he says that they were greatly to be pitied. They were "cooped up in the smallest of cages, with no proper shelter and no exercising yard."

THE *Kew Bulletin*, appendix ii. 1893, consists of a list of the new garden plants of the year 1892. The list includes not only plants brought into cultivation for the first time during 1892, but the most noteworthy of those which have been reintroduced after being lost from cultivation. Other plants included in the list have been in gardens for several years, but either were not described or their names had not been authenticated until recently. These annual lists, as the *Bulletin* points out, are indispensable to the maintenance of a correct nomenclature, especially in the smaller botanical establishments in correspondence with Kew, which are, as a rule, only scantily provided with horticultural periodicals. The lists also afford information respecting new plants under cultivation at Kew, many of which will be distributed from the Royal Gardens in the regular course of exchange with other botanic establishments.

PROF. P. H. SCHOUTE and some other Dutch mathematicians have undertaken to edit, under the auspices of the Mathematical Society of Amsterdam, a "Revue Semestrielle des Publications Mathematiques." The first part of the first volume has just been issued by W. Versluys, Amsterdam. The "Revue" appears likely to be of service to mathematicians far beyond the limits of Holland.

THE Smithsonian Institution has published a collection of translations of some of the best recent memoirs issued in European countries on "The Mechanics of the Earth's Atmosphere." The work has been prepared by Mr. Cleveland Abbe, who expresses his conviction that "meteorology can be advanced beyond its present stage only by the devotion to it of the highest talent in mathematical and experimental physics.

THE geological department of Colby University, U.S., has published a useful "Summary of Progress in Mineralogy and Petrography in 1892," by W. S. Bayley. The volume consists of monthly notes contributed to the *American Naturalist*.

MR. ELLIOT STOCK has issued a little volume, by "Medicus," showing how the height and chest measurement may be increased by systematic exercise. The title of the volume is "How to Improve the Physique."

IN the current number of the *Comptes Rendus*, there are two papers on the use of the electric current in producing high temperatures. In one MM. Moissan and Violle describe two forms of electric furnace which they have used in their experiments. The substance to be heated is contained in a small crucible made of carbon having two holes pierced through its side to allow the carbon rods, between which the arc is formed, to pass. This crucible is surrounded by blocks of lime to reduce the loss of heat on account of radiation. In one form of furnace there is an arrangement by which a piece of graphite, after being heated in the arc, is allowed to fall into a calorimeter, and by this means they have found that a temperature of 3000° can be reached. In the other paper MM. Lagrange and Hoho have investigated the fact, observed by Planté and others, that when you pass a sufficiently strong current through an electrolyte, using as negative electrode a fine wire, and as positive electrode a conductor of considerable surface, a kind of luminous sheath is formed round the negative electrode. The authors find that the heat developed in this sheath is very great, and that by its means a very intense heat can be applied at any point of a body while, on account of the rapidity with which the heat is disengaged, the rest of the body remains cold. As an application of this method they have heated to a bright red the outside of a bar of steel, while the inside remained comparatively cool, then by merely stopping the current the cold liquid has come in contact with the hot steel. In this way they have hardened the outside of bars of steel, while the inside has remained soft and therefore tough.

PROF. L. WEBER, of Kiel, has recently constructed a mercury barometer which can be filled without boiling, and whose vacuum can be freed from residual air at any time in a few seconds. It consists, according to the *Zeitschrift für Instrumentenkunde*, of a vertical tube with two bulbs, one on each side. One of these bulbs ends in a tube to which an indiarubber tube can be attached. The other is connected by a short tube with a capillary constriction. A narrow tube connects the lower end of the bulb with the top of the main tube, thus forming a kind of double barometer. To fill it mercury is poured into the first bulb and allowed to enter the main tube. In doing so it forces the air down through the narrow tube and out by the second bulb. Some mercury also enters the latter by the capillary constriction. On placing the instrument in a vertical position a vacuum is formed at the top of the two communicating tubes, which is slightly longer in the narrow one owing to capillary depression. Barometric readings are then taken in the usual way by means of a scale fixed to the main tube. The vacuum can be tested and easily restored in the following way: The indiarubber tube attached to the first bulb ends in an elastic ball with a small hole in it. This hole is closed by the thumb and the ball is compressed. Mercury is thus forced up the main tube and over into the capillary tube. If there is any residual air it will form a bubble between the two columns, which will on further compression be driven out through the second bulb. On releasing the pressure the vacuum is re-established, and the slight difference of level in the two bulbs is gradually obliterated by the passage of mercury through the capillary contraction. The latter can be replaced by a glass rod with a conically ground end, by means of which the communication between the two bulbs can be temporarily interrupted.

AN interesting communication concerning metallic osmium is contributed to the current number of the *Comptes Rendus* by MM. Joly and Vèzes. Metallic osmium, as usually prepared by the method of Berzelius, which consists in calcining the sulphide in a carbon crucible, takes the form of a powder or a spongy mass of a blue colour. As thus obtained it is rapidly attacked by the oxygen of the air with production of the volatile and dangerously poisonous tetroxide OsO_4; hence the metal constantly exhales a strong odour due to the vapour of the tetroxide. Sainte-Claire Deville and Debray some time ago succeeded in obtaining metallic osmium in the form of beautiful little greyish blue crystals, by passing the vapour of the tetroxide through a strongly heated carbon tube. The density of these crystals, 22·48, was the highest which has been observed for the metal. All the efforts, however, of Sainte-Claire Deville and Debray to fuse osmium in the flame of the oxyhydrogen blowpipe were unavailing. If enclosed in a crucible of carbon surrounded by another of lime, the metal simply remained unchanged, but if heated directly in the flame itself it rapidly disappeared, owing to its conversion into the volatile tetroxide, but no trace of fusion was ever observed. It is now shown that osmium does melt at the temperature of a very powerful electric arc, in a manner analogous to ruthenium. It is, of course, essential that special precautions should be taken in order to prevent loss of the extremely expensive metal by oxidation, and consequently volatility, particularly as the volatile product of the oxidation, the tetroxide, is so injurious to the experimenter. The operation was therefore performed in the electric furnace devised by Ducretet and Lejeune, which enabled the metal to be heated in a carbon crucible placed in a closed chamber traversed by a stream of carbon dioxide. Under these conditions when osmium is rapidly raised to the highest temperature of the electric arc it melts without sensible loss by volatilisation. After fusion osmium presents a very brilliant metallic surface of a beautiful blue colour slightly tinged with grey. It breaks with a crystalline fracture, and is distinguished by its remarkable hardness, being harder than both ruthenium and iridium, readily cutting glass and scratching quartz. Moreover, after fusion osmium appears to be no longer attacked by the atmospheric oxygen, its surface remaining bright greyish-blue.

THE whole of the refractory metals of the platinum family have now been obtained in the liquid form. Of them all osmium has been found the most refractory, its melting point being considerably higher than that of ruthenium. It resembles the latter metal very much in many of its properties, particularly as regards the ready formation of a volatile tetroxide. It differs entirely, however, from ruthenium in aspect, exhibiting as above described a remarkable blue metallic lustre, while ruthenium is more white than platinum, resembling in fact burnished silver. The six metals of the platinum group would appear to more particularly resemble each other in pairs, ruthenium and osmium having many physical and chemical attributes in common, rhodium and iridium being similarly very nearly allied, and palladium and platinum forming the third pair. In many respects, however, osmium exhibits a peculiar and somewhat isolated character, more akin to that of the metalloidal elements; indeed, so marked is this that Deville and Debray termed it the metalloid of the platinum group, Berzelius compared it to arsenic, and Dumas to tellurium.

NOTES from the Marine Biological Station, Plymouth:—Little change has been observed in the floating fauna since last week. *Sarsia prolifera* and medusæ of *Clytia Johnstoni* have again been taken; and *Obelia* medusæ have been plentiful, although for the most part very small and immature. A few *Polydora* larvæ have been taken. *Evadne Nordmanni*, which at times is abundant in the surface waters, has made its first appearance for the year; the few individuals noticed were carrying embryos in the brood pouch. The Nemertine *Cephalothrix lineare* and the crabs *Portunus depurator* and *holsatus* have begun to breed.

THE additions to the Zoological Society's Gardens during the past week include eleven Orbicular Horned Lizards (*Phrynosoma orbiculare*) from California, presented by Mr. William Chamberlain; a Stanley Parrakeet (*Platycercus icterotis*) from Australia, deposited; a wandering Albatross (*Diomedea exulans*) captured off Cape Horn, purchased; an Upland Goose (*Bernicla magellanica*) from the Falkland Islands, a Mute Swan (*Cygnus olor*) European, received in exchange; a Mouflon (*Ovis musimon*), four Shaw's Gerbilles (*Gerbillus shawi*), four Barbary Mice (*Mus barbarus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MELBOURNE OBSERVATORY.—On September 2 last Mr. Ellery, the Government Astronomer, made his annual report to the Board of Visitors to the Observatory. This report shows that with his staff a great amount of work was got through, the following being a brief summary:—With the meridian circle 3590 observations for Right Ascensions, and 2233 for N. P. D. were made, these numbers including the observations for the places of the guide stars used in the astrographic operations. The great telescope, owing to the demands on the staff for the astrographic work, has not been much in use, the routine work having been dropped altogether. It is pleasing to hear that a good start and considerable progress has been made in the part allotted to them in the photographic chart and catalogue of the heavens. Up to June 30, 278 plates had been exposed, excluding a great number obtained for purposes of testing adjustments, &c., although Mr. Ellery remarks that the weather since May was anything but inviting for such work. With the photoheliograph 201 sun pictures were obtained. The observations and records relating to terrestrial magnetism, meteorology, and intercolonial weather service, and time distribution have been continued as usual with satisfactory results. In the seventh paragraph of the report Mr. Ellery informs us of the necessity that has arisen for the reduction of expenditure. Mr. White, the chief assistant, and Mr. Moerlin, the second assistant, were both called upon to retire on September 30, having attained the age of sixty years, both a considerable loss to the observatory, having served there thirty-one and thirty years respectively, and Mr. Ellery found it necessary to close the observatory workshop, and dispense with the mechanic. In a re-organisation of the duties it will be necessary, he says, to put in abeyance observations with the great reflector, reduce meteorological work, including some photographic registration, stop ordinary extra-meridian observation, except the most important, reduce publications and issue of weather charts, and generally to limit operations to the most important and urgent kind. Such a reduction as this after so many years of smooth working and the loss of two such experienced and efficient officers must fall heavily on Mr. Ellery's shoulders, but we are glad to hear that the new scheme is now in working order. We hope to hear also that Mr. Wallace's services have been retained for the astrographic chart, as Mr. Ellery says in a supplementary report that without him this undertaking will have to be dropped.

NATAL OBSERVATORY.—Just as in his former report, Mr. Nevill, the Government Astronomer, is indebted to several ladies for assistance in the observatory, without whose aid he says the numerous astronomical and meteorological computations and reductions could not have been carried out (Report of the Government Astronomer for the year ending June 30, 1892). Again, he urges the necessity of removing the transit to another position, this instrument being so close to the equatorial that only one of them can be used at a time. Besides the usual observations for the comparison of the declinations deduced from observations made at observatories in both hemispheres, by a comparison (Talcott's method) of the zenith distances of northern stars and southern circumpolar stars, the work for determining the latitude of the observatory has been brought to a conclusion and awaits publication. The work, comparing the Greenwich lunar observations from 1851-1888 with the basis of Hansen's Lunar Tables, comprising a discussion of four thousand observations, has been completed, and auxiliary tables, founded on the corrections thus deduced, are now being formed. Several observations of Mars were made to determine the distance of the sun, and these are at present being reduced.

THE BIELIDS OF 1872, 1885, AND 1892.—In "Our Astronomical Column" on p. 451 we referred to a note by M. Bredichin on the Bielids, in which he said that from observations made last year it seemed very probable that the densest part of this swarm had undergone perturbations, amounting to a recession of the ascending node of nearly 4°, due to the proximity of the planet Jupiter. In the current number of *Astronomische Nachrichten*, 3156, he further suggests that the swarm has undergone a separation, perhaps into many parts, an analogous case of such a separation having occurred in the comet 1889 I. The force which accomplishes this division he denotes by I. at the commencement of separation and assumes that its direction coincides with the line of the radius vector, being positive and negative when directed towards and from the sun. Denoting by R the radius of the earth at the time of the meeting with the swarm, and the common radius vector, v the true anomaly of this radius in the original orbit, and v_1 that in the derived orbit; representing the angle between this common radius vector and the tangent to the original orbit by β, and with any one on the derived orbit by β_1, he educed the following values for the elements of the three orbits, where m is the value of the velocity of commencement for one second of time:—

	Comet.	I.	II.	III.
T	1859·390 G.M.T.	1872·986	1885·983	1892·976
π	109° 50′·4	108° 55′·0	108° 45′·3	108° 59′·2
☊	246 1·3	—	—	—
i	12 22·0	—	—	—
log a	0·54950	0·55149	0·54833	0·55050
log e	9·87711	9·87788	9·87668	9·87750
log q	9·94123	9·94087	9·94138	9·94103
U	6·672	6·718	6·645	6·695
log R	—	9·99395	9·99397	9·99426
log r	—	9·94216	9·94156	9·94146
v	—	+ 5° 48′·0	− 3° 34·0	+ 3° 3′·0
v_1	—	+ 6 43·4	− 2 28·9	+ 3 54·2
β	—	87 30·5	91 32·0	88 41·4
β_1	—	87 6·5	91 4·2	88 19·3
I	—	− 0·0099	− 0·0116	− 0·0095
m	—	292m.	342m.	279m.

COMET HOLMES (1892 III.).—M. Schulhof's ephemeris for this comet gives for the ensuing week:—

12h. Paris Mean Time.

1893.		R.A. (app.) h. m. s.		Decl. (app.) ° ′ ″
March 23	...	3 8 3·8	...	+35 47 40
24	...	9 52·5	...	50 28
25	...	11 41·4	...	53 16
26	...	13 30·5	...	56 3
27	...	15 19·8	...	35 58 49
28	...	17 9·3	...	36 1 34
29	...	18 58·9	...	4 10
30	...	3 20 48·7	...	36 7 2

PROF. HALE'S SOLAR PHOTOGRAPHS.—Among the latest advancements in obtaining photographs of the sun, including simultaneously the chromosphere, faculæ, spots, &c., Prof. Hale has distinguished himself especially in this direction. With regard to the method which he adopts, M. Janssen communicates to the *Comptes Rendus* for March 6 (No. 10) a few words. I ask the Academy, he says, "la permission de lui faire remarquer que le principe de cette seconde fente a été très nettement indiqué par nous dans les Communications faites à l'Académie en 1869, et, avec plus de détails, dans une Communication faite au Congrès de l'Association britannique tenu à Exeter la même année."

GEOGRAPHICAL NOTES.

THE recognition accorded to geography in the University of Cambridge is not confined to the lectureship. The subject of the English essay proposed for competition this year by members of the University is announced as "The influence exercised upon British literature by the geographical features of the country." Probably "conditions" would convey the meaning better than "features," but apart from such detail, the subject is one likely to turn the attention of competitors to a much neglected matter —the geography of their own country.

THE survey of Greece is being actively carried on by the Austrian Government surveyors, who undertook the work in 1889. The primary triangulation is already completed, and while filling in the topographical details of the provinces of Thessaly and Albania the survey officers will be accompanied by an Austrian botanist and geologist from whose studies much new information is expected.

ONE of the interesting minor results of M. Dybowski's recent journey from the Mobangi to the Shari was the discovery that the natives of that part of the Sudan use chloride of potassium instead of chloride of sodium to season their food. They carefully select plants which on burning yield an ash containing a minimum of carbonates, and extract their "salt" by boiling water, subsequently filtering and evaporating the solution.

DR. A. GLOY has recently published a very interesting discussion of the population of Schleswig-Holstein, tracing its distribution to the character of the land. In order to represent graphically the cause and effect on the same paper, the various agglomerations of people from single cottages to towns of over 2000 inhabitants, are shown by dots of increasing size on a geological map. It thus becomes apparent that the population is arranged so that the fertile fenlands and clay ridges which run from north to south are relatively thickly peopled, while the belt of sandy and barren soil separating them has few houses except along its boundaries. The type of dwelling in rural villages is also found to vary, showing a clear relation to the former extension of the Slav tribes westward before the time of Teutonic predominance.

IN a careful study of the political divisions of the earth, Dr. A. Oppel has come to the conclusion that about 1,700,000 square miles are uninhabited or ownerless, about 5,000,000 square miles more without settled government, and the remaining 45,000,000 square miles are occupied by definite states. He recognises seventy-five such states, but most of them are of such insignificant superficial extent that the eighteen largest make up 87 per cent. of the whole area.

FLIES AND DISEASE GERMS.

AS we become more intimately acquainted with the nature of pathogenic micro-organisms, the manner in which their distribution takes place also becomes more intelligible. For several years past, through researches made by Grassi, Cattani, and Tizzoni, it has been known that flies are capable of disseminating cholera bacteria. These authors placed minute quantities of these bacilli on to the bodies of flies and found that after carefully preserving them under a glass shade in diffused daylight for an hour and a half and longer, when introduced into sterile culture media these flies gave rise to typical cholera growths. These results have quite recently been confirmed by Simmonds. Further experiments on the part played by flies in the propagation of disease germs have been made by Celli, who *fed* flies with the sputum from phthisical patients, also with pure cultivations of the typhoid bacillus, of anthrax, and other organisms. The particular microbes experimented with were afterwards demonstrated in the excreta of these flies, partly by microscopic examination and partly by direct inoculation into animals. The latter method was especially successful in the case of the anthrax and tubercle bacilli. A paper which has just appeared by Sawtschenko in the *Centralblatt für Bakteriologie*, vol. xii. p. 893 ("Die Beziehung der Fliegen zur Verbreitung der Cholera") contains an account of some experiments which the author has made on the fate of cholera bacilli when introduced into flies. The flies used in these investigations were (1) the common small house-fly and (2) a much larger variety, which, from the description given, would seem to answer to our so-called "blue-bottle fly." It was further marked by its rapid flight, its rare occurrence within doors, by feeding on all manner of decaying substances, besides being frequently found on articles of food of all kinds. These flies were placed in shallow dishes containing a few drops of broth infected with cholera bacilli, after which they were removed and fed on raw meat or sterile broth. In some cases the excrements of cholera patients were substituted for the cholera cultures. It would appear very difficult to keep flies alive in captivity, for the healthy as well as those experimented upon died in nearly all cases after twenty-four hours; in only very few instances was it possible to preserve them four days. Not only were the excreta of the flies carefully examined for cholera bacilli, but in many cases the whole contents of the abdominal cavity were removed with all the proper antiseptic precautions, and inoculated into culture tubes. This latter practice was adopted in order to satisfactorily dispose of all suggestion of the presence of cholera germs in the excreta being due to their accidental contamination from the feet of the flies themselves. In all cases cholera bacilli were found, both in the alimentary tract and in the flies' excreta. Moreover, guinea pigs inoculated with cultivations of cholera microbes obtained from the former died quite as rapidly as when inoculated with ordinary cholera cultures, thus showing that their virulence had not been impaired through residence in the fly's body. In the intestinal tract of those flies fed with cholera excreta, not only were cholera bacilli found, but also other organisms resembling the vibrio Metschnikowi Gamaleïa, and which on inoculation into guinea-pigs and pigeons killed them in twenty-four hours. Similar results were obtained when the vibrio was separated out directly from the cholera excreta and inoculated into these animals. Thus in this case also the virulence of the organism had undergone no abatement during its sojourn in the fly's alimentary tract, thus fully confirming similar results with other organisms obtained by Celli. Sometimes enormous numbers of cholera bacilli were found in the alimentary tract of flies after seventy-two hours, in spite of their having been fed after the first infection with nothing but sterile broth, with the object, if possible, of washing out the bacilli. Sawtschenko makes the alarming suggestion that the bacilli may very possibly be able, under suitable conditions of temperature and nourishment, to multiply within the bodies of flies, in which case the latter must not only be regarded as dangerous carriers of infection, but as a hot-bed for the preservation and further multiplication of cholera bacilli.

SCIENTIFIC SERIALS.

American Journal of Science, March.—The specific heat of liquid ammonia, by C. Ludeking and J. E. Starr. The liquid ammonia used in the experiments was found to contain 0·3 per cent. of moisture, and on spontaneous evaporation to leave only a trace of residue. The specific heat was measured by Regnault's method, the liquid being enclosed in a steel tube of 16·122 cc. capacity, stoppered by a steel screw. The mean value for the specific heat deduced from two series of experiments was 0·8857.—A short cycle in weather, by James P. Hall. If a diagram is drawn exhibiting the changes of daily mean temperature in New York city for a few months it will be discovered that these fluctuations occur every three or four days, on an average, but that some have much greater amplitude than others. In the course of four weeks, perhaps, there will be only two or three conspicuous rises and falls. Upon further scrutiny there will be observed a tendency in these more prominent features of the curve to repeat themselves at intervals of about 27 days. That these and kindred oscillations in New York city are, in the main, representative of temperature changes over the greater part of the United States becomes evident on comparing temperature curves taken at Utah, St. Paul, St. Louis, and New York respectively. A conspicuous rise of temperature at New York is apt to be a day or two behind that at St. Louis, fully two days behind St. Paul, and sometimes nearly a week behind Utah. Mr. Hall attempts to find a relation between this 27-day period and the sun's rotation, which takes place in about the same time.—Kilauea in August, 1892, by Frank S. Dodge. The chief object of interest on the floor of Kilauea was the lava lake of Halema'uma'u, whose surface was found to measure 12·1 acres, which is much larger than any lake in recent years. The lake is nearly circular in form, its longest diameter being 860 feet, and the shortest 800 feet. The lava was about three feet below the rim on an average. Frequent breaks occurred in the rim, from which large flows took place, in some cases covering several acres of the floor. One large flow on the night of August 25th covered about one-third of the floor, and raised its level from one to four feet. The lake was at times very active, with fountains playing over its surface in every direction, as many as fifteen being counted at one time by a careful observer. Small fountains were always to be seen in some locality, and the whole surface was marked by long irregular seams always in motion.—Also papers by Messrs. Chamberlin, Darton, Upham, and Winslow, and the Address delivered before the American Metrological Society, December 30, 1892, by the President, Dr. B. A. Gould.

Bulletin de l'Académie Royale de Belgique, No. 1, 1893.—On Poisson's law of large numbers, by P. Mansion.—On the influence of time upon the mode of formation of the meniscus at the temperature of transformation, by P. de Heen. If a sealed glass tube is partly filled with carbonic acid in the liquid state, and then heated slightly above the critical temperature, the meniscus forming the surface of separation gradually disappears until all the liquid is converted into vapour. But for some time after this has taken place the density of the substance above the surface of separation is less than that below, as may be seen by the appearance of a generating line. If the tube is withdrawn from the water bath at 33°, the formation of a small cloud is observed in the region where the meniscus disappeared, and the latter is gradually reproduced in the same place. The phenomenon is not observed when the tube is inverted, or kept at 33° for 24 hours, thus allowing the two constituents to mix by diffusion. —Two experimental verifications relating to crystalline refraction by J. Verschaffelt.—Crystallographic note on the axinite of Quenast, by A. Franck.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, February 24.—Prof. A. W. Rücker, F.R.S., President, in the chair.—Mr. Everett, junr., read a paper on a new and handy focometer, by Prof. J. D. Everett, F.R.S., and exhibited the instrument described. The focometer is constructed on the principle of the "Lazy tongs," and so arranged that the distance between the object and screen can be varied whilst the lens is automatically kept midway between the two. This gives sharpest definition and the simplest calculation. The lazy tongs has eight cells, formed by eighteen bars $13'' \times \frac{3}{4}'' \times \frac{1}{4}''$, and is capable of being extended to about eight feet, or closed up to about one foot. Brass pins about $\frac{1}{4}''$ diameter and one and half inches long project upward from each joint in the middle row, and serve as supports for clips carrying the lens, object, and screen. The instrument can be used for any lens whose focal length lies between twenty-four inches and one inch or less. Details respecting the most appropriate objects and screens, and practical hints about the working of the instrument are given in the paper. The question of what accuracy is obtainable is also briefly discussed.—Mr. A. Hilger thought the instrument was too flexible to be used for accurate work.—Mr. Blakesley suggested that by using a plane mirror close behind the lens the light would be reflected back, and the length of the focometer could be reduced by one-half.—The President thought Prof. Everett never intended the instrument to compete, as regards accuracy, with the elaborate and expensive apparatus now used, but nevertheless the focometer was a very valuable one, especially for students' work, and was particularly well adapted to impress upon them the facts relating to conjugate foci.—A paper on a hydrodynamical proof of the equations of motion of a perforated solid, with applications to the motion of a fine framework in circulating liquids, by G. H. Bryan, M.A., was read by Dr. C. V. Burton. The object of the paper, which is a mathematical one, is to show how the equations may be deduced directly from the pressure-equation of hydrodynamics, without having recourse to the laborious method of "ignoration" of coordinates. The results are applied to determine the motion of a light framework of wires. When the framework has a single aperture it is shown that no force produces motion in its own direction, and no couple produces rotation about its own axis. In the case of a fine massless circular ring the direction of whose axis is taken as the axis of x, a constant force along the axis of y produces uniform rotation about the axis of z, and a constant couple about the axis of y produces uniform translation along the axis of z. In conclusion the author states that the results might be made to furnish mechanical explanations of certain physical phenomena. The President said the author had done good service by attacking the difficult problem by elementary methods. —Dr. C. V. Burton made a communication on plane and spherical sound-waves of finite amplitude. The first part of the paper refers to plane waves. This subject had been considered by Riemann, but Lord Rayleigh had criticised that part of Riemann's work, where it is held that a state of motion is possible in which the fluid is divided into two parts by a surface of discontinuity propagating itself with constant velocity, all the fluid on one side of the surface of discontinuity being in one uniform condition as to density and velocity, and on the other side a second uniform condition in the same respects. After quoting Lord Rayleigh's criticisms the author shows that the same objection applies when the velocity and density on either side of the surface may vary continuously in the direction of propagation, and the velocity of propagation of the surface of discontinuity is also allowed to vary. In each case the assumed motion violates the condition of energy, and can only exist under that special law of pressure for which progressive waves are of accurately permanent type. Inquiry is then made as to what becomes of waves of finite amplitude after discontinuity sets in (which condition must always occur with plane waves), in the course of which it is pointed out that the front of an air disturbance produced by a moving source which starts impulsively, travels faster than the source, even if the velocity of the source exceeds that of feeble sounds. A mechanical analogy suggests that a dissipative production of heat takes place when discontinuity occurs. In all cases Riemann had assumed that the pressure is a function of density only according to the isothermal or adiabatic law, and thus failed to take account of any heat which may be dissipatively produced. Part II. of the paper deals with spherical waves, and contains a mathematical investigation into the conditions under which the motion remains continuous or becomes discontinuous. The criterion is found in the finitude or infinitude of a certain integral. It is shown that if viscosity be neglected, then under any practically possible law of pressure the motion in spherical sound waves always becomes discontinuous. For waves diverging in four dimensions some cases occur in which the motion remains continuous. The general question of spherical sound waves of finite amplitude is then treated of, and the paper concludes with a method of finding the differential equation of an infinitesimal spherical disturbance which is superposed on a purely radial steady motion. Prof. A. S. Herschel inquired whether the nature of the solution for plane waves of finite amplitude was similar to that for ordinary waves-motion? In the latter case everything depended on the instantaneous impulses, for these alone determined the nature of the wave. The President said Mr. Boys' experiments on flying bullets might have some bearing on Dr. Burton's paper. If the conclusions there stated were correct, then the velocity of the air in front of a bullet should be greater than that of the bullet, even if the latter was travelling faster than ordinary sound waves. He now asked Mr. Boys if his photographs gave any evidence of this. Mr. Boys said the fact that the photographs showed disturbances in front of the bullet proved that the disturbance travelled faster. In one case where a large bullet was moving at a velocity rather greater than that of ordinary sound in the medium, the front of the disturbance was about half an inch in advance of the bullet. In another instance where the bullet was smaller and the velocity greater, the distance which the disturbance was in advance of the bullet was somewhat less. In all cases, even when the velocity of the bullet was four times that of sound, the character of the effects remained the same. Dr. Burton replied to the points raised.

March 10.—Prof. A. W. Rücker, F.R.S., President, in the chair.—Dr. C. V. Burton read a paper on the applicability of Lagrange's equations of motion to a general class of problems, with special reference to the motion of a perforated solid in a liquid. The paper shows that to apply Lagrange's equations it is not always necessary that the configuration of the system should be completely determined by the co ordinates, but that under certain conditions one need not consider whether the whole configuration is determined by the nature of the known co-ordinates, nor inquire what is the nature of the ignored co-ordinates. The result, which is arrived at by the aid of the "principle of least action," and the investigation given in Thomson and Tait's "Natural Philosophy," second edition, part i. § 327, is expressed by the following proposition:—If the kinetic energy of a material system can be expressed as a homogeneous quadratic function of certain generalised velocities $\dot{\psi}$, $\dot{\phi}$, . . . only, the co-efficients being functions of ψ, ϕ, . . . only, and if this remains always true so long as the only forces and impulses acting are of types corresponding to ψ, ϕ, . . ., the equations of motion for the co-ordinates ψ, ϕ, . . . may be written down from this expression for the energy in accordance with the Lagrangian rule. The author then applies the proposition to the case of a perforated solid with liquid irrotationally circulating through the apertures, and shows how it may be extended to any number of perforated solids. Incidentally it is mentioned that in equations $(10)^{v}$ and $(10)^{vi}$ (Thomson and Tait, part i. § 327) the sign of $\partial v/\partial \psi$ should be reversed. A difficulty which arises in applying the result of § 319, example G, in the same work, to the motion of solids through liquids is also referred to. A criticism by Mr. A. B. Basset on Mr. Bryan's recent paper and also on Dr. Burton's paper was read by Mr. Elder. Mr. Basset regards the process employed by Mr. Bryan in obtaining the equations of motion as a distinctly retrograde step, and thinks the most scientific way of dealing with dynamical problems is to avoid the unnecessary introduction of any unknown reactions. The advantages of the theory of the impulse are described by Mr. Basset, and the parts which require care when applying the theory to cyclic irrotational motion pointed out. Comparisons are then made as regards simplicity, between the different methods of treating the subject which have been used by Mr. Bryan, Prof. Lamb, and himself. With reference to Dr. Burton's paper he thinks it will tend to complicate rather than elucidate the subject. An account of how Lagrange's original equations had been modified by Hamilton, Routh, and himself is given at some length, and the advantages and power of the mixed transformation, which he had developed are pointed out. Prof. Henrici said he agreed with Mr. Basset in preferring the more general method, but thought the independent treatment of special problems as given by Mr. Bryan and Dr. Burton, very desirable.—Dr. Burton in reply said he concurred with Mr. Basset on some points, but thought it decidedly advantageous to look at problems from different points of view. The investigation he (Dr. Burton) had given was applicable to any number of solids, and on the whole simpler than Mr. Basset's. The President pointed out that no attack had been made on the validity or accuracy of Mr. Bryan's or Dr. Burton's work. As to simplicity of the various methods, different opinions might be expected to exist. He himself thought it very desirable that such problems should be approached from different sides.—Prof. G. M. Minchin read a paper on the magnetic field of a circular current.—A paper on the differental equation of electric flow, by Mr. T. H. Blakesley, was postponed.

Royal Microscopical Society, February 15.—Mr. A. D. Michael, President, in the chair.—Mr. E. M. Nelson exhibited a microscope made by Messrs. Watson, to which several novelties had been applied.—Mr. J. W. Lovibond read a note on the measurement of direct light by means of the tintometer. Mr. Nelson said that the wonderful results obtained by the author by means of his instrument were perfectly surprising. It was, in fact, equal to discovering differences down to millionths of a tint; having had the pleasure of seeing and using it he soon found that there was a very decided difference in the colour sensation of his own eyes, which until that time he had never suspected. It had done such marvels when applied to macroscopic purposes that he did not doubt it would do much also when applied to microscopic studies. — Mr. G. S. Marriott's form of mounting and dissecting stand was exhibited and described by Mr. Nelson.—Mr. T. F. Smith read a paper on the use of monochromatic yellow light in photomicrography.—Prof. F. J. Bell read a letter from Dr. H. G. Piffard bearing on the same subject.—A paper descriptive of two species of rotifers by Mr. J. Hood was also read by Prof. Bell.—Mr. Nelson read a paper on the chromatic curves of microscope objectives.—Dr. W. H. Dallinger said that Mr. Nelson was quite right in pointing out that unless we could devise means for employing the shorter wave-lengths of the spectrum we had approached very near to the limits of visual possibility with the means at present at our disposal. But as to the belief expressed by Mr. Nelson that glass such as was used in our objectives was not transparent to the higher violet and ultra-violet rays, and to some extent also to the blue, it must be remarked that there could be no doubt but that the figures of the lenses had much to do with this; it led them up to the consideration of the question as to what would be a suitable form and medium for lenses capable of allowing the higher rays to be used. There could be little doubt that all who believed in a future advantage in the use of monochromatic light foresaw that there must be lenses specially prepared for its use. They all knew now that they had reached the limit of possibility so far as present materials were concerned; for if a lens could be made with a N.A. of 2·00, there was no liquid medium to use with it, because no medium so employed would be tolerant of living or even organic substances. If, therefore, they could by some means use shortened wave-lengths, they would have accomplished something extremely useful.—The rest of the agenda was postponed in consequence of the lateness of the hour.

Entomological Society, March 8.—Capt. Elwes, President, in the chair.—Herr Pastor Wallengren, of Farhult, bei Höganäs Sweden, and Herr Hofrath Dr. Carl Brunner Von-Wattenwyl, of Vienna, were elected Honorary Fellows of the Society to fill the vacancies in the list of Honorary Fellows caused by the deaths of Prof. Hermann C. C. Burmeister and Dr. Carl August Dohrn.—Dr. D. Sharp, F.R.S., exhibited a fine species of *Enoplotrupes* from Siam, which was believed to be new, and which he thought Mr. Lewis intended to describe under the name of *E. principalis*. This insect has great power of making a noise, and the female seemed in this respect to surpass the male.—Mr. W. F. H. Blandford said he wished to supplement the remarks which he made at the meeting of the Society on February 8 last, on the larva of *Rhynchophorus*. He stated that he had since found that only the first seven pairs of abdominal stigmata were rudimentary. The posterior pair were well developed and displaced on to the dorsum of their segment, which was thickly chitinised, and bore a deep depression, on the margins of which the spiracles were situated. He added that dissection showed that the posterior pair were the principal agents of respiration.—Mr. W. H. B. Fletcher exhibited a long series of bred *Zygæna loniceræ* and *Z. trifolii*, hybrids of the first generation with the following parentage:—*Z. loniceræ*, male—*Z. trifolii*, female; *Z. trifolii*, male—*Z. loniceræ*, female; also hybrids of the second generation between *Z. trifolii*—hybrid, and *Z. loniceræ*—hybrid. He stated that many of the hybrids were larger than the parent species, and that some hybrids between *Z. loniceræ* and *Z. filipendulæ* were the largest he had ever seen. He added that *Zygæna meliloti* would not hybridise with *Z. loniceræ*, *Z. trifolii* or *Z. filipendulæ*.—Mr. F. W. Frohawk exhibited a bred series of *Vanessa atalanta*, showing the amount of variation in the red band on the fore wings of the female.—Capt. Elwes exhibited a large number of specimens of *Chrysophanus phlæas* from various places in Europe, Asia, and North America, with the object of showing that the species is scarcely affected by variations of temperature, which was contrary to the opinion expressed by Mr. Merrifield in his recent paper on the effects of temperature on colouring. Mr. McLachlan, F.R.S., Mr. A. J. Chitty, Mr. Bethune-Baker, Mr. Tutt, and Mr. Barrett, took part in the discussion which ensued.—Dr. Sharp read a paper entitled "On Stridulating Ants." He said that examination revealed the existence in ants of the most perfect stridulating or sound-producing organs yet discovered in insects, which are situated on the 2nd and 3rd segments of the abdomen of certain species. He was of opinion that the structures which Sir John Lubbock thought might be stridulating organs in *Lasius flavus* were not really such, but merely a portion of the general sculpture of the surface. Dr. Sharp said that the sounds produced were of the greatest delicacy, and Mr. Goss had been in communication with Mr. W. H. Preece, F.R.S., with the view of ascertaining whether the microphone would assist the human ear in the detection of sounds produced by ants. Mr. Preece had stated that the microphone did not magnify, but merely reproduced sound, and that the only sounds made by ants which he had been able to detect by means of the instrument were due to the mechanical disturbance produced by the motion of the insects over the microphone. A long discussion ensued, in which the President, Canon Fowler, and Messrs. Champion, McLachlan, Goss, Hampson, Barrett, Burns, and Jacoby, took part.—Mr. C. J. Gahan read a paper entitled "Notes on the Longicornia of Australia and Tasmania, Part I.; including a list of the species collected by Mr. J. J. Walker, R.N."

Geological Society, March 8.—W. H. Hudleston, F.R.S., President, in the chair.—The following communications were read:—On the occurrence of boulders and pebbles from the glacial drift in gravels south of the Thames, by Horace W. Monckton. North of the Thames near London, the glacial drift consists largely of gravel, which is characterised by an abundance of pebbles of red quartzite and boulders of quartz and igneous rock. With the exception of very rare boulders of quartz, the hill and valley-gravels of the greater part of Kent, Surrey, and Berkshire are entirely free from these materials. The author points out that the river Thames is not, however, the actual southern boundary of the distribution of these glacial drift pebbles and boulders, though the number of localities where they are found in gravels south of that river is few. The author describes or mentions several, of which the following are the most important:—Tilehurst, Reading, Sonning, Bisham at 351 feet above the sea, Maidenhead, Kingston, Wimbledon, and Dartford Heath.—On the plateau-gravel south of Reading, by O. A. Shrubsole. This paper contains observations on the gravel of the Easthampstead-Yately plateau. The constituent elements of the gravel are described, and the author notes pebbles of non-local material near Cæsar's Camp, Easthampstead, on the Finchampstead Ridges, and at Gallows Tree Pit at the summit of the Chobham Ridges plateau. He mentions instances of stones from the gravel of the plateau (described in the paper) which may bear marks of human workmanship. He furthermore argues that the inclusion of pebbles of non-local origin in the gravels may be due to submergence of the plateau up to a height of at least 400 feet above present sea-level, and cites other facts in support of this suggestion. He concludes that the precise age of the gravel can only be more or less of a guess, until the mode of its formation has been definitely ascertained. The reading of these papers was followed by a discussion, in which the President, Dr. Hicks, Mr. J. A. Brown, Prof. J. F. Blake, Mr. W. J. L. Abbott, Mr. Herries, Mr. Monckton, and Mr. Shrubsole took part.—A fossiliferous pleistocene deposit at Stone, on the Hampshire Coast, by Clement Reid. (Communicated by permission of the Director-General of the Geological Survey.) This is practically a supplement to a paper on the pleistocene deposits of the Sussex coast, that appeared in the last volume of the *Quarterly Journal*. An equivalent of the mud-deposit of Selsey has now been discovered about twenty miles farther west, and from it have been obtained elephant-remains, and some mollusca and plants like those found at Selsey. Among the plants is a South European maple. Some remarks were made on the paper by the President, Dr. Hicks, and Mr. W. J. L. Abbott, and the author replied.

Zoological Society, March 14.—Sir W. H. Fowler, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's menagerie during the month of February, 1893, and called attention to two terrapins procured on Okinawa Shima or Great Loochoo Island by Mr. P. A. Holst, and kindly presented by that gentleman. Mr. Boulenger had determined these tortoises as being Spengler's terrapin (*Nicoria spengleri*).—Mr. O. Thomas exhibited and made remarks on a rare antelope (*Nanotragus livingstonianus*) from Northern Zululand.—Dr. Forsyth-Major exhibited and made remarks on a tooth of *Orycteropus* from the Upper Miocene of Maragha, Persia, which he referred to *O. gaudryi*, of the Upper Miocene of Samos. Drawings of the remains of the latter were exhibited, as well as a photograph of a femur of a struthious bird from the same deposit in Samos. The habitats of *Struthio* and *Orycteropus* were thus shown to have been essentially identical in past times, as in the present. Therefore the general conclusions to be drawn from their geographical distribution would apply equally to both.—Mr. Oldfield Thomas made some suggestions for the more definite use of the word "type" and its compounds, as denoting specimens of a greater or less degree of authenticity.—Mr. P. L. Sclater, F.R.S., pointed out the characters of a new African monkey of the genus *Cercopithecus*; and took the opportunity of giving a list of the species of this genus known to him, altogether 31 in number, together with remarks on their exact localities.—Prof. F. Jeffrey Bell read a paper on *Odontaster* and the allied and synonymous genera of the Asteroidea.—Mr. A. D. Michael read a paper upon a new species (and genus) of *Acarus* found in Cornwall. The creature in question, which it was proposed to call *Lentungula algivorans*, was found in some quantity on a green alga (*Cladophora fracta*) near the Land's End. It was a minute creature belonging to the family Tyroglyphidæ, the remarkable feature about it being that, whereas the two hind pairs of legs were terminated by a hard and powerful single claw (which claw sprang from the end of the tarsus), the two front pairs had the tarsus itself hardened and curved strongly downward, forming clinging- and walking-organs; while from the side of the tarsus sprang a long peduncle, flexible in all directions at the will of the creature, and bearing an exceedingly minute claw. This apparatus was not used in climbing, but had become wholly tactile. Such an arrangement was previously unknown in the Acarina.—Prof. Howes described some abnormal vertebræ of certain Ranidæ (*Rana catesbiana*, *R. esculenta*, and *R. macrodon*) in which the so-called "atlas" possessed transverse processes and trans-atlantal nerves. Prof. Howes discussed the bearings of these specimens on the morphology of the parts, deducing the argument that the first vertebra of the Amphibia is probably to be regarded as a representative of at least two vertebræ, of which the formative blastema has become merged in the occiput in the Amniota. The author also described a stage in the development of the urostyle of *Pelobates*, and showed that, in this Batrachian, there is a provisional inversion in the order of development of the parts of the urostyle and precoccygeal vertebræ. He also described a reduced hind limb of *Salamandra maculosa*, in which the reduction and fusion of the parts remaining realised the condition normal for the Urodele limb with numerically reduced digits.

Royal Meteorological Society, March 15.—Dr. C. Theodore Williams, President, in the chair.—Mr. Shelford Bidwell, F.R.S., delivered a lecture on some meteorological problems, which was illustrated with numerous photographs and experiments. The lecturer said that one of the oldest and still unsolved problems of meteorology relates to the origin of atmospheric electricity. Many possible sources have been suggested, among them being the evaporation of water and the friction of dust-laden air against the earth's surface. Having granted some sufficient source of electrification, Mr. Bidwell said that it is not difficult to account for the ordinary phenomena of thunderstorms. Photography has shown that the lightning flash of the artists, formed of a number of perfectly straight lines arranged in a zig-zag, has no resemblance to anything in nature. The normal or typical flash is like the ordinary spark discharge of an electrical machine, it follows a sinuous course, strikingly similar to that of a river as shown upon a map. The several variations from the normal type all have their counterparts in the forms taken by the machine spark under different conditions, and the known properties of these artificial discharges may be assumed to afford some indication as to the nature of the corresponding natural flashes. Thus, for example, the ramified or branched flash, from which no doubt the dreaded "fork lightning" derives its name, is probably one of the most harmless forms of discharge. Ever since the time of Franklin it has been customary to employ lightning rods for the protection of important buildings. According to Dr. Oliver Lodge these are of no use in the case of an "impulsive rush" discharge, which, however, is of comparatively rare occurrence. Lightning conductors, however well constructed, cannot therefore be depended upon to afford perfect immunity from risk. Mr. Preece is of opinion that the "impulsive rush," though easily producible in the laboratory, never occurs in nature. Mr. Bidwell made some remarks as to the duration of a lightning flash and the causes of its proverbial quiver, and suggested an explanation of the characteristic darkness of thunder clouds, and of the large rain-drops which fall during a thunder shower. The lecturer concluded with some observations concerning the probable cause of sunset colours, which he attributed to the presence of minute particles of dust in the air.

Oxford.

University Junior Scientific Club, March 1.—The President in the chair.—Mr. C. H. H. Walker exhibited some compounds of the rare metals from the collection of the late Duke of Marlborough, which had been presented to the University by the Duchess.—Among the papers read was one by Dr. Leonard Hill on cortical localisation.

March 10.—The President in the chair.—Adjourned discussion on Dr. L. E. Hill's paper on cortical localisation.

Cambridge.

Philosophical Society, February 27.—Prof. T. McK. Hughes, President, in the chair.—The following communications were made to the Society:—On the histology of the blood of rabbits which have been rendered immune to anthrax, by Lim Boon Keng. The research was conducted in the pathological laboratory of the University. The rabbits were rendered immune to anthrax by inoculation with the lymph and blood of frogs which had been subjected to various treatment. Previous observers had succeeded in conferring immunity with the use of similar substances. The object of the investigation, however, was to ascertain the changes in the character and relative number of the white cells of the blood after protective vaccination and after the introduction of virulent anthrax. From four to several hours after the injection of the vaccine a great increase in the number of the white cells is noticeable; and the most remarkable feature is the augmentation in number of the coarsely granular (eosinophile) corpuscles. The relative proportion in the numbers of the different varieties of cells is therefore altered, so that instead of forming only from 2 to 4 per cent. of the total number of white cells, the eosinophile corpuscles now constitute about 10 to 25 per cent. This increase persists only a short time, and on the third day the cells may have returned to a normal condition; and at this stage hyaline cells ingesting granular cells may be detected in numbers in certain localities. Although the blood has thus apparently returned to the normal condition, it is found that the state of eosinophile leucocytosis is rapidly reproduced on the introduction of virulent anthrax. After inoculation with a virulent culture of the microbes, the eosinophile cells appear in great numbers, so that they may form 50 per cent. of the white corpuscles, and in one instance an even higher percentage was found. These cells are not only increased in number but are also larger and have larger granules. Similar changes were observed in guinea pigs rendered immune by Dr. Haffkine to the common bacillus. In non-vaccinated rabbits the introduction of anthrax causes profound leucocytosis, but the cells are all very small and the eosinophile corpuscles are only slightly increased in number. General infection occurs in 36 to 48 hours, rapidly followed by death.—On numerical variation in digits, in illustration of a principle of symmetry, by Mr. W. Bateson. An account was given of cases of variation in number of digits so occurring that the parts are symmetrical about a new axis in the limb. Of these the phenomena seen in the bones of a number of polydactyle cats were chiefly important. The normal hind foot of the cat has four toes, each bearing a claw retracted by an elastic ligament to a notch on the *external* side of the second phalanx. This circumstance differentiates digits formed as lefts from those formed as rights. As extra digits are added on the internal side of the limb the symmetry changes. The limb being taken as a *right*, the variations seen are as follows: (1) Hallux present, making five digits: index is then *intermediate between right and left*. (2) Six digits present, internal having two phalanges: the three external digits are then normal rights, *the next two are formed as lefts*; the internal, having a non-retractile claw, is indifferent. (3) Six digits present, internal having three fully-formed phalanges and rectractile claw: the three externals are then normal rights, and *the three internals are formed as left digits*, thus forming two groups in bilateral symmetry about an axis passing between the digits having the relations of index and medius. Several cases of "double hand" in Man form a similar progressive series, and analogous facts in other animals were instanced.

PARIS.

Academy of Sciences, March 13.—M. Lœwy in the chair. —On the true theory of waterspouts and tornadoes, with special reference to that of Lawrence, Massachusetts, by M. H. Faye. The tornado which ravaged the town of Lawrence on July 26, 1891, was observed to descend to the earth and reascend four times during its passage over a tract of country seventeen miles long. After each temporary ascent to the clouds no effect was produced on the land just below. This fact tends to confirm M. Faye's theory, according to which tornadoes, waterspouts, and cyclones have their origin not in hot convection currents from the soil, but in disturbances in the higher strata of the atmosphere. The observed cases of upward suction of heavy objects are explained as effects of the reflection of downward currents by the soil.—On an electric furnace, by MM. Henri Moissan and Jules Violle (see "Notes").—The pancreas and the nervous centres controlling the glycemic function; experiments tending to exhibit the parts played by each of these agents respectively in the formation of glycose by the liver, by MM. A. Chauveau and M. Kaufmann.—Description of a new species of bilateral Holothuria (*Georisia ornata*), by M. Edmond Perrier.—On the observation of the shadows of Jupiter's satellites, by M. J. J. Lauderer.—On the formulæ for annual aberration, by M. Gaillot.—On the transcendentals defined by the differential equations of the second order, by M. Paul Painlevé.—A theorem of infinitesimal geometry, by M. G. Kœnigs.—New semicircular interference fringes, by M. G. Meslin.—Photography of certain phenomena furnished by combinations of gratings, by M. Izarn. On placing a lens with large radius of curvature upon a grating, broad rings, concentric with the Newton's rings observed at the same time, were seen and fixed photographically by means of a layer of sensitised gelatine poured over the lens. On placing one photographic copy of a grating upon another of the same grating, a series of more or less rectilinear fringes was observed, running on the whole transversely to the rulings. A similar phenomenon is described by Brewster in the *Philosophical Magazine* of 1856.—Photographic properties of cerium salts, by MM. Auguste and Louis Lumière. Cerium gives rise to two principal types of salts, cerous and ceric. The former are very stable, the latter are easily reduced, the organic salts being so easily reduced that they cannot be isolated. The best photographic results were obtained with ceric sulphate and nitrate. Paper was soaked in aqueous solutions of these salts and exposed to light under a transparency obtained from a negative. Where the light penetrated, the ceric salt was reduced and the paper changed colour. The image was developed by treating with some carbon compound of the aromatic series, forming an insoluble pigment with the unreduced ceric salt, and fixed by washing. In an acid solution the prints turned grey with phenol, green with aniline salts and orthotoluidine, brown with amidobenzoic acid, &c. The ceric salts are considerably more sensitive than the corresponding ferric and manganic salts.—Intense and rapid heating process by means of the electric current, by MM. Lagrange and Hoho. A bar of steel 1 cm. thick formed one electrode of a strong current passing through an electrolyte. The other electrode had a large surface. The heating was so rapid that, on breaking the current, the liquid suddenly cooling the bar was found to have imparted a brittle structure only to a superficial layer, the rest not having been heated (see also the *Bulletin* of the Belgian Academy). —On metallic osmium, by MM. A. Joly and M. Vèzes (see "Notes").—Researches on thallium; redetermination of its atomic weight, by M. Ch. Lepierre.—On the fluorides of zinc and cadmium, by M. Poulenc.—Quantitative determination of mercury in dilute solutions of sublimate, by M. Léo Vignon.—Alkaline polyphenolic phenates, by M. de Forcrand.—Isomerism of the amido-benzoic acids, by M. Oechsner de Coninck.—Action of carbonic oxide upon reduced hæmatine and upon hæmochromogen, by MM. H. Bertin-Sans and J. Moitessier.—The toxic substance which produces tetanus results from the action of a soluble ferment produced by Nicolaïer's bacillus, by MM. J. Courmont and H. Doyon.—Action of cold on visceral circulation, by M. E. Wertheimer.—On the affinities of the genus *Oreosoma*, Cuvier, by M. Léon Vaillant.—On a new mineral species from Bamle, Norway, by M. Leopold Michel.—On a chloritoid schist of the Carpathians, by MM. L. Duparc and L. Mrazec.

BERLIN.

Physical Society, February 10.—Prof. du Bois Reymond, President, in the chair.—Dr. Raps exhibited and explained a photographic registration-apparatus which he had constructed, primarily for the purpose of obtaining a permanent record of the readings of the voltmeter at central electric stations, but which could also be used for meteorological and physical purposes. The principle of the instrument is as follows. Parallel rays from an incandescent lamp are made to fall on a narrow slit in front of which is the recording needle of the voltmeter or other instrument. The shadow of this needle then causes a white break in the dark image of the slit as cast on to sensitised paper. The paper is moved forward by clockwork, and the hour-intervals are simultaneously printed on it by means of a rotating glass disc. The apparatus is arranged so as not to necessitate any dark chamber for its use, or for the manipulation of the sensitised paper. Prof. Kundt exhibited as lantern pictures two photographs of spectra, of which one showed very marked colours from the red to the violet end, and a photograph of some green twigs with red berries on them. The three photographs had been taken by Lippmann in Paris, and sent to Prof. von Helmholtz. Prof. Kundt then gave an account of some experiments carried on in his laboratory on the influence of temperature on electromagnetic rotation of light in iron, cobalt, and nickel. Trustworthy results could only be obtained with nickel owing to the oxidation of the thin films of iron and cobalt at high temperatures (300°). With nickel a rise of temperature produced at first no change in the rotation, but above 300° a sudden diminution was observed which rapidly became progressively greater; the relationship of the diminution of rotation to the increase of temperature was the same as for the magnetic susceptibility of the metal.

February 24.—Prof. Schwalbe, President, in the chair.—Dr. Raps demonstrated his latest and most improved form of automatic gas-pump for blood-gas analysis. Dr. Richarz developed, in accordance with the kinetic theory of gases, and under certain assumptions as to the constitution of solid bodies, the formulæ for the law of Dulong and Petit. The formulæ furnished an explanation of the divergence from this law which is exhibited by certain elements. Dr. Gross spoke on the laws of energy, proceeding with his criticism of Clausius's views, stating that he regarded Clausius's second law as unproved, and finally coming to the conclusion that entropy is constant.

Physiological Society, February 17.—Prof. Zuntz, President, in the chair.—Dr. von Noorden gave an account of four experiments on nutrition carried out under his direction on men. The first established the fact that nitrogenous waste, as in the case of diabetes, even when excess of proteid is given, can be most definitely lessened by the ingestion of large quantities of carbohydrates. Fats cannot take the place of carbohydrates in the above. The second showed that when carbohydrates are given in increasing quantities over a prolonged period to a person in nitrogenous and calorimetric equilibrium, they lead for the most part to a storage of fat (95 per cent.), and to a less extent of proteid (5 per cent.). The speaker expressed the opinion that this proteid is laid on in the living cell as a sort of non-living reserve proteid. The third set of experiments showed that when the food of a fat person is diminished down to the requirements of a seven- to ten-year-old child, then any increase of its proteid constituents leads to a storage of proteid with a simultaneously considerable loss of fat. Experiments on the respiratory interchange of the person experimented upon showed that the intake of oxygen had been reduced to a minimum and that the respiratory quotient was 0·7. The last set of experiments, made on a gouty patient, showed that with a constant diet, the ratio of intake and output of nitrogen was very variable, at one time a large amount of nitrogen being retained in the body while at another time much more nitrogen was excreted than was given with the food.

AMSTERDAM.

Royal Academy of Sciences, February 25.—Prof. van de Sande Bakhuyzen in the chair.—Mr. Weber read a paper on the origin of the mammalian hair. The author gave a *résumé* of his earlier researches on the scales of mammals, which led him to the hypothesis that the primitive mammals were covered with true scales. A weak point in this hypothesis was, that except Manis and the Dasypodidæ, generally the tail alone is scaled. The author showed, however, that according to the researches of H. de Meyere, the arrangement of the hairs on scaleless skin of numerous mammals is the same as that in scaled parts. Both are placed in alternating groups. The author believed that primitive mammals were covered with scales, and that few and small hairs were placed behind them. On acquiring a constant temperature the hair coat got denser as a good protection from loss of heat. This was the cause of the reduction of the scales, and also mostly of their final loss.—Mr. Lorentz dealt with the influence of the motion of the earth on the propagation of light in doubly refracting media. In the September meeting the author communicated a simple form for the equations which determine the propagation of light in isotropic bodies, moving through the æther with a constant velocity p, the æther itself being supposed to remain at rest. It is now shown how these formulæ are to be modified in the case of a crystallised medium, and to what consequences they lead, as to the motion of light, relatively to the ponderable matter. The velocity of propagation of a *ray of light* (to be distinguished from that of the waves) is found to be $W = W_0 - \frac{p}{n^2} \cos \delta$, W_0 being the value for the same direction and for $p = o$, δ the angle between the ray and the velocity p, V the velocity of light *in vacuo*, and $n = \frac{V}{W_0}$. The course of reflected and refracted rays may be deduced from Huygens's principle or from the condition that $\int \frac{ds}{W}$ must be a minimum (ds being a linear element). Owing to the above value of W, the motion of the earth will neither affect the course of the rays nor the interference phenomena. In this way some experimental results of Ketteler (*Astronomische Undulationstheorie*, pp. 151–173, *Pogg. Ann* Bd. 147), and Mascart (*Ann. de l'Ecole normale*, 2ᵉ série, t. i. pp. 191–196) may be explained.—Mr. Kamerlingh Onnes gave the results of measurements of Dr. Zeeman on the dispersion of Sissingh's magneto-optic difference of phase in Kerr's phenomenon. The dispersion is contrary to the theory of Drude.—He described further a new entoptical phenomenon found by Dr. Zeeman in sighting a split, and communicated the results of the measurements of Dr. de Vries on the variation of the ascension of capillary tubes for æther with the temperature from − 102°C. to the critical temperature 193°·6. The surface work plotted in function of temperature gives a curve turning the convex side to the axis of temperature and ending tangentially to it.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—An Elementary Treatise on Pure Geometry: J. W. Russell (Clarendon Press).—Comité International des Poids et Mesures, Quinzième Rapport (Paris, Gauthier-Villars).—The Intelligence of Animals: G. W. Purnell (Christchurch, N.Z., Whitcombe and Tombs).—How to Improve the Physique: "Medicus" (Stock) Handbook of Jamaica, 1893 (Stanford).—Modern Meteorology: Dr. F. Waldo (Scott).—Gesammelte Abhandlungen über Pflanzen Physiologie: J. Sachs, Zweiter Band (Leipzig, Engelmann).—An Elementary Treatise on Modern Pure Geometry: R. Lachlan (Macmillan).—The Food of Plants: A. P. Laurie (Macmillan).—Elements of Physiography: Dr. H. Dickie (Collins).

PAMPHLETS.—Ueber die Bestimmung der Geographischen Länge und Breite und der Drei Elemente des Erdmagnetismus, &c.: Dr. H. Fritsche (St. Petersburg).—Diseases incident to Workpeople in Chemical and other Industries: W. Smith (Eyre and Spottiswoode).

SERIALS.—Himmel und Erde. Marz (Berlin, Paetel).—Revista Internazionale di Scienze Sociali e Discipline Ausiliarie, February (Roma).—Journal of the Chemical Society, March (Gurney and Jackson).—Annales de l'Observatoire de Moscou, deux série, vol 3, liv. 1 (Moscou).—Medical Magazine, March (Southwood).—Botanische Jahrbucher, Fünfzehnter Band, v. Heft (Williams and Norgate).—Transactions of the Wagner Free Institute of Science of Philadelphia, vol. 3, part 2 (Philadelphia).

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