THURSDAY, JULY 11, 1895.

THE TEACHING OF PATHOLOGY.

The Elements of Pathological Histology. By Dr. A. Weichselbaum. Translated by W. R. Dawson. (London: Longmans, Green, and Co., 1895.)

THOSE who have watched the progress of pathological teaching, in this country especially, must have recognised that during recent years its scope has become much wider, or that at least there is a tendency towards broader conceptions. Cohnheim made an attempt to cast off the narrow fetters of Morbid Anatomy, and to instil into his pupils that wonderful enthusiasm which he himself felt for General Pathology, or, as we may term it, "Morbid Physiology." His "Vorlesungen über Allgemeine Pathologie" still form a monumental record of what he has achieved, and his method must and should be the ideal of every teacher of pathology. Strange to say with his death things reverted into the old groove, and until recently, pathological teaching restricted itself almost exclusively to Morbid Anatomy. "Nec silet mors" is the motto of the Pathological Society: it is not appropriate, because pathology deals not merely with death; its soul and essence, however morbid, is "life." Bacteriology, now a recognised branch of pathology, in spite of all the harm it has wrought, has achieved this, that it has carried us away from the dead-house to the laboratory, and has awakened in us the spirit of experimental inquiry.

Bacteriology should be regarded, however, as an adjunct to pathology, *i.e.* so far as it applies to disease; beyond that it belongs to botany. Every bacteriologist should be a pathologist, and every pathologist should possess an adequate knowledge of physiology as well as a complete mastery over morbid anatomy. The day is to be regretted when we follow the footsteps of our continental brethren, and become mere specialists in the art of growing bacteria and of immunisation. It is not intended to give the erroneous impression that morbid anatomy is not pathology—it still is, as ever it was, the most important partner from the student's as well as the investigator's point of view and for practical purposes; but this must be insisted upon, that the morbid physiology of the body and of disease has been too much neglected. This becomes evident when we look through our text-books and manuals of pathology. Year after year we have fresh treatises on morbid anatomy and histology, or on bacteriology, but there is, if we except Cohnheim's classical work, hardly a book on the pathology of disease and its processes. If we wish to learn this, we have to turn to our standard works on medicine or to the journals. The present volume, the subject of this review, deals exclusively with morbid histology and bacteriology, and for that reason, however valuable it may be, it may be asked whether there was the need for Dr. Dawson to give up so much time to its translation. We have a sufficient number of similar works already; why give us a stone when it is bread we want? Prof. Weichselbaum's name is sufficient to lead us to expect a useful book on bacteriology, and a satisfactory one on morbid histology; more we cannot look for from that source. A careful perusal of the translation justifies our expectations.

Of 441 pages, more than eighty are devoted exclusively to bacteriology, *i.e.* to the description of bacteriological methods, and to a *résumé* of the general principles. If we keep in mind that under each organ also subsequently all the various infective and microbic lesions are carefully discussed, it seems to us that the author has given undue prominence to this, which is, after all, a small part of his subject.

It is difficult to serve two masters, and the result must be that for bacteriological methods and principles we shall continue to consult special works: they are numerous, and it would be difficult to find works of greater usefulness than Prof. C. Fränkel's excellent text-book or the elaborate compilation of Dr. Heim. The directions given for bacterial staining or cultivation are too meagre to be of much use to the beginner. Gram's well-known method, *e.g.*, is described thus: "Sections are placed for half an hour in aniline gentian violet, then for two or three minutes in iodine and potassium iodide, and then in alcohol, which is changed as it becomes coloured. Finally they are cleared and mounted." One can imagine the poor tyro mournfully contemplating the result of those instructions. We therefore adhere to the opinion that in works intended for students, too much should not be offered between the covers; but, if a comprehensive treatise is intended, fulness and completeness of directions and instructions are imperative. The descriptions of the micro-organisms, though short, are succinct and good, so far as they go; but the German edition having appeared in 1892, new discoveries and altered views are wanting, and the briefness is often exasperating.

Now as to the purely anatomical or histological part of the book, it also suffers from shortness, and we must confess that we have works in the English language which are sure to occupy a higher position than this translated importation. Useless Dr. Dawson's work certainly is not; the beautiful illustrations and a chapter on blood examination, short though it be, recommend it. Many of the illustrations are new and original, and are exactly the kind of representation wanted to bring out the salient points in a histological specimen. The English publishers also have done all they could to give the work a good appearance, and altogether it is a pleasant book to possess. It is essentially an annotated picture-book; but as a picture-book it is excellent, and will be of great use to those who consider the study of morbid anatomy and histology a form of "Anschauungsunterricht"; and, indeed, much can be learnt from good pictures. One point this work brings home to us in a painful manner, viz. the decline of pathological anatomy. Bacteriology swamps everything. On the continent, professorial chairs of pathology are occupied by bacteriologists, and the instruction of hygiene is also given over to bacteriologists. The result is that sound pathological anatomy is pushed steadily into the background. So far in this country, fortunately, we have suffered less; in principle, at least, we still consider bacteriology merely a fraction or an element of pathology, but already the spectre has risen, and unless we take care, we also shall be ruled by the bacillus, and find contentment in the haven of mediocrity which so-called bacteriological research opens up to those who, incapable of doing real pathological or physiological work, have leisure to practise bacteriology as a "fireside" game.

In conclusion, a word in praise of the translator and editor: he has done his work excellently, so well, in fact, that one cannot help regretting that he used his gifts and expended his labours on a book hardly worthy of so much conscientious energy and patience. The translation is better than the original in arrangement, type and general "get up." Since it is pleasing to most to possess a nice book, and one which is at the same time instructive, in spite of some remarks which may appear severer than they are meant to be, we may recommend it safely as an addition to the student's library.

A. A. KANTHACK.

THE NATURAL HISTORY OF AQUATIC INSECTS.

The Natural History of Aquatic Insects. By Prof. L. C. Miall, F.R.S. (London: Macmillan and Co., 1895.)

PERHAPS no country possesses so many amateur naturalists as England, at least in proportion to its population, and it is not without significance in this direction that many of our best professional men of science have not thought it undignified to furnish sound information on their special subjects in a popular and yet accurate manner. The present work is a good example of this, and Prof. Miall deserves praise for the admirable account he has put together of the insect inhabitants of our lakes, ponds, and watercourses.

Of course it has not been without forerunners. One of the last works of that well-known writer on popular science, the late Rev. J. G. Wood, was entitled "The Brook and its Banks," and covered much the same ground; but one may say, without any disparagement, that his book was more picturesque or anecdotal natural history than strictly scientific.

Again, Prof. Miall, like every subsequent writer on entomological subjects, is greatly indebted to the laborious researches of Swammerdam, Réaumur, Lyonnet, and others of the early naturalists, but in every case this is freely acknowledged, and he adduces their works as models of patient investigation on the living animal, particularly worthy of emulation at the present time, when attention is almost exclusively paid to phylogeny and classification, to the neglect of the actual life history, where so much still remains to be discovered. Some essential matters are briefly treated in an introductory chapter, such as the equilibrium of aquatic insects, the tension of the surface film of water and its effect on small objects, and also the question of the original habitat of insects, whether terrestrial or aquatic, which Prof. Miall confidently decides as the former, mainly from the universal presence of tracheæ and functionally active spiracles even in purely aquatic insects, showing that such as are fitted for breathing only dissolved air are those that deviate from the general and primitive rule. The chief aquatic Coleoptera are taken first, and certain curious structures in the larva and imago of several families somewhat fully described. Among these we may mention the mouth organs of the larva of Dytiscus, which have been a subject of controversy from the time of Swammerdam and De Geer up to Meinert, Schiödte and Burgess, whose description has been verified by Prof. Miall, and also the well-known tarsal clasping suckers of the adult male, the real structure and action of which was first pointed out by Lowne. The method of respiration in the adult Hydrophilus is well explained, and the extraordinary arrangement for obtaining air from cavities in submerged roots adopted by the larva of Donacia, as discovered by Siebold. Flies with aquatic larva receive considerable attention, no less than 122 pages being devoted to these extremely interesting creatures, which from their transparence, in many cases, have long been favourite objects with microscopists. The development of the Gnat, Chironomus, Simulium, Eristalis, and numerous others is fully gone into, and the amateur naturalist will find plenty of occupation, and derive no little benefit, by following out their structure with this book as his guide. There is a short account of that very beautiful aquatic hymenopterous insect Polynema, which, according to Ganin, deposits its eggs in the eggs of a Dragon-fly; and another form, Agriotypus, said to be parasitic on a Caddis-worm. Caddis-flies (Trichoptera), Sialis, the alder-fly of anglers, the stone-flies, may-flies, dragon-flies, pond-skaters, water-boatmen, and all the rest of the host of insects which pass a large part of their existence in the water, are dealt with in due order, and the descriptions are frequently supplemented with bibliographies, which will be useful to those who require further information on special points. A word must be said for the illustrations, which in large part have been drawn by Mr. A. R. Hammond for this work; they are extremely clear and well executed—quite a relief, indeed, from the old cliches usually considered good enough by publishers to adorn a work of this kind. Altogether, the "Natural History of Aquatic Insects" is a very good and useful specimen of its class.

OUR BOOK SHELF.

The Royal Natural History. Edited by Richard Lydekker, F.R.S., &c. Volume iii. (London: Warne, 1895.)

THE third volume of this excellent "Natural History" finishes the mammals, and commences the birds.

Among the former the Cetaceans, the Rodents, the Edentates with the pouched mammals, and the Monotremes are described at appropriate length. The information is generally up to date, and the illustrations are good. To the notices of the occurrence of Sowerby's whale on the coasts of England and Scotland, may be added that of its being captured some years ago in Brandon Bay, Kerry, the head of the specimen being in the Dublin Museum. The immense group of the Rodents is judiciously treated, most of the more important facts of their history being given. Only six pages are devoted to the egg-laying mammals, and there is no figure of the duckbill's egg.

The chapters on the perching birds and Picariæ are contributed by Mr. H. A. Macpherson and Dr. Bowdler Sharpe. "The number of the existing species of birds being in all probability considerably over ten thousand," the authors are obliged to treat of them even in a more condensed form than were the mammals; still the order of Passeres, which includes by far the majority of known birds, is fairly treated, and most of the well-known or interesting birds are alluded to. Dr. Sharpe confesses his inability to give a diagnosis of the Picariæ, that is in the logical sense, but claims that the group as selected by him possess "certain common features not found among the Passeres." In the last chapter in this volume, he treats of the Jacamars to the Toucans.

Cours Elémentaire d'Électricité. By M. B. Brunhes. Pp. 265. (Paris: Gauthier-Villars et Fils, 1895.)

THE experimental laws and general principles belonging to the study of technical electricity are set forth in this book in an elementary, but strictly scientific, manner. The book reproduces the author's first-year course of theoretical electricity at the Institut industriel du Nord de la France, and its contents furnish just the kind of foundation needed by students of electrical engineering. In several respects, the treatment differs from that generally followed in text-books; hydrodynamic analogues are entirely omitted, and the word potential is not employed, voltage, or E.M.F. between two points, being used to express potential difference.

Off the Mill: Some Occasional Papers. By G. F. Browne, B.D., D.C.L., Bishop of Stepney. Pp. 271. (London: Smith, Elder, and Co., 1895.)

ALPINE climbers, and others who find delight in mountain-peaks and glaciers, may like to read the papers on Alpine subjects reprinted in this volume. The papers originally appeared thirty years ago, and they offer to the present generation of mountaineers an interesting picture of the way in which climbs were then made. The ice-caves in the neighbourhood of Annecy form the subject of one of the papers appealing to scientific readers.

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.*]

A Cyclonic Indraught at the Top of an Anticyclone.

BETWEEN June 7 and 12 an anticyclone, with maximum pressure of 30·20 to 30·30 inches, passed slowly from the north-west across southern New England. The isobars formed well-defined ovals, with their longer axes running from south-west to north-east. It was difficult to locate the centre of the anticyclone because the isobars were broken on the side toward the ocean; but, by drawing a line through the stations showing the maximum pressure, the crest or ridge of the anticyclone could be easily located up to the 11th, after which it passed off the coast and its position became somewhat uncertain, although the pressure continued above normal over southern New England until the night of the 12th.

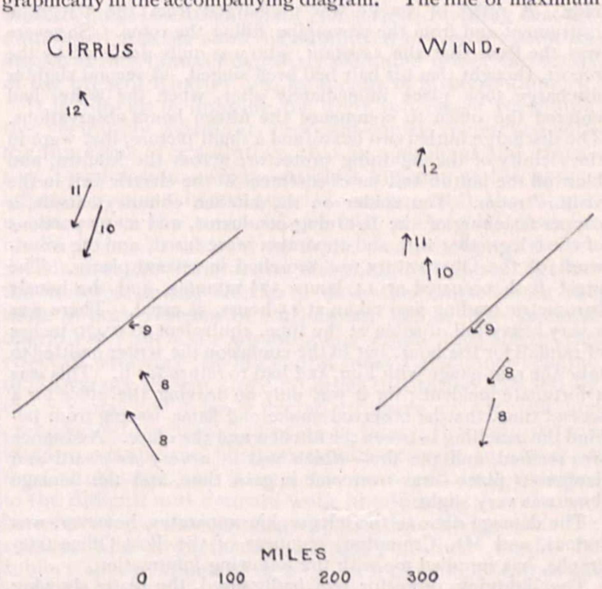
The interest attaching to the anticyclone lies in the fact that cirrus observations obtained on both sides of the line of maximum pressure indicate an indraught at the top of the anticyclone of the same nature as that observed at the bottom of cyclones.

The anticyclone passed nearly centrally over the Blue Hill Meteorological Observatory. As it approached from the north-west, the cirrus clouds on the 8th were observed moving from the south-south-east. As the line of maximum pressure passed over the observatory on the 9th, the cirrus movement shifted to the north-east, from which direction it was observed on the 10th and 11th. This change corresponds almost exactly with what would be observed in the surface wind should a trough of low pressure pass over Blue Hill from the same direction. On the 12th the cirrus shifted to south, and on the 13th to the west, with the approach of a cyclone from that direction.

The direction of cirrus movement and the mean direction of the wind is recorded at the observatory in degrees of azimuth beginning with the south point. The first is measured with a nephoscope, and the second recorded by a Draper anemoscope. The following table gives the cirrus and corresponding wind observations between the 8th and 10th, no cirrus observations being obtained on the 7th. The velocities of the cirrus were obtained by multiplying the observed relative velocities by a factor to reduce to absolute velocities. This factor was determined from direct measurements of cloud heights and velocities carried on for some time at this observatory. The last column in the table contains the directions in degrees of azimuth of the line of maximum pressure in the anticyclone, taken from the maps of the United States Weather Bureau.

	Cirrus.		Wind.		
	Dir. from. °	Veloc. Miles.	Dir. from. °	Veloc. Miles.	Line of max. press. °
June 8, 8 a.m.	329	48	203	29	50
,, 8, 8 p.m.	320	34	225	23	45
,, 9, 8 a.m.	243	6	233	18	47
,, 10, 5 p.m.	213	34	13	12	60
,, 11, 2 p.m.	245	34	21	14	70
,, 12, 8 a.m.	340 ?	16 ?	47	13	?
,, 12, 5 p.m.	341	18	8	22	?

The changes in the direction of the cirrus and of the surface wind, as related to the line of maximum pressure, is shown graphically in the accompanying diagram. The line of maximum



pressure is indicated in each case by the long slanting line. The arrows fly with the cirrus and with the wind, and the length of the arrows indicate the velocity, though on a different scale in the two cases. The small figures near the arrows give the dates of observation.

Repeated observations of this kind, here and elsewhere, ought to throw some light on the causes of cyclones and anticyclones. If an indraught prevails at the top of the anticyclone of the same nature as the indraught at the earth's surface in a cyclone, it seems difficult to avoid the conclusion that there is an area of low pressure in the upper air above anticyclones, notwithstanding the fact that studies of mountain observations by Hann and others lead to an opposite conclusion. In the present case the inward gradient above appears not to have extended entirely to the outer limit of the anticyclone as indicated by the observations on the 12th.

Direct observations of the anticyclonic inflow must, however, be rare: first, because of the infrequency of cirrus in the proper positions, and the general absence of exact methods of measuring the slow motions observed; second, because there is usually a strong eastward drift in the upper air, which greatly interferes with the anticyclonic circulation, and generally overrides it, so that it only becomes strongly marked under stagnant conditions of the general atmosphere; third, the upper air isobars are usually distorted by strong contrasts of temperature in the area of the anticyclone. But notwithstanding these drawbacks, I am confident that with the increasing attention given to cloud observations, cases like the present will be frequently observed. With a great many observations the anticyclonic inflow can be brought out by a system of averaging, as shown in the *American Meteorological Journal* for August 1893.

H. HELM CLAYTON.

Blue Hill Meteorological Observatory, June 17.

Effects of a Lightning Flash in Ben Nevis Observatory.

WHENEVER a thunder-storm passes the summit of the Ben, there occurs almost invariably a discharge from metallic bodies in the Observatory, as the cloud is passing away. A flash of greater or less extent is given off the stoves, accompanied by a sharp crack. In January 1890 there was an exceptionally severe flash; "one of the observers was almost knocked down when sitting writing, and the telegraph wire was fused, and all communication stopped for five days." But more destructive than any previous flash was that which occurred this year on June 19, when the Observatory narrowly escaped being destroyed by fire. Between two and three o'clock on that afternoon, repeated clicks on the telegraph instrument were heard by one of the assistants who was sitting in the office; he had been carefully noting the times at which the clicks occurred, when suddenly the whole office was filled with a brilliant flash and deafening roar. A pillar of smoke was discharged from the telegraph instrument and from the stove-pipe, filling the room. So severe was the flash that the assistant, who was quite deafened by the report, thought that his hair had been singed. A second slighter discharge took place immediately after, when the writer had entered the office to commence the fifteen hours observations. The discharge hurled two boxes and a small picture, that were in the vicinity of the lightning protector, across the kitchen, and blew off the button and outer casement of the electric bell in the visitors' room. The solder on the kitchen chimney outside, a copper fastening of the lightning conductor, and many portions of the telegraphic wire and apparatus were fused, and the woodwork of the Observatory was scorched in several places. The great flash occurred at 14 hours 57½ minutes, and the hourly barometric reading was taken at 15 hours, as usual. There was a very heavy fall of snow at the time, equivalent to 0·470 inches of rainfall for the hour, but in the confusion the writer omitted to take the rain-gauge with him, and had to return for it. This was a fortunate incident; for it was only on leaving the office for a second time, that he observed smoke and flame issuing from behind the panelling between the kitchen and the office. Assistance was secured, and the fire—which was in a very awkward and dangerous place—was overcome in good time, and the damage done was very slight.

The damage done to the telegraphic apparatus, however, was serious, and Mr. Crompton, engineer of the Post Office telegraphs, has supplied me with the following information.

The lightning protector was badly fused, the plates showing a patch of fusion as large as a sixpence. This saved the cable from serious damage. All connecting wires within the building were rendered useless. The majority were so heated as to melt the insulation off, and, in one or two cases, the copper conductors were melted by the discharge. In one case, the fusion set fire to the woodwork.

The coils of Neale's sounder were fused and rendered useless. The keys suffered worst of all, the left pedal or "tapper" bearing the strongest evidence of the severity of the discharge. The back contact (platinum), the brass extension holding the same, and the steel spring (platinum-tipped) above, all being fused into one solid amalgam. The pillar, to which the zinc leading wire from the battery was connected, had a large patch of fusion near its base, and the front platinum contacts of the same (left-hand) pedal were consumed entirely. There were small traces of fusion on the right pedal, but of a trifling character. The line wire connected to the left-hand terminal of the coil had been fused close to the terminal. The interior of the instrument case was considerably blackened, as also the greater portion of the keys, as a result of the "arc" caused by the discharge at the moment of fusion.

The vacuum protector at Aslimtee, the base of the cable, also the plate protector in Fort William Post Office, were fused, but only slightly, the main discharge having expended itself on the summit. The Low Level Observatory instrument and protector were unimpaired, and communication between there and Fort William Post Office was carried on as usual after the removal of the fault in the Post Office protector.

The registering aneroid shows a slight upward kick at the time, but the curve is otherwise fairly steady; the temperature was 31·7° F., the wind south-south-east and light. Heavy snow was falling at the time, which, with a fall on the 17th, made a total depth of nine inches on the summit. St. Elmo's Fire was very strongly felt and heard until after seventeen hours.

WILLIAM S. BRUCE.

The Kinetic Theory of Gases.

IT seems to me that Mr. Burbury's and Prof. Boltzmann's last letters will enable us to reconcile all the *main* differences of opinion which were brought to light in our recent correspondence in the columns of NATURE. From Prof. Boltzmann's letter it appears that the Minimum Theorem can only be applied with absolute certainty to gases whose molecules are not too closely crowded together. Thus the proof that an aggregation of molecules tends *continuously* towards the Boltzmann-Maxwell distribution depends quite as much on assumptions as to the mixing of the molecules *between* collisions as on consideration of what happens *at* collisions. We cannot prove for certain that densely crowded assemblages of molecules such as solids and liquids tend to assume this distribution, and this is just as it should be, for when a substance is capable of existing simultaneously in two states, the distribution cannot be unique. For the same reason the proof does not apply to molecules moving about in a *continuous* medium such as the ether. So far from this limitation being a weak point in the proof, it precludes the theorem from proving too much, or from leading to results which may not accord with experience.

If we do not know that solids and liquids satisfy the Boltzmann-Maxwell distribution, we, nevertheless, know that they are subject to the Second Law of Thermodynamics. It cannot be said that any dynamical "proof of the Second Law" that has yet been given, is so conclusive as the mere statement of the Law itself, but the proof of the Minimum Theorem subject to "Condition (A)" leads to a result somewhat analogous to the statement that when two or more bodies at unequal temperature are brought into thermal contact, their entropy tends to increase. For let the probability of the coordinates on momenta of the molecules of one body lying between certain limits be proportional to F (*all* the coordinates and momenta being included in the multiple differential by which F is multiplied). Let the corresponding probability for a second body be proportional to f. Then when the two bodies are placed in thermal contact, we know of no relation connecting the two simultaneous probabilities, and we may therefore assume them to be independent, so that condition (A) is satisfied, at any rate initially. The theorem then asserts that at all subsequent instants of time, the value of the Minimum Function will be not greater than its initial value, and therefore it either remains stationary or decreases every time the process is repeated. Thus far we can get if no further.

The application of the Second Law depends largely on the distinction between *available* and *unavailable* energy. When we construct a thermodynamic engine for converting heat into work, we introduce just the kind of external disturbances that Mr. Burbury requires every time that the "working substance" is placed in contact with either the "source" or the refrigerator."

G. H. BRYAN.

An Abnormal Rose.

I HAVE in my garden at Reigate a *white* Moss rose-tree, every blossom on which is white except one which is half white and half red, divided diametrically in nearly equal portions.

The colours are not shaded one into the other, but are perfectly distinct, and one petal is half red and half white, the edge of the colouration being quite sharp.

I am told that one similar blossom was produced earlier in the season.

I imagine this is an attempt to revert to its ancestral colour, but by what mechanism such a partial result has been accomplished seems difficult to understand.

NEWNHAM BROWNE.

THERE are several varieties of rose that sport or revert in the manner described by Mr. Newnham Browne. The "York and Lancaster" rose is a familiar example. In this, the recognised or genuine condition is red and white striped; but the proportions of white and red are rarely exactly the same in any two flowers on a bush, and very frequently some are wholly red and some, perhaps, wholly white, though I am not sure on this point. Many other cross-bred plants exhibit this inconstancy, which is supposed to be due to an imperfect blending of the elements of parentage. That the sporting is irregular and inconstant is not to be wondered at, when we consider that a plant is not an individual in the sense of possessing only one set of organs. Any vegetative bud of a plant is capable of producing any and all of the organs of the whole plant, or, if detached from the parent plant, to develop into a similar organism, with all its attributes. Given, then, a cross-bred variety, which is not constant, or "fixed," as florists term it, any vegetative bud may give rise to the cross or to one or the other of the parents.

W. BOTTING HEMSLEY.

Mineralised Diatoms.

NEARLY twenty years have elapsed since you allowed me to announce in NATURE the unexpected discovery of mineralised diatoms in the London clay of Sheppey.

Subsequent investigations demonstrated the existence of these unique microscopic fossils on the same geological horizon at several widely separated localities in the south-east of England; leading to the assumption that the band of diatomiferous earth was continuous throughout the formation.

Herne Bay was one of the places at which, in accordance with expectation, search was followed by success. Revisiting this place, a few days ago, for the first time since the discovery, I readily found the fossil diatoms as abundant as before in some recently fallen blocks of clay about half-way between Herne Bay and Oldhaven Gap. As there has been much waste of land at this spot during the interval, it is interesting to observe the presence of these diatoms in the newly exposed clay, giving support, as it does, to the hypothesis of their general distribution at a definite level throughout the London clay.

Perhaps some readers of NATURE may be going to that part of the coast before long, and will then take the opportunity of verifying my observations.

W. H. SHRUBSOLE.

SIR JOHN LUBBOCK AND THE TEACHING UNIVERSITY FOR LONDON.

THE address in which Sir John Lubbock solicits the suffrages of the Electors of the University of London has aroused feelings of surprise and regret among the friends of higher education in London, owing to the unfortunate nature of the references made to the Teaching University question. Six paragraphs out of ten are devoted to this important subject, and it seems almost incredible that so far from recognising that the Gresham Commissioners' scheme has enlisted a considerable measure of support in the University (*cf.* vol. l. 269; li. 298), Sir John Lubbock refers only to the views of its opponents, and, in accepting them, makes the remarkable statement:

"Feeling that Convocation ought to be consulted on a matter so vitally affecting the University, I would strongly urge, and do my best to secure, that the scheme when arranged should be submitted to Convocation for their approval, to be signified as at a Senatorial Election, and would oppose the Bill unless this were conceded."

Now it must be borne in mind that the Report of the Gresham Commissioners has met with a degree of approval from educational authorities and institutions, which not only far exceeds that extended to any previous attempt to solve the vexed question of University reform in London, but has been sufficiently unanimous to lead to the introduction of the "University of London Act, 1895," in the House of Lords by the late Government. This Bill, in accordance with the general tenour of the resolutions passed by the various institutions named in the Report as constituent colleges of the teaching University, enacted (clause iii. para. 1):

"The Commissioners will have power to make statutes and ordinances for the University of London in general accordance with the scheme of the Report hereinbefore referred to, but subject to any modifications which may appear to them expedient after considering any representations made to them by the Senate or Convocation of the University of London, or by any other body or persons affected."

And further (para. 2):

"In framing such statutes and ordinances, the Commissioners shall see that provision is made for securing adequately the interests of non-collegiate students."

Convocation in January last had the opportunity of exercising its veto in meeting assembled as provided by the Charter of the University on the scheme of reconstitution proposed by the Commissioners, which had previously received the general approval of the Senate. Instead of insisting on this right, it preferred to bring itself into line with the other institutions affected by the scheme, by adopting a resolution in terms almost identical with those employed in the Bill. Only so recently as May, it declined to reconsider this attitude by a majority of two to one, yet it is clear that the Bill, if again brought forward, is to meet with opposition from Sir John Lubbock, if re-elected, unless an amendment is inserted providing that the completed scheme shall be submitted to Convocation for approval in a manner expressly excluded under the terms of the present Charter, viz. by means of a referendum.

It is difficult to imagine by what process of reasoning this seemingly gratuitous proposal can be reconciled with the functions of a statutory, that is a judicial and executive, Commission. Convocation is but one of the bodies affected by the scheme, and in common with the others, it can, under the terms of the Bill, present its case for modifications in the scheme to the Commissioners before the statutes are framed, and like them can appeal against the statutes during the forty days they must lie on the table in both Houses of Parliament before they become operative. Such an amendment could only have the effect of wrecking the latest and most satisfactory scheme of University reform, since no other institution affected by the scheme could be expected to agree to such an unprecedented proposal. Nor is it likely that any person fitted to occupy the position would consent to serve on the Commission, and devote his time and best energies to the difficult and delicate work of adjusting the relations between these institutions, with the knowledge that the statutes and ordinances eventually framed would be subject to the approval of any irresponsible, non-judicial body, let alone one of the institutions closely affected.

For the most part, Sir John Lubbock has held aloof from the controversy on the Teaching University question. Once only does he seem to have taken sides. It is on record that he voted with the majority when the Senate in June of last year passed a resolution expressing general approval of the proposals of the Gresham University Commission, with which action his present attitude is wholly inconsistent. It would be interesting to know whether his descent on the other side of the fence is in any way connected with the absence of opposition to his candidature on the part of the opponents of the scheme. Be this as it may, this uncalled for proposal to subordinate the interests of higher education in London to the pleasure of Convocation, ascertained not after debate, but by a referendum, is not to pass without protest, and we are glad to note that the following letters have already appeared in the press. The first is from Prof. Michael Foster, Sec.R.S., and President of Sir John Lubbock's Parliamentary Election Committee.

"Shelford, Cambridge, July 4, 1895.

"Dear Sir John,—As you know, I am wholly opposed to your view that the scheme for the University of London to be proposed by the Statutory Commissioners ought to be submitted to Convocation for approval. You also know that this difference of opinion, important as it is, does not prevent my desiring that you should continue to represent the University of London in Parliament. I find, however, that your letter addressed to me is understood to show that I agree with all the opinions expressed by you in that letter, and in justice to myself I must make known to my fellow electors and others how wholly we disagree on the above point, and how much I regret the attitude you assume in the matters in question.

"Yours very truly,

"Sir John Lubbock, Bart." "M. FOSTER.

The second has been addressed to Sir John Lubbock by the President and a number of Fellows of the Royal Society:—

"July 6, 1895.

"Dear Sir John Lubbock,—The interests of learning and of education are so closely bound up with the future development of the University of London that we hope you will not regard us as interfering between yourself and the Electing Body of the University if we venture to express our regret at some of the opinions you have put forward in your Election address.

"You state that you would do your best to secure that the scheme (for the reorganisation of the University), when arranged, should be submitted to Convocation for their approval, to be signified as at a Senatorial Election, and would oppose the Bill unless this were conceded.

"You must allow us to point out that this proposal would confer upon Convocation a right, which is without precedent, to supervise the acts of a Commission entrusted with the reorganisation of the University of which Convocation itself is a part.

"The scheme of the 'Gresham Commissioners' has been approved not only by all the institutions concerned, but by the great body of educated public opinion. It is, however, certain that very grave difficulties will arise if the ultimate fate of the scheme is to depend upon the voting papers of Convocation.

"We, therefore, believe that the proposal you support, if adopted, will result in the failure of another attempt to establish a Teaching University in London, and will indefinitely postpone the solution of a question which, after prolonged discussion, seemed to be on the eve of settlement.

"We are, yours faithfully,

"KELVIN (P.R.S.), JOHN EVANS (Treas.R.S.), M. FOSTER (Sec.R.S.), JOSEPH LISTER, RAYLEIGH, DOUGLAS GALTON, T. G. BONNEY, T. E. THORPE, HORACE LAMB, J. H. POYNTING, ARTHUR W. RÜCKER, E. FRANKLAND, N. STORY MASKELYNE, HENRY E. ROSCOE, P. H. PYE-SMITH, J. NORMAN LOCKYER, JOHN ERIC ERICHSEN, WILLIAM RAMSAY, G. CAREY FOSTER."

In his address, Sir John Lubbock states that the opinions of the present Government on the University question have yet to be made known. In view of the fact that the Commission, whose report has been so generally approved, was appointed during Lord Salisbury's last term of office, this attitude ought not to be doubtful.

THE ELECTRICAL MEASUREMENT OF STARLIGHT.

THAT the light of a star is able to produce at the surface of the earth a measurable effect, other than the action on a photographic plate, is a fact which was published in these pages in January last year. The light of stars and planets produces two effects—the one photographic and the other electric. The first—which has, of course, been known for many years—is slow in its operation; the second—which was discovered only a year ago in Mr. Wilson's observatory at Daramona, Westmeath—is almost instantaneous.

In order to obtain the electrical effect, a photoelectric cell of extremely great sensitiveness to light is employed. Such a cell is constructed with selenium, aluminium, and the liquid œnanthol. If we take a strip of clean aluminium—say half an inch long, one-tenth of an inch wide, and thick enough to be fairly stiff—lay it on an iron plate which is heated by a Bunsen flame, and place on the end of the strip a very small particle of selenium, this selenium will melt and form a small black globule of liquid. Let the flame be now withdrawn, and the globule of melted selenium spread over the end of the aluminium strip, by means of a hot glass rod, so that it forms a thin uniform layer of area about ·1 of an inch square on the end of the strip, and let this dark layer cool to a few degrees below its melting point (about 217° C.). Now apply heat again to the under surface of the iron plate until the aluminium strip becomes nearly hot enough to re-melt the layer of selenium. In this process the colour of the layer will gradually change from black to a greyish brown. When it is just on the point of melting, withdraw the heat and blow over its surface; this will instantly check the tendency to melt, and will leave the surface of the selenium in the state in which it is most sensitive to light. If this strip (or rather its selenium-covered end) is immersed in a glass tube containing acetone or œnanthol, and connected with one pole of a quadrant electrometer, whose other pole is connected with a platinum wire sealed into the glass tube, we have a photoelectric cell, in which the action of light falling on the selenium layer results in giving the selenium a positive electric charge and the liquid a negative one, the former charge being conveyed to one pole of the electrometer by the aluminium plate, and the latter to the other pole by the platinum wire sealed into the cell.

Roughly speaking, the difference of potential produced in such a cell as this by ordinary diffused daylight is something between one-third and one-half of a volt.

Such were the seleno-aluminium cells used in the measurement of starlight in January 1894, the liquid in them being œnanthol. This liquid was found to be better than acetone (which had been previously used), not only because of the greater ease with which it can be sealed up in glass tubes, but because it does not act chemically on selenium, which acetone seems to do sooner or later. But it is obvious that a cell formed in this way contains an element of inconstancy; for, the strip of aluminium will at the same time convey to the insulated pole of the electrometer the positive charge generated by light in the selenium and a portion of the negative charge imparted to the liquid, so that the effective E.M.F. is less than it should be; and, again, there will be currents circulating perpetually between the selenium and the back of the aluminium strip, and such currents deteriorate the cell. Hence it happened that such cells always fell off in strength after about six hours. They sufficed, however, to show very easily measurable electromotive forces from the light of the planets, and even from the light of Sirius.

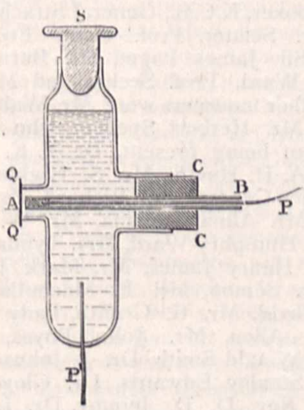
Shortly after January 1894, a very notable improvement was made in the construction of the cells, this improvement resulting from the perception of the cause of deterioration above explained. Instead of a strip of aluminium as a base for the selenium layer, the end of an aluminium wire, about one millimetre in diameter, was used. This wire was enclosed in a glass tube (A, B, in the figure on p. 247), into which it fitted tightly, one end of the wire being flush with an end of the tube. On this end was deposited the layer of selenium, with the same process of heating as that already described. The other end of the aluminium wire inside the glass tube was connected with a fine platinum wire, P, which emerged from the second end of the tube, and which formed the selenium pole of the photoelectric cell.

In this way the liquid is kept out of contact with the aluminium wire, and the deteriorating local currents in the cell are avoided, if the glass tube *exactly* fits round the aluminium wire; but this desirable result has not yet been perfectly attained, the liquid finding its way into the tube after some considerable time. However, in this way have been constructed cells which have remained constant for about three weeks.

In the figure, C C is a cork in which the glass tube, B, containing the aluminium wire at the end A and the attached platinum wire, fits, this cork fitting tightly into the side of the glass cell which contains the liquid. The tube B passes close up to a quartz window, Q Q, cemented to the cell opposite the cork C C. The light of the star is received on the window, Q Q, and is made to fall on the selenium layer at the end A of the tube B. A platinum wire, P′, is sealed into the bottom of the glass cell, and conveys the charge taken by the liquid to one pole of the electrometer, while the platinum wire P conveys the charge taken by the selenium to the other pole of the electrometer; S is a ground stopper at the top of the cell, where the liquid is poured in.

This cell is fitted into a holder which can be fixed to a telescope in place of the eyepiece; and this cell-holder allows of the adjustments which are necessary to bring the point A to the position of the image of a star.

This is the form of photoelectric cell with which, in conjunction with Prof. Fitzgerald and Mr. W. E. Wilson, I measured the electromotive forces of the lights of



Jupiter, Saturn, Vega, Arcturus, Regulus, Procyon, and some other stars last April, in Mr. Wilson's observatory at Daramona, Westmeath. The telescope used was Mr. Wilson's 2-feet reflector.

In order to give a notion of the sensitiveness of the cell to light, I may say that if an ordinary paraffin candle is held at a distance of 9 feet from the window Q Q, it will produce an electromotive force of about ·03 volts; or, to put the matter differently, suppose an ordinary quadrant electrometer, of Clifton's pattern, charged so that a Daniell cell gives a deflection of 400 divisions on the ordinary scale (placed at a metre distance); then the light of the candle at 9 feet falling on the photoelectric cell would give a deflection of twelve divisions, and the deflection varies inversely as the distance of the candle.

Now the light of Vega as concentrated in the 2-feet telescope gives a slightly greater deflection than the (of course unconcentrated) light of the candle; so that we are evidently dealing with easily measurable quantities.

The cell is sensitive to all the rays of the spectrum, but the maximum effect is produced by the yellow. It is sensitive to rays considerably below the visible red and beyond the blue.

The light of Arcturus was found to give 0·82 of the E.M.F. produced by the candle at 9 feet; the light of Saturn 0·56, which was also about the value of the light of Regulus. Unfortunately neither Sirius nor Capella, nor any star in Orion, nor any in the Great Bear, was available for our observations; but these we hope to include, before long, in the list of measured stars.

It will be observed that in this electrical measurement of starlight we do not measure *currents*, but *electromotive forces*—we do not use a galvanometer, but an electrometer; and an electrometer of small capacity was specially constructed for these experiments, with the aid of the Government grant dispensed by the Royal Society.

It is not desirable to allow the light to generate *currents*: the electrical charges must be allowed to flow back into the cell, so that it may not be temporarily deteriorated during the observations. Hence the preference for the electrometer.

The space at my disposal will not allow of my entering into many details; but I may mention, in particular, the importance of having the whole of the sensitive surface in the cell covered by the light of the star. It matters not to the value of the E.M.F. produced how far behind the focal image of the star the sensitive surface, A, is placed—provided that the image of the star just covers the surface A. This is essential in all photoelectric cells, and also in thermopiles; and the neglect of this condition may partly explain the failure of attempts to obtain thermoelectric indications from the stars and planets, although we should scarcely expect success from methods which aim at measuring merely a very limited portion of the radiation (viz. the heat, or infra-red). The photoelectric cell integrates the whole energy of the radiation on the sensitive surface; and the *square* of the observed E.M.F. is the measure of this incident energy.

It is interesting to know how the *photelectric* measures, so far as they have gone, compare with the *photometric* measures of "magnitudes" hitherto employed by astronomers. In the latter, if B and B′ are the "brightnesses" of two stars of the magnitudes m and m' respectively, we have by definition

$$\log_{10}\frac{B}{B'} = \frac{4}{10}(m' - m) \quad \ldots\ldots \quad (1)$$

This equation defines merely the difference of the magnitudes, and the definition is quite arbitrary. The essential things are B and B′. How are they measured? The photoelectric method says that they are E^2 and E'^2, the squares of the electromotive forces generated in a given cell by the lights of the two stars. The photometric method says that they are measured by the thicknesses of certain interposed glass prisms which extinguish the lights, or by polarising apparatus which render the shades of the transmitted lights "equal." Hence we may expect, perhaps, a fair amount of agreement between the two methods, if we are comparing two or more stars of the same colour. Thus, in the photoelectric method, we have for any two stars

$$m' - m = 5 \log_{10}\frac{E}{E'} \quad \ldots\ldots \quad (2)$$

Applying this to Arcturus and Regulus, and taking the magnitude of the former as ·2, we find the magnitude of Regulus to be 1·33. In Miss Clerke's "System of the Stars" (Appendix), Regulus is quoted as 1·4, Arcturus being ·2.

Comparing in the same way Procyon and Regulus, the latter being taken as of magnitude 1·33, the magnitude of Procyon would be ·46. Miss Clerke quotes Procyon as of magnitude ·5.

But no agreement between the two methods is to be expected when two stars of different colours are compared. The photometric method of equalisation seems to be just as meaningless as the ordinary "grease spot" method of attempting to equalise a blue and a red light! In this case the only intelligible comparison of two lights consists in measuring the energies which they radiate per unit time per unit area at a given distance—just, for example, as Newton's Second Axiom defines two masses to be "equal" when the same force produces the same acceleration in both; an equality which is *real* if the substratum at the basis of all bodies is the same, but merely *conventional* if it is not.

If the distance of a star is known, we can determine its intrinsic energy, *i.e.* the quantity of energy which it radiates into all space per unit time.

Thus, let I be the intrinsic energy of a star whose distance from the earth is R; let E be the electromotive force of its light as measured by the cell; let i, r, e be the analogous quantities for a candle or any other chosen source of light; and let A and a be the areas of the aperture of the telescope and the selenium surface in the cell. Then we have

$$\frac{I}{i} = \frac{R^2 . E^2 . a}{r^2 . e^2 . A} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let us take, for example, a result which Prof. Boys recently told me that he had obtained. He found, in conjunction with Mr. Watson, of South Kensington, that if the light of a standard candle was observed across a valley and almost in the line of sight of Arcturus, the light of the candle and that of the star seemed to be equal when the candle was at a distance of five-eighths, or ·625, of a mile.

Now, let x be the distance at which the candle light seems to be as bright as that of the star. Then

$$\frac{I}{R^2} = \frac{i}{x^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

And if D and d are the diameters of the telescope aperture and the circular layer of selenium in the cell, we have from (3)

$$x = r\frac{eD}{Ed} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Put, now, $r = 9$ feet, $e = 10$, $E = 8·2$, $D = 24 \times 25$ millimetres, $d = 2$ m.m., as in our experiments, and we find

$x = 3300$ feet, nearly
$= ·62$ miles.

This agrees remarkably well with the observation of Prof. Boys. GEORGE M. MINCHIN.

FUNERAL OF PROFESSOR HUXLEY.

IN accordance with his own wish, the late Prof. Huxley was buried at the Marylebone Cemetery, Finchley, last Thursday afternoon. The coffin came up from Eastbourne in the morning, and the numerous mourners assembled at the cemetery to meet it. Wreaths from members of the family, and from friends and fellow workers of the great naturalist whose loss we mourn, covered the coffin. The Royal College of Science, with which Huxley was connected so many years, sent a large wreath, and there were also wreaths from Lady Hooker, Mrs. Tyndall, the members of the staff at the Royal Gardens, Kew, Mr. Herbert Spencer, Sir Henry Thompson, Sir Henry Roscoe, Messrs. Macmillan, and the Editor of NATURE, among others.

The funeral service was performed by the Rev. J. Llewelyn Davies, an old friend of Prof. Huxley's, now rector of Kirby Lonsdale, but formerly vicar of Marylebone, where he was for a long time Huxley's neighbour.

The family was represented by Mrs. Huxley, the two sons, Mr. Leonard Huxley and Mr. Henry Huxley, and three daughters, the Hon. Mrs. Collier, Mrs. Waller, and Mrs. Eckersley (the remaining daughter, Mrs. Roller, is in Switzerland with her husband, who is ill), Mrs. Heath (a niece), and two sons-in-law, the Hon. John Collier and Mr. F. W. Waller.

No announcements of the funeral were sent out, and the large number of distinguished men who attended, and the various learned Societies that sent representatives, did so on their own initiative. The Royal Society was officially represented by Lord Kelvin, Sir John Evans, Prof. Michael Foster, and Sir J. Lister, many of the Fellows also being present. The Geological Society was represented by Dr. Henry Woodward, Dr. Blanford, and Prof. Bonney. Dr. Frankland, Mr. Crookes, Dr. Thorpe, and Dr. Gladstone were the representatives of the Chemical Society. The mourners from the Royal College of Science included Prof. Rücker, Prof. Norman Lockyer, C.B., Prof. Tilden, Prof. Judd, C.B., Prof. W. C. Roberts-Austen, C.B., Prof. Howes, Prof. Farmer, Dr. Wynne, Mr. J. W. Rodger, and Mr. Woodward. Major-General Sir J. F. D. Donnelly, K.C.B., Major-General Festing, Captain Abney, C.B., Mr. T. Armstrong, Mr. F. R. Fowke, and Mr. A. S. Cole represented the Science and Art Department; Sir William Flower, K.C.B., Dr. A. Günther, Mr. George Murray, Mr. C. E. Fagan, Prof. Jeffrey Bell, and Mr. F. A. Bather, the Natural History Museum; Prof. Armstrong, Prof. S. P. Thompson, Prof. Perry, and Prof. Ayrton, the City and Guilds Institute; Mr. Stanley Boyd, Mr. H. F. Waterhouse, Mr. J. F. Pink, the Charing Cross Hospital Medical School; Mr. J. J. H. Teall, Mr. F. W. Rudler, and Mr. E. T. Newton, the Geological Survey. In addition to the Fellows of the Royal Society not included in the above, there were present Prof. E. Ray Lankester, Dr. Dallinger, Sir Joseph Hooker, K.C.B., General Strachey, Dr. Lauder Brunton, Dr. Sclater, Prof. Carey Foster, Prof. G. H. Darwin, Sir James Paget, Dr. Burney Yeo, Prof. H. Marshall Ward, Prof. Seeley, and Mr. F. Darwin. Among the other mourners were Mr. Walter Troughton, representing Mr. Herbert Spencer, who was prevented by illness from being present, Dr. T. K. Rose, Mr. W. Darwin, Mr. A. H. Heath, Mr. S. Highley, Mr. W. S. Stewart, Major-General Sir Richard Pollock and Mr. D. Pollock, Mr. Alma Tadema, Mr. W. E. H. Lecky, Mr. and Mrs. Humphry Ward, Mrs. Tyndall, Mrs. W. K. Clifford, Mr. Henry James, Mr. Mark Judge, Mr. H. Saunders, Dr. Semon, Mr. F. Macmillan, Mr. G. L. Craik, Mr. Clodd, Mr. G. Griffith, Lady Staveley Hill, Mr. Paynter Allen, Mr. John Boyes, Mr. Spencer Walpole, Mr. Woodd Smith, Dr. J. Johnson, Mr. James Hulme, Mr. Stanley Edwards, Dr. Glover, Mr. T. B. Windsor, the Rev. D. D. Jeremy, Dr. J. Malecki, Mr. J. Spiller and Mr. and Mrs. Briton Rivière.

The funeral was at first announced to take place at 3 o'clock, whereas the time fixed upon was 2·30. Owing to a delay in the train, a number of workers in science, from the Midlands and the North of England, did not arrive at the cemetery until the ceremony was over, and thus, to their deep regret, they were deprived of the melancholy satisfaction of being present when the remains of an esteemed master and friend were laid to rest.

The memory of Huxley will always be cherished among men of science, and it is imperative that there should be a permanent memorial of some kind to show the world how great is their regard for him. The memorial should be a truly national one, and not limited to any particular institution. We understand that the Dean of Westminster is willing that a tablet shall be erected in the Abbey if desired, and this is one of the forms which the memorial might take. Sir William Flower suggests another form, in a letter to the *Times* of Monday. He writes:

"In the great hall of our national Museum of Natural History the noble statue of Darwin will hand down to posterity the image of the man as he appeared to all who knew him in life. Near this will soon be placed another statue remarkable for the accuracy with which the striking personality of Owen is represented, as all who see it now at the Royal Academy Exhibition can testify.

Surely this group of the great naturalists of this country and this century must be completed by the one we have just lost, in some respects the greatest of the three. The statues of Pitt and Fox stand side by side in Westminster Abbey. Huxley and Owen, often divided in their lives, would here come together after death in the most appropriate place and amid the most appropriate surroundings."

What is now wanted is a representative committee to take the matter up; we are confident that an appeal for funds would meet with a ready response, and we are glad to know that steps are being taken in this direction. A circular signed by Dr. Foster and Sir William Flower has been issued, calling a meeting at the rooms of the Royal Society this afternoon.

NOTES.

THE meeting at which the Prince of Wales presided in St. James's Palace on Tuesday, ought to further the interests of the British School at Athens, in support of which it was held. A distinguished and representative company was present, among them being many well-known men of science. The Prince of Wales has concerned himself with the existence and welfare of the School from the time of its foundation in 1883, and we are glad to notice that in his remarks to the meeting he drew attention to the fact that the scantiness of the means provided was out of all proportion to the valuable archæological work carried on. The School only has a precarious annual income of £500, whereas the French School at Athens has an assured income of over £3000 a year, and the German School more than £2000 a year. Owing to this state of affairs, it is quite impossible for the British School to enter into competition with such undertakings as the explorations of the Germans at Olympia, the French at Delphi, the Americans at Argos, or the Greeks at Eleusis and Epidaurus. The sum required to bring England approximately into line with other nations is at least £1500 a year. Fortunately, as the Prince of Wales remarked at the meeting, there are hopeful signs that matters will soon be placed on a more satisfactory footing. A petition for support addressed to the late Government, met with a ready response; and before leaving office Sir William Harcourt took steps to use some portion of the public funds devoted to the encouragement of scientific investigation for the support of the School, and it is understood that the present Ministers are willing to confirm the action of their predecessors. One of the colleges at Cambridge, which has been most severely tried through the agricultural depression, has generously made an annual appropriation out of its reduced funds, and three colleges at Oxford have voted annual grants. The public schools are also moving in the matter. The Prince of Wales suggested that perhaps some of our City Companies, whose funds are devoted not only to local charities, but which have extended their sphere to the support of educational and scientific institutions, may see their way to encourage research in Greece; and he hoped that our colonies, which are so intimately bound up with our own culture and our higher national aspirations, will recognise the fact that all the privileges of the Athens School are open to their qualified students, and will make some effort towards securing its adequate efficiency. Lastly, he appealed to the liberality of private individuals, and expressed himself convinced that the appeal would find a response throughout the country. Every year excavation, both in Greece and elsewhere, is becoming more important to science. The following resolutions, confirmatory of the object of the meeting, were carried unanimously:—(1) "That the British School at Athens has already done excellent work during the nine years of its existence, and is well deserving of increased support." (2) "That this meeting pledges itself to use every effort to place the School upon a sound financial basis, so that in point of dignity and efficiency it may worthily represent this country among the other foreign institutes in Athens."

PROF. CURTIUS, of the University at Kiel, has been appointed successor to the late Prof. Lothair von Meyer at Tübingen.

PROF. DANIEL C. EATON, well known in botanical circles by his work on ferns, has just died at New Haven, U.S.

WE learn that M. J. Deby, one of the leading authorities on diatoms, whose magnificent collection was recently acquired by the British Museum, is dead. He was in his seventieth year, having been born at Lacken, in Belgium, in 1826.

To the list of honours given last week should have been added Sir Bernhard Samuelson, M.P., F.R.S., who has been made a Privy Councillor, and Dr. H. D. Littlejohn, who has been made a knight. On Thursday last, Mr. Thornley Stoker, President of the Royal College of Surgeons in Ireland, and Dr. Christopher Nixon, were knighted by the Lord-Lieutenant of Ireland.

THE date of the annual meeting of the Society of Chemical Industry, which is this year to be held in Leeds, has been postponed from July 17 to July 31, in consequence of the General Election. It is not thought that any material change will have to be made in the programme.

PROF. SCHWARZ has been elected a Correspondant of the Paris Academy, in the Section of Geometry; Baron von Müller has been elected to the late Prof. Pringsheim's place in the Section of Botany, and Prof. Engelmann succeeds Ludwig in the Section of Medicine and Surgery.

WE are glad to be able to announce that the Italian Meteorological Society, which was temporarily dissolved after the death of Padre Denza, has again been reorganised, under the presidency of Count Vigodarzere, who is the proprietor of an observatory at Fontaniva. The central observatory will be at Moncalieri, as before, and we look forward to a continuation of the useful work carried on formerly by the Society.

WE are informed that King's College, London, will open next October a department for training teachers for Secondary Schools. There will be a two-years' course of technical studies combined with the preparation for the B.A. degree of the University of London. Detailed instruction in the art of teaching particular subjects will be given by the Professors of the College. Six Exhibitions of £15 are offered. Names of students should be sent in before September 16.

A REUTER correspondent at St. John's reports that the steamer *Kite* left there for Greenland on Tuesday to bring home the Peary Arctic Expedition. It is expected to return on October 1. The party on board includes Prof. Salisbury, of Chicago University, who goes to study the glaciers and geology of the region; Prof. Dyche, of the State University, Kansas, who will collect specimens of the fauna and flora; and Mr. Boutillier, of Philadelphia, who represents the Geographical Society.

THE influence of the Royal Gardens at Kew is felt in widely different regions of the world, through the men who are trained at the Gardens and sent out to various Botanic Stations. Three new appointments of men who have benefited by the Kew training, are notified in the current Kew *Bulletin*; they are Mr. C. H. Humphries, who has been made Curator of the Botanic Station of Aburi, on the Gold Coast; Mr. J. C. Moore, who has been appointed Curator of the Botanic Station at St. Lucia, in the Windward Islands, West Indies; and Mr. H. McMillan, who goes as Head Gardener to the Royal Botanic Gardens at Peradeniya, Ceylon.

MR. A. B. BASSET has sent us a letter referring to the proposed changes in the size of the pages of the Royal Society's publications. He directs attention to chapter xii. section ii. of the Statutes of the Society, empowering any six Fellows to convene a special general meeting, and suggests that such a meeting should be summoned, and the following resolutions submitted to it: (1) That this meeting is of opinion that the present form of publishing the *Transactions* should be continued. (2) That this meeting is of opinion that the present form of publishing the *Proceedings* should be continued. The resolutions are drawn up separately, so as to obtain the votes of Fellows who approve of a change being made in the form of one kind of publication, but disapprove of any change as regards the other.

IN the recent death of Prof. Verneuil, France has lost one of her most eminent surgeons. His name is intimately connected with the history of contemporary surgery. At first, Assistant of Anatomy, Prosector, as well as Professor of Anatomy to the Faculty of Medicine, he devoted himself to anatomical and physiological studies, and left his mark by important works, chiefly on the heart, and on the anatomy and physiology of the venous system. Later, he formed part of that noted phalanx which, under the auspices of Lebert, with Robin, Broca, Follin, introduced histological studies into France. From this time date a series of original memoirs, notably on the demoid cysts of the face, and on the scrotal enclosure, in which he expounded new views, and established the scientific theory which is now generally adopted. Later still, when hospital surgeon and professor in the Faculty of Medicine, he introduced important methods of operation. Animated by the most ardent love of science, he knew how to communicate his enthusiasm to those around him; he had all the requisite qualities of a founder of a school. His activity showed itself by a great number of communications to learned societies of which he was a member.

THE extensive science laboratories and buildings recently opened at Lille are described in detail in the *Revue Générale des Sciences*. The buildings comprise a physical institute, an institute of natural science, and an institute of chemistry, erected at a cost of £65,000. The cost of the whole work was nearly £140,000, and this has been borne by the Municipal Council and the Academy at Lille, assisted by a gift of £4000 from M. Philippart. The town of Lille has guaranteed an annual grant of £800 for twenty years, to be used in the interests of higher education, and has shown the greatest interest in the work of the new institute. The department of chemistry is divided into two parts, in which general chemistry and applied chemistry are respectively dealt with; and in each section laboratories are provided for research as well as for instruction. The physical department occupies a separate building, in which accommodation is provided for experiments of extreme delicacy as well as routine work. On account of the great stability now demanded by many physical investigations, all the research laboratories are on the ground floor; for the same reason, numerous large isolated pillars of masonry have been provided, and strong slate slabs have been fixed into the corners of the laboratories. The natural science building provides accommodation for geology, zoology, and botany; and a room is reserved for the Geological Society of the North of France. Every facility for study under good conditions appears to be offered by the new laboratories, and higher education in France will derive benefit from the increased opportunities now offered it at Lille.

THE third International Agricultural Congress will take place at Brussels from September 8 to 16; hence it will clash with the meeting of the British Association at Ipswich, which begins on September 11. The Congress will be held under the patronage of the King of the Belgians, and embraces twelve sections. In the section of agricultural education the subjects for discussion include rural schools, fields for experiment and demonstration, the possibility of devising an international programme of superior agricultural study, and the professional training of farmers' sons by interchange of the young people of different districts. The section of agricultural science will embrace chemistry and physiology as applied to agriculture; the utilisation and conservation of natural manures; agricultural meteorology; experiment stations and laboratories of control for manures, foods, and seeds. The third, fourth, and fifth sections deal respectively with co-operation, legislation, and currency. The section of animal production will discuss practical questions relating to stock-breeding, selection and crossing, the improvement of breeds, and the feeding of stock in times of drought. The veterinary section will concern itself with the organisation of veterinary sanitary police and the contagious diseases of animals, including pleuro-pneumonia, anthrax, and tuberculosis. The section of plant production is to discuss the selection of seed, the cultivation of malting barley, "sidération," the cultivation of peaty and mossy soils, drainage, and irrigation. The ninth section—southern agriculture and colonisation—embraces grape and silk culture; the cultivation of flowers for perfume, of oil-yielding plants, and of coffee, tea, and sugar-cane; the agriculture of the Congo and of Tunis; and the conditions of countries to which emigrants might be sent. The tenth section takes in forest economy, the eleventh deals with pisciculture, and the twelfth with agricultural industries, such as dairying, brewing, and bee and poultry culture.

THE results of a competition organised at Paris last month, by the *Petit Journal*, are of some scientific interest. Sixty thousand carrier pigeons from all parts of France, and from some places in Belgium, were released from the Eiffel Tower at known intervals and times. The first pigeon travelled a distance of 150 kilometres (93½ miles), with a velocity of 76 kilometres (47 miles) per hour. The highest average rates of flight ranged between this and 43 miles per hour for a distance of 264 miles. These rates are low compared with previous records. A distance of 600 miles has been covered at an average rate of 50 miles an hour, and in June 1860, a pigeon travelled from Blois to Dijon, a distance of 290 miles, in 4h. 46m., which gives a rate of about 60 miles per hour. There is also evidence that much higher average velocities than these have been reached.

DR. J. HANN, Secretary of the Vienna Academy of Sciences, laid before it, on the 20th ult., an investigation on the daily range of the barometer on clear and cloudy days, especially on mountain summits. It was known that at ordinary stations the daily barometric range in clear and cloudy weather only exhibited a difference in the single daily oscillation, while the double daily oscillation remained unchanged. But a similar investigation for mountain stations had not yet been made. With this object the author undertook the tedious operation of calculating the daily barometric range at a number of mountain stations for the summer season, and found that at these the double daily oscillation remained the same in both kinds of weather. At the earth's surface the daily curve showed a much greater amplitude in clear than in cloudy weather, and a totally different epoch. The average form of the daily curve for the mountain stations is represented by the formula: $0{\cdot}48 \sin (353^\circ + x)$ on clear days, and $0{\cdot}26 \sin (101^\circ + x)$ on cloudy days. On clear days the maximum of the single daily oscillation occurs at 6h. 30m. a.m., while on cloudy days it occurs at 11h. p.m. The author also found that the differences in the daily range on clear and cloudy days corresponded entirely to the differences which exist over the land, as compared with those over adjacent seas.

A RETURN has been issued showing the number of licensed experiments performed on living animals during 1894. The total number of persons holding licences during the year was 185, and of these 56 performed no experiments. The tables given afford evidence that licences and certificates have been granted and allowed only upon the recommendation of persons of high scientific standing, and that the licencees are persons who, by their training and education, are fitted to undertake experimental work and to profit by it. All the experimental work has been conducted in suitable places; the number of experiments performed was 3104. In more than one-third of these the animal suffered no pain, because complete anæsthesia was maintained from before the commencement of the experiment until the animal was killed. More than fifteen hundred of the remaining experiments were of the nature of hypodermic injections or inoculations. In about five hundred experiments the animal was anæsthetised during the operation, but was allowed to recover. These operations, in order to insure success, are necessarily done with as much care as are similar operations upon the human subject; and the wounds being dressed antiseptically, no pain results during the healing process.

THE Geologists' Association will visit the coast of Antrim and the Mourne Mountains this summer (July 29 to August 3). The programme includes the examination of sections in sedimentary rocks ranging from the Ordovician to the Chalk, pre-Devonian gneisses, and the basalts, rhyolites, and drusy granites of the Tertiary eruptive series. The illustrative papers by Messrs. McHenry and Lloyd Praeger will shortly be issued as a pamphlet, in advance of publication in the *Proceedings*. The country to be visited is classic, and additional interest is added to it by the recent publication of two papers in the *Geological Magazine*. The first of these, in the June number, by Mr. McHenry of the Irish Geological Survey, describes valuable evidence as to the age of the trachyte (rhyolite) of the district. In a section at Templepatrick Quarry, the acid lava is seen, by the arrangement of its columnar and flow-structure, to have flowed over the surface of the Chalk, sweeping the overlying gravel before it, and piling it up against the denuded edge of a mass of basalt belonging to the *earlier* of the two basic series. As fragments of the trachyte occur elsewhere in gravels overlain by the *later* basalts, it may be said to be of "mid-basaltic" age. The second paper, in the July number, is by Prof. Cole, and deals with the nature of the acid rocks poured out from the Tardree volcano, which are said to equal in variety the better known rhyolites of Hungary.

THE numbers of the *Botanical Gazette* for May and June contain a translation, by Mr. G. J. Peirce, of Prof. Strasburger's paper on the "Development of Botany in Germany during the Nineteenth Century." In the latter number there is also a very instructive article, by Mr. J. M. Coulter, on the "Botanical Work of the American Government." At present four distinct divisions of botanical work are organised under the Department of Agriculture, although other divisions also do a certain amount of work that may fairly be called botanical. These four divisions are those of botany, vegetable physiology, and pathology, agrostology, and forestry. The Division of Botany, under the general supervision of Prof. F. V. Coville, of Cornell University, is engaged in strictly scientific work, such as the working out of local floras, the examination of seeds, investigation of weeds, &c. To this department the Government appropriates, during the present year, 33,800 dollars. The division of vegetable physiology and pathology (26,300 dollars) is concerned with investigations into the phenomena of the growth of plants, and into the diseases of cultivated plants. Its chief is Prof. B. T. Galloway, University of Missouri; but investigations on behalf of the department are carried on also at the following centres:—University of Nebraska, University of Michigan, University of Illinois, Kansas Agricultural College, University of Copenhagen. The function of the Division of Agrostology (15,000 dollars) is to deal with forage plants as well as grasses, to instruct and familiarise the people with the habits and uses of these plants, to conduct investigations relative to their natural history and adaptability to different soils and climates, to introduce promising native and foreign plants into cultivation, and to identify grasses and forage plants. Its chief is Prof. F. Lawson-Scribner. The Division of Forestry, under the charge of Mr. B. E. Fernow, has at present chiefly been occupied with the study of the character and value of different timbers.

THE current number of the *Journal de Physique* contains the second part of the paper, by M. P. Curie, on the magnetic properties of bodies at different temperatures (see NATURE, June 6, 1895, p. 134). The present paper deals with iron, nickel, and magnetite. In the case of iron, measurements have been made at temperatures between 20° C. and 1360° C., and for field strength of from 25 to 1350 C.G.S. units. The observations on nickel and magnetite were only made at temperatures above that at which the great change in the magnetic properties of these bodies takes place. The values obtained with iron up to about 756° C. agree with those previously obtained by Dr. Hopkinson. Above this temperature the author finds that the curves showing the relation between the intensity of magnetisation (I) and the strength of the field are straight lines passing through the origin for temperatures between 750° and 1280° F. decreases more and more slowly. At first (I) decreases to half its value for a rise of temperature of a few degrees, but between 950° and 1280° the susceptibility is almost a constant, only decreasing very little as the temperature rises. At a temperature of about 1280° the susceptibility suddenly increases by about 50 per cent., and then again gradually decreases up to 1365°. The author, with some hesitation, gives the following explanation of this behaviour:—"Up to a temperature of 860° iron behaves like any other paramagnetic body. At a temperature of about 860°, however, it begins to change into a second allotropic form, this transformation being complete at about 920°, and the iron remaining in this condition up to 1280°, and behaving like such a body as oxygen or palladium. Finally at 1280° the iron changes suddenly back to its first condition." The attractiveness of the above theory can only be appreciated by a study of the author's curves, for if the curve showing the connection between the logarithm of the susceptibility and the logarithm of the temperature is plotted, it is found that the curve between 750° and 860° would, if prolonged, form with the curve above 1280° a curve in all respects similar to the curves obtained in the case of nickel and magnetite. With nickel the author finds that the temperature of the magnetic transformation is about 340°. After this temperature the susceptibility is independent of the strength of the field, and decreases regularly and very rapidly as the temperature rises. In the case of magnetite the chief magnetic transformation takes place at a temperature of 535°. At temperatures between 550° and 1370° the susceptibility is independent of the strength of the field, and decreases regularly, and between 850° and 1360° varies inversely as the absolute temperature. The value of K (see previous note, *loc. cit.*) being given by the expression $K = \frac{0.0280}{T}$ where T is the absolute temperature. From the differences exhibited by the behaviour with change of temperature of diamagnetic and paramagnetic bodies, the author considers that these two properties must be attributed to different causes.

LAST week the *Pharmaceutical Journal* began the first of a new and enlarged series (the fourth). The journal, which is now in its fifty-fifth year, has done much to promote pharmaceutical organisation and progress.

THE second part of the Report of the International Meteorological Congress held at Chicago in 1893, has just come to us from the United States Department of Agriculture (Weather Bureau). The papers included in the Report were communicated to the sections of history and bibliography, agricultural meteorology, and atmospheric electricity and terrestrial magnetism. Part iii. will comprise climatology, instruments and methods of observation, and theoretical meteorology.

THE most important articles in the *Kew Bulletin* for April to July, are one on the various sugar-cane diseases in Barbadoes, one on maple sugar, containing information with regard to the growth of the sugar-maple in the United States; and one on anbury, club-root, or finger-and-toe, describing the mode in which this disease is produced in a number of species of Cruciferæ by the attacks of the parasite *Plasmodiophora Brassicæ*, and the best modes of counteracting it.

THE new quarterly number of the *Journal* of the Royal Agricultural Society contains a paper on "Cross-bred Sheep," by Mr. H. J. Elwes, in which many facts of biological interest are recorded. The value of a first cross between two pure breeds is insisted upon, whilst due importance is attached to the dangers which beset the breeder should he venture beyond the first cross. Mr. Elwes is in a position to draw upon the results of long practical experience in the cross-breeding of sheep. The general improvement which the sheep of this country have undergone within recent years is attributed to the increasing resort to the services of pure-bred sires, but much remains to be done by those breeders who possess the necessary skill, patience, and energy. Another paper of scientific interest is one by Prof. G. T. Brown, C.B., on "Ringworm of Calves," which is illustrated with five original drawings. It is demonstrated that the living spores of the fungus of ringworm may be transmitted from one animal to another by means of lice. Prof. Edgar M. Crookshank contributes a popular paper on "Microbes in Health and Disease," and economic botanists will find much that is interesting in Mr. Glenny's paper on "The Onion and its Cultivation." This issue also contains a schedule of such native wild birds as are "undoubtedly beneficial to agriculture." Altogether, 38 species are enumerated, and details are given concerning their food, nests, and eggs.

THE additions to the Zoological Society's Gardens during the past week include an Anubis Baboon (*Cynocephalus anubis*, ♂), a Leopard (*Felis pardus*), two Two-spotted Paradoxures (*Nandinia binotata*), a Sharpe's Wood Owl (*Syrnium nuchale*) from Accra, Gold Coast, presented by Mr. W. H. Adams; two Red-crested Cardinals (*Paroaria cucullata*) from South America, presented by Dr. G. Fielding Blandford; a Small Hill Mynah (*Gracula religiosa*) from India, presented by Mr. W. Norbury; a Brown Capuchin (*Cebus fatuellus*) from Brazil, presented by Mr. W. E. Gibbs; a Spiny-tailed Monitor (*Varanus acanthurus*) from Roebuck Bay, West Australia, presented by Mr. Saville Kent; a Campbell's Monkey (*Cercopithecus campbelli*) from West Africa, an Egyptian Uromastix (*Uromastix spinipes*) from Egypt, deposited; two Mantchurian Cranes (*Grus viridirostris*) from North China, purchased; two Mule Deer (*Cariacus macrotis*), a Japanese Deer (*Cervus sika*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

SHORT-PERIOD VARIABLE STARS.—The recent spectroscopic observations of δ Cephei by Belopolsky (NATURE, vol. li. p. 282), and of β Lyræ by Pickering, Lockyer, and others, have shown that we have still a great deal to learn as to the cause of the light-changes in variables of short period other than those of the Algol type. In these inquiries, it has become clear that a study of the light-curves must go hand-in-hand with that of the spectroscopic changes, and we therefore welcome the publication, by Dr. Schur, of new light-curves of δ Cephei, η Aquilæ, and β Lyræ (*Ast. Nach.* 3282–83). The observations were made at Strassburg in the years 1877–85 by Argelander's method, an opera-glass providing the requisite optical aid.

In the case of δ Cephei, the observations and light-curve agree very well on the whole with those of Argelander and Schönfeld, but the interval from minimum to maximum is reduced by Dr. Schur from 1d. 14·6h. to 1d. 13·7h., and the period derived is 5d. 8h. 47m. 38·947s., or 1·027s. less than that of Argelander. There does not seem to be any ground for the idea that the length of the period is sensibly changing. Dr. Schur also obtained distinct evidence of a standstill in the light-curve in the descent to minimum. The period arrived at for η Aquilæ is about 4s. less than that of Argelander, namely, 7d. 4h. 13m. 59·318s. A very decided "hump" is shown on the descending side of the light-curve; this is not merely a halt like that in the case of δ Cephei, but an actual increase of light, commencing about 3d. 20h. after minimum, and reaching an abortive maximum about twelve hours later. The interval from minimum to maximum is 2d. 6h. The observations of β Lyræ give a light-curve of which the general form is almost identical with that given by Argelander, but the agreement of individual minimum with calculated times is not very good. To bring these into better agreement, Argelander's formula is corrected to the following: Epoch 424, Bonn mean time, 1855 Jan. 6, 15h. 28m. + 12d. 21h. 47m. 23s.·72 E + 0s. 315938 E^2 – 0·0000121 1s. E^3.

The paper gives full details of the observations and their reduction, and its value is increased by a plate showing the forms of the light-curves of the three variables in question.

THE NICE OBSERVATORY.—Vol. iv. of the *Annales* of the Nice Observatory is a monument to the industry of the director and staff of the magnificent observatory founded by M. Bischoffsheim. M. Perrotin, the director, contributes an elaborate investigation of the inequalities of the first order in the elements of Vesta, produced by the action of Jupiter, employing interpolation methods. M. Javelle furnishes full particulars of 505 new nebulæ discovered by him during 1890 and 1891 with the great equatorial of 15 inches aperture. The positions of these were determined by micrometric measures of distances from comparison stars, and awaiting accurate meridian observations of these, provisional positions for 1860 have been computed. Some of these objects are easily visible, but the majority of them are rather difficult, and others are at the limit of visibility of the Nice refractor. Star clusters have been rigorously excluded from the catalogue.

The meridian work at the observatory is particularly directed to the double stars of the Dorpat catalogue, and the already numerous stars which have been used as comparisons in the observations made with the equatorial. The period covered by the present publication is 1888 April 5 to 1889 December 23.

From May 1887 to December 1892, 26 new minor planets were discovered at Nice by M. Charlois, the last 11 by photography. A vast number of observations of these and other minor planets have also been made by M. Charlois, full details of which are recorded in the present volume. Observations of 19 comets are also included.

FOUCAULT'S PENDULUM EXPERIMENT.—The experimental demonstration of the earth's rotation, devised by Foucault in 1851, has recently been repeated at the De La Salle Training College, Waterford, on a somewhat smaller scale than in the original experiment. The weight of the pendulum bob was 19 lbs., and it was suspended by a wire 37 feet 6 inches in length. To set the pendulum in vibration, the usual method of burning the string by which the bob is tethered was employed. Thirty-three observations of the hourly motion of the pendulum plane were made during February and March of the present year, and the mean result was 11° 48′, the calculated value being 11° 53′ 37″. The time of the earth's rotation, or length of the sidereal day, thus deduced is 24h. 7m. 30s., an amount only about 11m. in excess of the true time. Foucault's observations gave 23h. 33m. 57s. as the time of rotation. Particulars of the Waterford experiment, and an explanation of the principles involved, are given by Dr. M. F. O'Reilly in *Engineering*, July 5.

THE SUN'S PLACE IN NATURE.[1]

VIII.

TWO objections, however, have been made to these hypothetical two swarms. It has been urged that the secondary swarm which we saw moving in a closed orbit round the primary one would soon spread out into a line along the orbit, so that there would always be some parts of it mixed up with the constituents of the parent swarm. That is a perfectly fair objection, supposing we are dealing with millions and billions of years, but I think that those who have made it do not know the history of astronomy. Let us take, for instance, the history of the November swarm which cuts the earth's orbit, so that in certain Novembers, generally about thirty-three years apart, we get this swarm of meteorites passing through our atmosphere, getting burnt out in that passage, and giving us one of the most magnificent sights which it is possible for mortals to see—a whole hemisphere of sky filled with shooting stars. Some of you may remember such a phenomenon as that in the year 1866, some of us are hoping to see the recurrence of it in 1899, for which we have not long to wait. But the fact that we only get this appearance every thirty-three years shows that, at all events, that swarm of meteorites to which the phenomena are due has not changed during our life-time—nay, it has not changed during the last thousand years, for man has known of that November swarm for more than a thousand years, and we have only known of the variability of Mira for 300 years; so that you see such an objection as that is entirely out of court, because it lacks the historical touch.

Another objection which has been urged is that there are certain irregularities in the light-curves of these bodies; that Mira, for instance, does not always come up to the same amount of brightness at its maximum, and perhaps, for all we know, does not always go down to the same low magnitude when it is at its lowest. That also is perfectly true, and on this account: there is no reason why we should suppose that these phenomena of the waxing and waning light of the body are produced by the movement of one body only; suppose, for instance, that there is some cosmic eye a billion miles away from our solar system, so beautifully and exquisitely wrought, so delicate in its construction, that it can see an increase in the light of the sun every time a big comet goes round it. Now we know from our own experience of comets that it would be absolutely impossible for that delicately constructed eye to see anything like a constant variability in the light of the sun under these conditions, because sometimes the brightest comets which come to us are absolutely unpredicted, they come at irregular times. It must also be pointed out in connection with this objection that there are other obvious causes for considerable variations in the light, both at the maximum and at the minimum. You remember that I showed you those beautiful spiral nebulæ of which Dr. Roberts has given us such magnificent photographs; suppose them to represent the parent swarms, and that another minor swarm tries to pass them; it is impossible to imagine that the minor swarm would exactly pass through all the intricacies of those magnificent spirals, and go and come through it precisely on the same path. It would be certain that in consequence of perturbations, the secondary swarm would sometimes go through a denser portion, at other times through a less dense portion, and then you see that would be quite sufficient to give us a considerable difference of luminosity.

I have another interesting series of diagrams, which will show you that almost any amount of variability and *irregular* variability in the light curves of these bodies may be explained on very simple grounds, supposing we acknowledge that we are dealing with the movements of more than two bodies. For instance, suppose we have one cause at work which gives us a maximum and minimum, and another cause which gives us two very much smaller maxima and minima occurring at a different period represented in Fig. 34 in the upper part of the diagram.

If we add these two together, we get the irregular light curve shown below the two simple curves in the diagram. But the amount of irregularity may possibly only reveal the amount of our ignorance, and when the time comes when we can isolate these two causes, and thus see how the addition of them should be made, we shall find that every part of this curve is really the result of a most beautiful law. I am very glad to say that quite recently Mr. Maxwell Read, of the Harvard Observatory, has put forward this very same suggestion, so that we may hope that it will soon be worked out on pretty broad lines.

But suppose for a moment that this view of two bodies is not accepted. What have we got in place of it? Well, we have to explain all the phenomena of variability by one body. That has been attempted more or less happily. Suppose, for instance, we have the case of a body waxing and waning quite regularly; you have only to say that body is like a soup-plate, and rotates on an axis, so that sometimes you see the face, sometimes only the edge. But that is not very satisfactory, because we do not know any stars which are like soup-plates. Another way is to say that the stars which are variable have great dark patches on one side of them, great bright patches on the other. Well, of course you can get a variation of light by such a scheme as that; but we have not observed that, we are simply inventing, merely suggesting ideas to nature that I fancy nature will tell us by and by are quite erroneous. For instance, I have shown you the facts with regard to β Lyræ. What is the explanation put forward for the variability of that star? Simply this, that it is a surface of revolution, the ratio

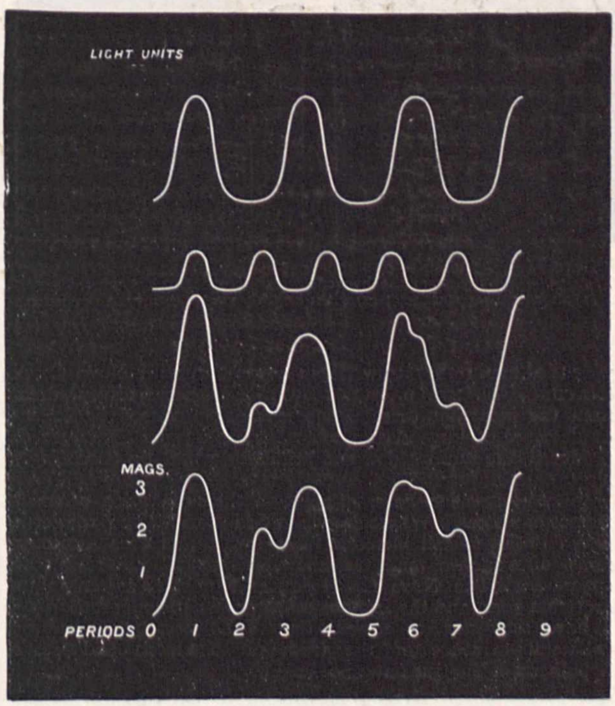


FIG. 34.—Indicating how apparently irregular light-curves may be due to the summation of two regular light variations.

of the axes being 5 to 3, *i.e.* elliptic beyond any experience of ours with regard to any other bodies; there is a dark portion at one end of the axis symmetrically situated. This thing then has to turn and twist with its axes and the black spot, and so on, and at the end of the chapter you are to have such a light curve as that of β Lyræ. That you see is blown into thin air by the spectral facts. I think you will acknowledge that these things are irrational, because they have no true basis of fact, and we must remember that in all this work we must deal strictly with the facts in accordance with the rules of philosophising; *i.e.* we must never have a complicated explanation until we are perfectly certain that a simpler explanation will not do, and the simplest explanation of all is that which occurs most frequently in the region of facts. That puts the soup-plate theory with regard to variable stars entirely out of court. Further, remember that supposing those gentlemen who still hold to the one-body theory, one star, one variability, object to the possible explanation of variability by the meteoritic hypothesis, they will find it very much more difficult to explain the departure from regularity by any geometric system, because a geometric system must certainly be

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 207).

more rigid than any other, and therefore any irregularity under it would be almost impossible.

Closely associated with this reference to double swarms in the case of variable stars are the phenomena of so-called "new stars." Indeed, the whole conception of the meteoritic hypothesis arose from a consideration of those bodies which sometimes quite suddenly make their appearance in the heavens. We have had during the last thirty years five of these new stars, and it was during the appearance of one in the constellation Cygnus in 1876 that I was led to the views which I still hold with regard to their origin.

One of the most remarkable features of these new stars is the rapidity with which they lose their brilliancy, and it was this

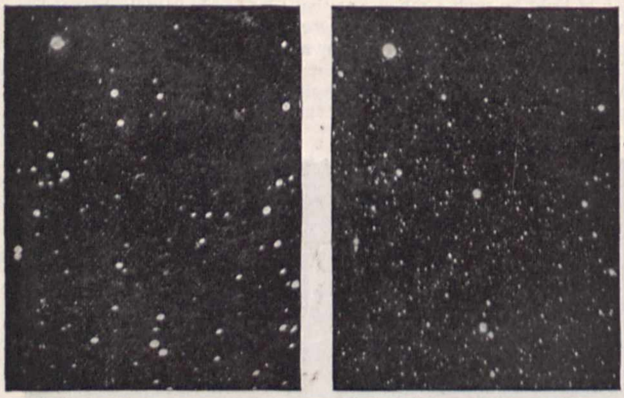


Fig. 35.—The region in the heavens where Nova Aurigæ was observed (1) after its disappearance; (2) when brightly visible (nearly in the centre).

which led me in 1877 to write, in connection with Nova Cygni (Nature, vol. xvi. p. 413, 1877): "We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceedingly slow.

"Let us consider the case, then, on the supposition of small masses of matter. Where are we to find them? The answer is easy: in those small meteoric masses which, an ever-increasing mass of evidence tends to show, occupy all the realms of space. . . . The Nova now exists as a nebula, so far as its spectrum goes, and the fact not only goes far to support the view I have suggested, as against that of Zöllner, but it affords collateral evidence of the truth of Thomson and Tait's hypothesis of the true nature of nebulæ."

we get the greater light formed at the moment when two swarms, one revolving round the other, are nearest together.

Fortunately for science, one of these new stars appeared in 1892; it is known as Nova Aurigæ, and two photographs will give us an idea of the sort of thing which an astronomer sees in the heavens when the discovery of a new star is announced. The photographs show a portion of the constellation of Auriga, and a star which is very clearly seen in the photograph taken very soon after this star had burst upon us, is absent from one taken a few months later.

Since the spectroscope was first applied to the stars, five new stars have been observed and spectroscopically examined. One appeared in Corona Borealis in 1866, one in Cygnus in 1876, and one in Andromeda in 1885; then came the one in Auriga in 1892, to which reference has already been made, and last of all was one in the southern hemisphere, discovered in 1893. The first three of these were observed by eye only, but in the two recent ones we have the immense benefit of photographic records.

It was therefore a very interesting point when a new star came along, to see whether there was any additional light thrown by it upon the problem of two bodies; and further, upon one of the points in which, if the meteoritic hypothesis failed, it was worth absolutely nothing at all. If there was any truth in the idea of the light of these bodies being produced by the clash of meteor-swarms, when the clash was over the swarms should go back into their native obscurity, or condition of low temperature, and should, if they were seen at all, put on the spectrum of sparse swarms in other parts of the sky; that is, they should put on the spectrum of a nebula.

That, you see, was a very crucial point; it was a point which could be settled by the spectroscope, provided always we had one of these marvellous bodies at such a distance from us that we could still observe it spectroscopically, and see what the different changes really amounted to.

Already in the case of Nova Cygni, the spectrum had been observed to change from a rather complicated one of bright lines and flutings to a very simple one, similar to that of a planetary nebula. The observations did not, however, furnish any direct evidence that more than a single body was concerned in the outburst.

The appearance of Nova Aurigæ, however, furnished a splendid opportunity of testing the many theories which have been at various times advanced to account for the phenomena. This Nova was discovered at Edinburgh by Dr. Anderson, who was modest enough to announce his discovery by sending an anonymous post-card to Dr. Copeland, the Astronomer Royal for Scotland, on February 1, 1892. It was then a star of the fifth magnitude, and on confirming the true nature of the newly-discovered star by means of the spectroscope, Dr. Copeland made the news public. Information was received at most observatories on February 3, and on the same evening two photographs of the spectrum were taken at South Kensington. During the next two or three weeks the star fluctuated considerably in brightness, though being generally on the down grade; and by April 26 had fallen to the

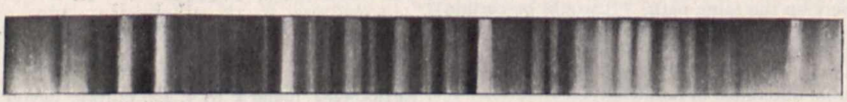


Fig. 36.—Photograph of the spectrum of Nova Aurigæ, taken at South Kensington, February 7, 1892.

Returning to the subject of new stars in 1887, in a general discussion of the meteoritic hypothesis, I saw no reason to change my views, and an inquiry into the spectroscopic phenomena led me to state that—"New stars, whether seen in connection with nebulæ or not, are produced by the clash of meteor swarms, the bright lines seen having low temperature lines of elements, the spectra of which are most brilliant at a low stage of heat."

A very detailed investigation of all the new stars which had been observed up to 1890 formed the subject of a communication to the Royal Society, and it was shown that the hypothesis would explain the fluctuations of light, the changes of colour, and the spectroscopic appearances.

To make a very long story short, it was suggested that the phenomena of new stars were produced by exactly the same cause as that which was at work in the variable stars in which

16th magnitude, so that it could only be picked up at all in the very largest telescopes. Thanks to the photographic records of the stars, it was possible to learn something of the earlier history of the new star. It had really been photographed by Prof. Pickering two months before its existence was known.

Fig. 36 shows us a photograph of the spectrum of this wonderful star itself, and it will be seen that in the case of all the chief lines we get a bright line and a dark line side by side. There are the hydrogen lines; that is, in the spectrum of that body we were dealing with the giving out of hydrogen, and the absorption of hydrogen. Now, the same set of particles cannot be producing bright and dark lines at the same time. We were then obviously dealing with two sets, and the first photograph, therefore, which was taken of the spectrum of this strange body, put beyond all question the fact that we were really dealing with two bodies, and not with one. That was very important; but you will see from the photograph, that it is very unlike the spectrum of nebulæ, so that it required a certain amount of faith when the spectrum was observed to be such as you see it here, to suppose that after a certain time, when the action which produced the greater luminosity was reduced and the light toned down, we should eventually get the spectrum of a nebula.

Well, as a matter of fact, the Nova reappeared in August 1892, and was observed to have increased in brightness from the 16th magnitude in April to about 9th magnitude. What, then, was the spectrum? It had almost completely changed; and among the first to observe the new spectrum was Prof. Campbell, of the Lick Observatory. This observer then stated that "the spectrum resembles that of the planetary nebulæ." In the following month the spectrum was also observed by Drs. Copeland and Lohse, and their observations seemed to them to "prove beyond doubt that Nova Aurigæ is now mainly shining as a luminous gas nebula." The most striking evidence on this point, however, is that afforded by the photographic investigations of Von Gothard. He not only shows us the photographic

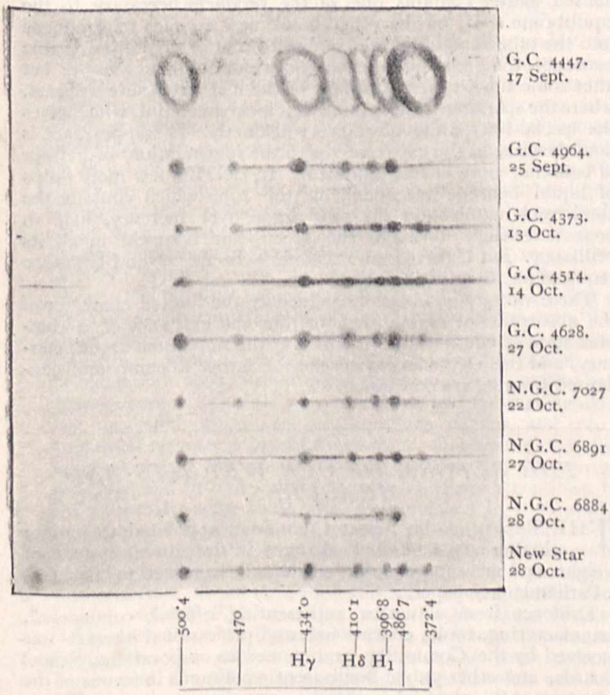


FIG. 37.—The spectrum of the new star in Auriga, as compared with the spectra of planetary nebulæ (Gothard).

spectrum of the new star at this stage of its history, but gives us also the spectra of several nebulæ to compare with it; and it is evident that we were certainly dealing, in the case of the Nova, with the same spectrum as in the nebulæ. Dr. Gothard, at least, was satisfied on this point, and stated that "the physical and chemical state of the new star resembles at present (September and October 1892) that of the planetary nebulæ."

So you see we get, first of all, the hard fact that the spectrum indicated the existence of two bodies; and then the very much harder fact for some, that, after the war was over, we got back to the condition of the nebulæ. I need not tell you that there is not universal agreement on this point, and chief among those who do not yet acknowledge it are Dr. and Mrs. Huggins. Writing of their observations of February 1893, they say: "We wish to speak at present with great reserve, as our knowledge of the Nova is very incomplete; but we do not regard the circumstance that the two groups of lines above described fall near the positions of the two principal nebular lines as sufficient to show any connection between the present physical state of the Nova and that of a nebula of the class which gives these lines."

But I may say, at all events, that I have the great authority of the names of Campbell, Copeland, and Gothard, who state that they have certainly observed the spectrum to be that of the nebulæ, and in reply to Dr. Huggins, Prof. Campbell says: "If the spectrum is not conceded to be nebular, I must ask what else we should expect to find in that spectrum, if it were nebular?" The answer to that is, that you would not expect to find anything else because it is all there already. In fact, out of nineteen lines observed or photographed by Prof. Campbell in the spectrum of the Nova, eighteen correspond perfectly with nebular lines. "Therefore the spectrum is nebular, and the fact that the lines have remained broad, or may have remained multiple, does not militate against the theory."

Further, there is even telescopic and photographic evidence of the fact that Nova Aurigæ became a nebula. Dr. Max Wolf's photographs of the Nova and its surroundings in 1893, resulted in the discovery of a number of new diffuse nebulæ in its vicinity, "and there even appeared to be traces of nebulous appendages proceeding from the star itself."

Another new star appeared in the southern constellation Norma in 1893. This was discovered on October 26, on a photograph taken at Arequipa, Peru, on July 10, 1893. Fortunately the photograph was one showing the spectra of stars instead of the simple images of the stars themselves, and the spectrum was seen to be identical with that of Nova Aurigæ. Even more important were the observations of Campbell in February and March 1894, when the star was about 10th magnitude. As the result of his work, he stated that "there can be no doubt that the spectrum of Nova Normæ is nebular."

J. NORMAN LOCKYER.

(*To be continued.*)

THE FLUORESCENCE OF ARGON, AND ITS COMBINATION WITH THE ELEMENTS OF BENZENE.[1]

M. BERTHELOT read the following paper, containing observations by M. Deslandres and himself, before a recent meeting of the Paris Academy of Sciences:—

I have thought it useful to study more closely the conditions of the combination with benzene under the influence of the silent discharge and those of the special fluorescence which accompanies it.

M. Deslandres, whose great competence in spectroscopic questions the Academy is well aware of, has been kind enough to help me in these new determinations, made with higher dispersion, and rigorously determined by photography. It is my duty to thank him here for this long and laborious work.

We must remember that the combination of argon with the elements of benzene, under the influence of the silent discharge, is a slow process; according to the present research, it is accomplished with the help of mercury, which intervenes under the form of a volatile compound. The use of very frequent discharges appears not to modify the general characters of the reaction.

At the beginning, nothing is seen in daylight, and it is only in a dark room that one perceives a feeble violet glow, similar, in its intensity, to that which the discharge develops generally in gaseous systems. At the end of an hour, when in a dark room, a green glimmer is seen, which occupies the middle of the interval between the spirals of the platinum band wound round the discharge tube, the luminous spectrum gives two yellow lines at λ 579 and 577, a green line at λ 546, and a green band at λ 516·5. These different lines will be defined by-and-by.

The photographic spectrum, taken during this time, with an hour's exposure, shows the principal bands of nitrogen, as well as a blue line λ 436, a violet line λ 405, and an ultra-violet line λ 354; these latter being more feeble than the bands of nitrogen.

During the following hours, the green glow constantly increases, the yellow lines and line λ 546 increase, and the band λ 516·5 diminishes. At the end of eight hours, the bands of nitrogen have almost entirely disappeared in the photograph; without doubt it is because the corresponding nitrogen has been absorbed by the benzene.

Seven additional hours of sparking bring the fluorescence to a brilliant emerald colour, visible in broad daylight; the intensity of this phenomenon, as I have already had occasion to say,

[1] Translated from *Comptes rendus*, June 24, pp. 1386–1390

not to be compared with the fluorescence developed by the discharge in any known gas. The yellow and green lines can be seen and measured in the spectroscope in full daylight.

The photographs give the following lines wave-lengths, λ λ 579, 577 and 546; 436, 405, 354, 313 and 312 (ultra-violet); one can see two violet lines besides, 420 and 416, scarcely visible, and the lines 385 and 358.

The spectrum observed at the end of fifteen hours was maintained during thirty consecutive hours.

Although advantage has been taken of photography for the registration of these phenomena, care must be taken not to confound such effects, observed in the daytime and under normal pressure, with the glows developed by the discharge in very rarefied gases, such as are generally observed in a spectroscope.

The meaning of these lines is as follows:

The line λ 579 is simply one of the lines visible in daylight, and under normal pressure, which I had described in *Comptes rendus* (t. cxx. p. 800), pointing out that it was probably double. The lines λ λ 580·1 and 577·1 described in the spectrum of rarefied argon, by Mr. Crookes (Jan. 24, 1895), must be compared with them.

Line λ 546 is also described (547) in my preceding note, and answers to a strong line 545·6 attributed to the spectrum of rarefied argon by Mr. Crookes. M. Deslandres has recognised the same lines in the spectrum of rarefied argon, which he had prepared by means of lithium. I have verified, by juxtaposition, the coincidence of the last line of rarefied argon with that of my tube.

I have also announced line 436, found again in the photograph, and very close to 434·5 of rarefied argon (Crookes). The lines 420 and 416 coincide with the very strong lines 420·1—419·8 and 415·96 of rarefied argon (Crookes). The line 405 can be identified with the line 404·4 of Crookes (argon). I have verified the coincidence. Line 385 coincides with Crookes' strong line 385·15 (argon). Line 354, with a group of strong lines at 354·7—353·4 of argon (Crookes). Line 358 with Crookes' group of strong lines 358·7—357·5 (argon).

λ 516·5 is a hydrocarbon band; 313 and 312 are the lines of the vapour of mercury vapour.

None of these lines, as I have already stated, coincide either with the line of helium (587·5) or with the principal line of the aurora borealis (557), although the latter is very near to a strong line of argon (555·7). If the actual fluorescence is not the same as that of the aurora borealis, still its development, and the nearness of the preceding lines, establish a probable relation between this meteor and the existence of argon in the atmosphere.

Here a very important circumstance presents itself. While examining the table of argon lines, given by Mr. Crookes, certain lines were seen to coincide with certain lines of the vapour of mercury. The same coincidence is found in the straight lines visible in daylight, under the normal pressure, in the fluorescence developed during the reaction of benzene on argon. Such are, according to M. Deslandres, the yellow lines 579 and 577; also the very characteristic green line 546, the blue line 436, the violet line 405, the ultra-violet line 354. On the contrary, the lines 420, 416, 385, 358, belong to argon only, the lines 313 and 312 to mercury.

M. Deslandres attributes the common lines to the presence of the vapour of mercury, either in rarefied argon, or in the fluorescent light obtained with benzene under normal pressure.

Nevertheless, as no known gas gives this fluorescence, or these lines, under normal pressure in operating with mercury, it is not possible to explain their production merely by the presence of this vapour alone. Otherwise it would not be understood why they did not show themselves in pure argon, in the presence of mercury under normal pressure, and that they would not produce themselves during the first moments of discharge, either with argon saturated with benzene, or with sulphide of carbon over mercury, or with nitrogen under the same conditions, where it combines with benzene and sulphide of carbon. On the contrary, with argon saturated with benzene, they develop themselves only at the end of several hours, and after the progressive transformation of the benzene into a series of compounds more and more condensed. It is one of these compounds which, immediately it is formed, unites itself both with argon and mercury, associated perhaps by reason of their common character of monatomic molecules. The fluoresence begins when there still exists a notable quantity of liquid benzene in the tubes; it is then accompanied by a diminution of gaseous volume.

This fluorescence continues a very long time, even after there is no apparent benzene; at last the fluorescence ceases to be visible in the daylight, in consequence of the very prolonged action of the discharge, which at last extinguishes the green tint, and brings back this gaseous system to a glow analogous to that of ordinary gases, this happens doubtless in consequence of the destruction of the last traces of benzene (or the products of intermediate condensation), which maintained the equilibrium of the dissociation of the system.

Once the green fluorescence is well established, the compounds which develop it are stable by themselves; even after twelve hours' break, if the apparatus has not been disturbed, it suffices to pass the discharge, to see that the fluorescence re-establishes itself with all its brilliancy in an instant. But it ceases so soon as the electric current is stopped.

But if the gas is separated from the condensed matter, the phenomenon cannot be immediately produced, either on the one or on the other. The gas alone, subjected to the action of the discharge, puts on almost immediately a special violet fluorescence, visible in darkness, and which generally precedes the development of the beautiful green fluorescence. Nevertheless this does not reproduce itself then, which seems to indicate that the condensed matter contains one of the products necessary to the equilibrium. If, on the other hand, new argon is reintroduced into the tube containing the condensed matter (free from visible benzene), the green fluorescence does not reproduce itself; but after some time, near the surface of the mercury, there appears, where the sparking is most intense, a local green tint, which gives the special lines, although not very distinctly. This appearance is doubtless due to the existence (or to the regeneration) of a trace of benzene, more or less modified. In fact, if a few more drops of liquid benzene are added in the tube which contains the condensed matter and the new argon over mercury, half an hour is enough to make the green tint reappear in all its brilliancy. But if there is an excess of benzene, several hours are required for its reappearance.

These various observations, added to the limited character of the absorption of argon, demonstrate the existence of a complex state of equilibrium, in which at the same time argon, mercury, and the elements of benzene, or rather a compound condensed from it, are concerned.

THE REFORM OF OUR WEIGHTS AND MEASURES.

THE Report of the Select Committee appointed to inquire whether any, and what, changes in the present system of weights and measures should be adopted, has been published as a Parliamentary paper.

Evidence from witnesses representing official, commercial, manufacturing, trade, educational, and professional interests was received by the Committee, and numerous corporations, School Boards, and other public bodies sent resolutions in favour of the adoption of the metrical system.

All the witnesses expressed a strong opinion as to the complicated and unsatisfactory condition of the present weights and measures in use, and of the distinct and serious drawback to British commence, especially in the foreign trade, which this system entails, differing as it does from the system (metrical) now adopted by almost every European nation, as well as by far the majority of non-European countries with which this kingdom trades. The evidence also showed that the home trade would be benefited if more simple and uniform standards of weights and measures than those now existing were adopted.

Moreover, strong evidence was brought forward as to the serious loss of time incurred by English school-children in having to learn the complicated system of tables of existing weights and measures, and the urgent need of the adoption of a simpler system. It was stated that no less than one year's school time would be saved if the metrical system were taught in place of that now in use.

Evidence from competent witnesses proved to the satisfaction of the Committee that a compulsory change from an old and complicated system to the metrical had taken place in Germany, Norway and Sweden, Switzerland, Italy, and many other European countries without serious opposition or inconvenience; that this change was carried out in a comparatively short period; and that as soon as the simple character of the new system was understood it was appreciated by all classes of the population, and no attempt to use the old units or to return to the old system was made.

In the United States, where a system founded on the English units exists, a Commission is at present engaged in an investigation of the same character as that with which the Committee was charged, and the Federal Government has this year passed an Act rendering the metrical system compulsory for pharmaceutical purposes.

The Committee believes that the adoption of the metrical system by England would greatly tend to render that system universal.

It is recommended :—

(*a*) That the metrical system of weights and measures be at once legalised for all purposes.

(*b*) That after a lapse of two years the metrical system be rendered compulsory by Act of Parliament.

(*c*) That the metrical system of weights and measures be taught in all public elementary schools as a necessary and integral part of arithmetic, and that decimals be introduced at an earlier period of the school curriculum than is the case at present.

SCIENCE IN THE MAGAZINES.

THIS month's *Contemporary Review* is remarkably rich in articles of scientific interest. Mr. Herbert Spencer's third paper on professional institutions deals with the "Dancer and Musician." So far back as 1857, Mr. Spencer showed that, excluding movements which are reflex and involuntary, muscular movements in general are originated by feelings in general. "As a consequence of this psycho-physical law, the violent muscular motions of the limbs which cause bounds and gesticulations, as well as those strong contractions of the pectoral and vocal muscles which produce shouting and laughter, become the natural language of great pleasure." From the ways in which children manifest their joy were evolved the expressions of elated feeling with which peoples meet their conquering chief or king, and eventually the natural displays of joy came "to be observances used on all public occasions as demonstrations of allegiance, while, simultaneously, the irregular jumpings and gesticulations with unrhythmical shouts and cries, at first arising without concert, gradually by repetition became regularised into the measured movements we know as dances, and into the organised utterances constituting songs. Once more, it is easy to see that out of the groups of subjects thus led into irregular ovations, and by-and-by into regular laudatory receptions, there will eventually arise some who, distinguished by their skill, are set apart as dancers and singers, and presently acquire the professional character." In support of this interpretation evidence obtained from many nations is adduced, and the separation and secularisation of the twin professions of dancing and music are traced. Mr. G. F. Scott-Elliot writes in the same review on "The Best Route to Uganda." He is in favour of a route following the line of the African lakes. The route enters the Zambesi at Chinde, and continues up the Zambesi and Lower Shiré as far as Chiromo, from whence a railway of approximately 120 miles would be required across the Shiré Highlands to Matope, from which point the Upper Shiré is navigable, and goods can be carried to the north end of Lake Nyassa. Here another railway would be required from Karonga to South Tanganika (240 miles). From the north end of Tanganika a line would run to Kagera. The Kagera river rises on the easterly flanks of the mountains to the east of Tanganika, and eventually falls into the Victoria Nyanza. A cataract is said to exist on the river, but even if this is so, and a length of line is required to avoid it, the cost of the whole line would only be about £1,700,000, or one-half that necessary for the Mombasa railway. Other considerations point clearly to the Lake route as the better of the two suggested lines. Prof. Lombroso contributes a paper on "Atavism and Evolution." He gives a number of instances of what he regards as atavistic phenomena in social life. "England," he says, "has succeeded in establishing a form of monarchy the most liberal in Europe ; and is working out without disturbance the aims of Socialism. But, at the same time, she not only maintains the privileges of her Peers, but actually dresses them up, as well as her judges, in the wigs and robes of the Normans ; and still uses, on ceremonial occasions, the language of her ancient conquerors. . . . Then this very positive and practical nation insists on retaining a system of weights, measures, and coins, which is opposed to that of all modern Europe, and is an obstacle both to commercial exchange and to scientific research." He classifies recent inventions which are shown to be old as evidence of atavism, and explains the duplication by the dislike with which, according to him, human nature regards novelties. Too rapid advance in the arts provokes reaction and causes the tide of progress to ebb when it should be flowing. A sensible article on the "Physiology of Recreation" is contributed by Mr. Charles Roberts, in the course of which he gives the following classification of physical recreations according to their physiological value. Outdoor: running, athletics, games, skating, skipping, &c. ; riding, rowing, swimming, walking, cycling, marching. Indoor : fencing and other military exercises with arms, boxing and wrestling, dancing, billiards, dumb-bells, machine gymnastics, trapeze and high gymnastics, singing and reading aloud, playing musical instruments. Recreations of a leisurely sort, physiologically considered, are :—Outdoor : natural history, gardening and farming, carpentry and other technical work. Indoor : reading ; chess, draughts, and cards ; music. Another paper in the *Contemporary*, entitled "The Origin of Man and the Religious Sentiment," by A. Fogazzaro, invites criticism from the standpoint of evolution.

Prof. Case, Professor of Moral and Metaphysical Philosophy in Oxford University, champions the cause "Against Oxford Degrees for Women," in the *Fortnightly*. He holds that the admission of women to University examinations has brought out the difficulties of teaching mixed classes, and that a mixed University is not desirable, especially at Oxford. Let women have facilities for higher education, by all means, thinks Prof. Case, but let these opportunities be afforded by a University especially founded for women. Mr. Grant Allen writes on "The Mystery of Birth," in the same review, the object of his article being to raise the question, "Is there any real and essential difference between the transmission of functionally-acquired modifications to offspring, and their registration or persistence in the individual organism?" Disciples of Weismann, and biologists generally, will be interested to know that Mr. Allen proposes "to throw back upon assimilation, in its widest sense, the burden of the mystery hitherto attached to the reproductive function."

The *Reliquary and Illustrated Archæologist* has among its articles one by Mr. H. W. Young, on the discovery of an ancient burial-place and a symbol-bearing slab at Easterton of Roseisle. A large number of flint instruments, such as arrow-heads, axes, scrapers, &c., found associated with the remains, make the discovery interesting and important, especially in relation to the geology of the "Laich of Moray."

Natural science predominates in *Science Progress* this month. The pathological results of the Royal Commission on Tuberculosis are discussed by Dr. Sidney Martin, and Mr. Arthur Keith uses Dr. Dubois' *Pithecanthropus Erectus* as a text for a helpful review of human fossil remains. The geology of the Sahara forms the subject of a paper by Mr. Philip Lake. As in July 1894, Mr. Chree shows, in an extremely valuable table, the recent values of the magnetic elements at the principal magnetic observatories of the world. In an article entitled "A Type of Palæozoic Plants," Mr. A. C. Seward directs attention to the histological structure and affinities of the genus *Calamites*, and finally Dr. W. D. Halliburton describes the formation of lymph.

Among the articles in *Knowledge*, we notice "The Sugar Cane," by Mr. C. A. Barber ; "Scorpions and their Antiquity," by Mr. Lydekker, illustrated by two fine pictures of the giant sand-scorpion of Namaqualand, reproduced from photographs, and "The Great Nubecula," by Mr. E. W. Maunder. There are also articles on the field of diameter of the field of view of a telescope, Dr. Roberts' photographs of star-clusters and nebulæ, the cause of earthquakes, and on Prof. Fraser's experiments to find a cure for snake-bites.

Blackwood's Magazine contains a paper in which Colonel Knollys dwells upon public school and Army competitive examinations. He holds that the imperfections of the training at our public schools, and the character of some of the examination papers, are responsible for the cramming now so common with candidates for the Army. Two other articles, in which our readers may be interested, are "Mountaineering Memories," by Mr. H. Preston Thomas, and "The Territorial Waters and Sea Fisheries."

A passing notice must suffice for the remaining articles of scientific interest in the magazines and reviews received by us. The *Century* has an article on "Picturing the Planets," by Prof. J. E. Keeler ; the article is illustrated by views of Jupiter, Mars, and Saturn, obtained at the Lick Observatory. To the *English Illustrated*, Mr. Grant Allen contributes another "Moorland Idyll"; and the inhabitants of "The Monkey House in the Zoo" are described and illustrated by Mr. F. Miller. In the *Humanitarian*, Mr. J. G. Raupert has a pseudo-scientific article upon "Some Results of Modern Psychical Research"; and in *Chambers's Journal*, there are articles worth reading on death from snake-bite in India, the Carstairs Electric Light Railway, and citric acid. Geographers will be interested in a paper on "England and France in the Nile Valley," contributed by Captain F. D. Lugard to the *National*. Here we may also mention that the *Geographical Journal* contains a valuable paper in which Dr. H. R. Mill describes his bathymetrical survey of the English lakes. *Good Words* has an illustrated article upon the manufacture of coal-gas, but neither *Scribner* nor the *Sunday Magazine* have articles calling for comment in these columns.

THE RELATION OF BIOLOGY TO GEOLOGICAL INVESTIGATION.[1]

I.

THE CHARACTER AND ORIGIN OF FOSSIL REMAINS.

IN prosecuting the study of the fossil remains of animals and plants, the investigator may have either one or the other of its two leading objects in view; but each being so closely related to the other, it is always essential that they should be pursued with direct relation to each other. In the first case, the leading object to be attained is the extension of our knowledge of the animal and vegetable kingdoms far beyond that which may be acquired by the study of living animals and plants; and in the second case, it is to apply that knowledge to the study of structural and systematic geology. The object in the first case is purely palæontological; in the second, it is not only to acquire palæontological knowledge, but to apply it to various branches of geological investigation.

There are seven different natural conditions in which fossil remains are recognisable, three of which relate to substance, three to form, and one to both. To those relating to substance the terms permineralisation, histometabasis, and carbonisation are here applied; to those relating to form, the terms moulds, imprints, and casts; and to the one relating to both form and substance, the term pseudomorphism.

The term permineralisation applies to that condition of fossil remains of animals which differs least from their original condition as parts of living animals; such, for example, as bones of vertebrates, shells of molluscs, tests of crustaceans, &c. The term histometabasis is applied to that condition of fossilisation in which an entire exchange of the original substance for another has occurred in such a manner as to retain or reproduce the minute and even the microscopic texture of the original. Pseudomorphism of fossils is so nearly like that of mineral crystals, that this term is equally applicable to both. It consists in the replacement of the original substance of the fossil by a crystallisable or crystallised mineral, such, for example, as calcite, pyrite, quartz in the form of chalcedony, &c., the original form of the fossil being perfectly retained. The term carbonisation is applied in this connection only or mainly to such masses of vegetable remains as coal, lignite, and peat. Moulds are cavities in sedimentary rocks which were originally occupied by fossils, the latter having been subsequently removed by the percolation of water containing a solvent of the fossils but not of the rock. Imprints do not differ materially in character from moulds, the former term being usually applied to impressions left in the rock by thin substances like leaves of plants, wings of insects, &c., after their removal by decomposition. Sometimes, however, the moulds of shells and other fossils have been reduced to the character of imprints by the extreme pressure to which the strata containing them have been subjected. Casts are counterparts of fossils, having been produced by the filling of moulds with a substance other than that of the original fossil. These are the principal conditions in which fossils occur, or by which they are represented, but one occasionally finds specimens which indicate certain conditions that are not fully recognised in the foregoing descriptions.

[1] By Charles A. White. (Abstract of a series of eight essays published in the Report of the United States National Museum.)

SEDIMENTARY FORMATIONS, THEIR CHARACTER AND LIMITATION.

There has been much difference of custom among geologists as regards the use of the term formation, some applying it to the smallest assemblages of strata which possess common characteristics, while others designate by the same term those series of formations for which the word system has been generally used. That is, some apply the term formation to local or limited developments of strata, while others apply it to such systems as the Devonian, Carboniferous, Cretaceous, &c. This term has generally been confined to the stratified rocks, but by a few authors it has been applied to the eruptive, and also to the great crystalline, rock masses. In this paper, however, the use of the term formation is not only confined to the stratified rocks, but it is restricted to those assemblages of strata which have common distinguishing characteristics, whether they have little or great geographical extent, or whether they aggregate a few feet or thousands of feet in thickness. That is, the use of the term is confined to those assemblages of stratified rocks of sedimentary origin[1] to which many authors have applied the term group, and others the term terrane.

The foregoing remarks concerning the characterisation of formations have been made with special reference to those which are more or less fossiliferous. It sometimes happens, however, that fossils do not exist, or are not discovered, in certain formations which are evidently of sedimentary origin. This may have been due in some cases to the uncongeniality, as a faunal habitat, of the waters in which the formation was deposited, and in others to their failure to receive any fossilisable remains of animals and plants from the land. In other cases, the absence of fossils may have been due to their destruction or obliteration. The latter has probably been the case with many metamorphic rocks and with the great pre-Cambrian series of stratified rocks generally. In all these cases the formations, while they may possess more or less distinct physical characteristics, lack the chief characteristics of sedimentary formations, namely, the biological.

The occurrence of an unfossiliferous sedimentary formation as a member of an otherwise fossiliferous series is unusual, but in such a case its definition and limitation would be effectually accomplished by the underlying and overlying formations. In the case, however of a great unfossiliferous series of stratified rocks like the pre-Cambrian it is necessary to adopt a method for their study and classification based wholly upon physical data, after the fact that they are pre-Cambrian has been determined from biological data. Such a method of classifying and characterising those unfossiliferous stratified rocks as they occur in North America has been proposed by Prof. R. D. Irving[2] and afterwards elaborated by others. This great series of rocks, as it is developed in America, has such distinguishing general characteristics and such magnitude and geographical extent, that some geologists have thought it worthy of being assigned to a special division of study, but because no certain traces of organic forms have been discovered in them, they have, so far as it is now known, only the indirect relation to biological geology that has just been referred to. Still it is not improbable that those strata were once fossiliferous, and that the great series was once made up of formations similar to those which have been already defined, but it does not necessarily follow that the divisions which are now recognisable by physical characteristics correspond to those formations. It is probable that they more nearly correspond to systems or to the larger divisions of systems as they are recognised in the great scale of the fossiliferous rocks of the earth.

The following conclusions concerning formations are deducible from a consideration of the available facts:—

While formations are physical objects and have only a physical existence, their proper characterisation is chiefly biological.

They are characterisable mainly by the fossil remains of aquatic faunas.

Neither their physical nor biological limits are sharply defined except as a result of accidental causes.

Their geographical limitations are indefinite except those which were occasioned by shore lines.

[1] To avoid frequent repetition, the terms sedimentary formation and stratified formation are used interchangeably when applied to formations as defined above. The terms sedimentary rocks, stratified rocks, and fossiliferous rocks are also used interchangeably, but with a somewhat more general meaning than is intended by the two former terms.

[2] Irving, R. D.: "Classification of the Early Cambrian and Pre-Cambrian Formations." (Seventh Ann. Rep. U.S. Geol. Survey, pp. 371-399.)

They do not necessarily bear any close relation to one another as to geographical area, thickness, or the duration of time in their accumulation.

Although they are thus unequal to one another, they constitute the only available physical units for local or regional stratigraphic classification.

Because of their limited geographical extent they cannot be used as units of the universal classification of the stratified rocks.

THE RELATION OF FOSSIL REMAINS TO STRUCTURAL GEOLOGY.

There are two methods by which the study of fossils may legitimately be applied to geological investigation, and the following statement of the character of these is in part explanatory of the results that may be obtained by their aid. For convenience, one of them may be termed empirical and the other philosophical, because in the one case results are obtained by experience, and in the other by reasoning upon the various results thus obtained. Still, discrimination between these two methods cannot usually be sharply drawn, because, while all geological investigation is largely empirical, it is always more or less philosphical. Such a division of the subject, however, besides being a convenience, gives an opportunity to emphasise the fact that a large proportion of the work that is done in structural geology is based mainly upon the empirical observation and collection of biological data.

Both these methods are not only important but indispensable, the one not less so than the other. Both may be, and aften are, used together, but the empirical method is more largely used in practical field studies than in others, because in such studies fossils are to a large extent treated as characteristic tokens of formations, or as arbitrary means of identifying them and distinguishing them from one another. Such identification necessarily constitutes one of the first steps in the practical study of structural geology, but the subsequent study of the fossils thus empirically used is necessarily more philosophical.

The philosophical method of treating fossil remains, however, is largely applicable to systematic geology or those branches which pertain to the universal chronological classification of the sedimentary formations and to their correlation in different parts of the world. The naturalist studies fossil remains as representatives of the long succession of progressively and differentially developed organic forms which, during geological time, have existed and become extinct, and of which succession the now existing forms of life constitute only the terminal portion. It is the results of such studies as these that the geologist uses in the philosophical studies referred to.

Of the two ways in which formations are naturally characterisable, one is physical and the other biological. Physical characterisation may be direct or general, that is, it may be by identity of kind or kinds of rock of which the formation is composed, or by its possession of that more general or indefinite property or condition which indicates homogeny.

Formations are biologically characterised only by the fossil remains of animals and plants which lived while they were in process of deposition, and the more intimate the natural relation of any of those animals and plants to the physical conditions which produced a formation, the more characteristic of it are their remains. This implies that, while no kind of fossil remains is to be rejected in practical studies of structural geology, there is much difference in the value of the different kinds for this purpose. These differences in value will be specially discussed later on.

Much has been written on methods of distinguishing between formations of marine and non-marine origin, and the legitimate inferences that may be drawn from them, respectively, as to the physical conditions which prevailed while they were accumulating. It is desirable here to present some remarks upon the relative value in practical geological field work of the fossils found in marine and non-marine formations, respectively.

That the fossil remains of marine faunas are far more valuable as indicators of the chronological divisions of the geological scale and of the correlation of its divisions in different parts of the world than are those of non-marine faunas, is apparent to every one who is familiar with even the general facts of biological geology, but it does not follow, and it is not true, that the latter are intrinsically less valuable than are the former in field studies of practical geology. For this practical work, both marine and non-marine fossils are treated by the empirical method already explained, and both are found to characterise the respective formations in the same manner.

Certain conditions, however, give each an advantage over the other under different circumstances. For example, the geographical range of the non-marine invertebrate fossil faunas, especially those of fresh water, having been sharply defined by shore lines, the species which constituted them are to that extent more characteristic of the formations in which they occur than is the case with marine faunas. Certain species of the latter faunas, as already shown, usually ranged beyond the limits of the area which was occupied by each fauna as a whole.

Non-marine formations, as a rule, occur singly in a series of marine formations, in which case the vertical as well as the geographical range of their invertebrate species is sharply defined. It is true that in the interior portion of North America there is a continuous series of fresh-water formations, and that certain of the species range from one into another. These, however, are notable exceptions to the rule referred to, and they at most only make such non-marine faunas equal to the average marine fauna as regards exceptional vertical range of species. Again, non-marine formations usually have the advantage of the presence of remains of plants and of land vertebrates and invertebrates, which in marine formations are usually so extremely rare as to be unavailable.

On the other hand, marine faunas embrace such a wide diversity of forms as compared with the non-marine, and their progressive and differential evolution from epoch to epoch has been so much greater, that they offer as faunas much more abundant means for the characterisation and identification of formations. It is clear, however, that the opinion which some geologists have expressed or implied, that the fossil contents of non-marine formations are of little value in practical geological investigation, is not well founded. The following conclusions sum up the case :—

Formations being the only true units of local or regional stratigraphic classification, their correct identification is the first, and an indispensable, step in the practical field work of structural geology.

Although formations as such have only a physical existence, their biological characteristics are always the best, and often the only, means of their identification, and therefore the exhaustive study of fossils is of paramount importance in connection with all practical investigations of that kind.

The value of fossils in this respect is as purely practical as is that of any other aid to geological investigation, and it may be made available without reference to their great value in other respects.

Although all fossil remains are valuable for this practical use, those of aquatic faunas are more valuable than any others.

Remains of non-marine faunas are of similar value for this purpose to those of marine origin.

THE RELATION OF BIOLOGY TO SYSTEMATIC AND HISTORICAL GEOLOGY.

It has been made apparent in the preceding sections that each case of structural classification of stratified rocks based upon formations as physical units is independent of all others, and that its application is necessarily of limited geographical extent, because formations are themselves thus limited. It therefore follows that the structural geology of any district or region, embracing even an extensive series of formations, may be practically and thoroughly investigated, as regards both scientific accuracy and economic requirements, independently of that of any other district or region, especially of those regions which are not adjacent. It is now to be shown how the multitude of series of formations thus locally classified throughout the world have been grouped into a universal system of classification in connection with a scale having its divisions arranged in chronological order.

When the fossil faunas and floras which characterise each of a given series of sedimentary formations are compared with those which severally characterise the formations of the next preceding and succeeding series, and the whole are systematically compared with living faunas and floras, there is to be observed among those fossil forms, when studied through an unbroken vertical range of formations, an order of successive changes and modifications indicative of a general advance in biological rank, and also an indication of structural relationship. Furthermore, when the faunas and floras of a given series of formations are compared with those of other series in other parts of the world, it frequently appears that there is a close similarity between those of a certain portion of each series which indicates their correlation. In such cases an order of biological rank is to be observed similar to that which was observed in the original case. It also frequently occurs that the range of rank is found to be greater in one or both directions than is to be observed in other cases. By such means a knowledge of the order of faunal and floral, as well as of stratigraphical, succession far beyond that which could be obtained in any one region, has been acquired.

It is upon such empirical facts as these that the early geologists based their investigations concerning the chronological arrangement of the sedimentary formations of the earth, and the grand result of which was the adoption of a general scheme and the construction of a corresponding scale for their classification. This scale, which in its present condition is a masterpiece of inductive reasoning, necessarily originated in Europe, because it was there that geology was first systematically studied, and it is there also that its adaptation is more complete than elsewhere.

Although the scale now in use was established before the truth of the progressive evolution of organic forms was accepted by naturalists, and when all differences between those forms was believed to be due to special creations, general progression in average biological rank during geological time was perceived by the early geologists, as well as by those of the present day; but with them it was the perception of a progressive succession in rank of faunal and floral groups of great assemblages of organic forms, and not the recognition of the principle of evolution. Therefore they sought methods of explaining the facts and conditions which they observed with reference to the geological scale which they had established that should accord with the biological views which then prevailed, and which were largely of a supernatural character. Indeed, in the absence of the now prevalent natural method of explaining these facts, the supernatural method of the early geologists seems to have been necessary.

The following deductive propositions which now remind a naturalist of the articles of a creed more than of a statement of scientific principles, are presented as indicating the fundamental ideas held by the early geologists in connection with the construction of the geological scale, and as illustrating the state of prevalent opinion among leading geologists upon biological subjects in their time. It is true that no one author has ever published these propositions in the exact form in which they are here presented, but they have been formulated from the published utterances of numerous authors, and from personal recollections of an active participation in geological work during a number of years, immediately preceding the great revolution in methods of biological thought and investigation which has been referred to. These propositions are :—

(1) That every species of animals and plants, both living and extinct, was specially created, and that they are, and always have been immutable. That genera, and also the higher groups into which both the animal and vegetable kingdoms are systematically divisible, are categories of creative thought, and that they also are immutable.

(2) That although secular extinction of certain species, and even genera, occurred during every stage of the geological scale, at the close of each stage, except the Tertiary, all life upon the earth was simultaneously destroyed, and that at the close of each sub-stage life was at least in large part destroyed.

(3) That, at the close of each stage coincidently with, and the divinely ordained instrument of, the complete extinction of life, there was a universal physical catastrophe, and that the close of each sub-stage was, at least in part, physically catastrophic.

(4) That all life for each successive stage was created anew.

(5) That the life of each stage embraced specially ordained generic, or more general, types which were distinctive of and peculiar to it, and that their distribution was world-wide.

(6) That there was a special ordination of characteristic types for each sub-stage, which received world-wide and simultaneous distribution within its narrow time limits.

(7) That no identical, and few similar, specific forms were created for any two or more stages.

(8) That the world-wide distribution of the distinctive types of animals and plants which were ordained to characterise any stage or substage was effected in connection with the act by which their respective faunas and floras were created; or that in the case of species not having a world-wide distribution the typical integrity of faunas and floras was preserved by the introduction of representative—that is, closely similar—but distinct species.

(9) That by creative design the average biological rank of each new creation was higher than that of the next preceding one.

(10) That upon the fossilisable parts of the animals and plants which were created for each stage, and upon those designed to characterise each sub-stage, was impressed not only their own structural features, but recognisable evidence of their chronological ordination.

These propositions represent only those views of the pioneer geologists which pertain to biological geology. Other views which were held by them are unassailable, even in the light of the present advance of science, and their biological views are not introduced here for the purpose of disparagement, but to show that they gave origin to certain erroneous methods which are in part retained as an inheritance by some palæontologists, even though they ostensibly accept the principles of modern biology.

The foregoing propositions relate to what were regarded by the early geologists as fundamental ideas in the construction of the geological scale, while the following relate to those ideas which are now held to constitute its true basis because they only accord with natural laws. These are therefore essentially a counter-statement of the preceding propositions; but the principal object of their preparation is to point out the true relation of biology to systematic, historical, and correlative geology. They consist largely of the statement of certain of the principles involved in the theory of organic evolution, but they are by no means intended as a full statement of those principles, nor are they presented for the purpose of either discussing or defining them as such. That is, the statements are made not for the purpose of formally enunciating these principles, but for the purpose of making practical application of them to the subject in hand. Such of these have been selected for statement and comment as are believed to be accepted by all naturalists who admit the truth of organic evolution, and such application is made of them as will necessarily commend itself to all geologists who admit that truth and its applicability to biological geology.

These propositions are not intended to embrace the whole range of biological geology, but only such of its leading principles as are discussed in these essays. Therefore a certain lack of immediate relevancy will appear in the order in which they are stated.

(1) All species of animals and plants have originated genetically from pre-existing forms, and therefore all are more or less mutable as regards their reproduction. These, together with the various divisions higher than species into which the animal and vegetable kingdoms are divisible, have respectively acquired their distinguishing characteristics by differential and gradually progressive evolution. The extinction of all species and other divisions of the animal and vegetable kingdoms which has taken place during geological time, has always been by natural means and in accordance with natural laws. It has generally been secular and gradual, but in many cases locally or regionally accidental. No universal extinction has ever occurred.

(2) Coincident with the progress of evolution, notwithstanding the retardation, inertion, and even degradation that have occurred along certain lines, there has been during geological time a general average advancement in biological rank of animal and vegetable forms, evidence of which is afforded by certain characteristics of their fossil remains. The evidence of this general advancement constitutes the ultimate standard of measures of geological time as a whole, and the principal means of ascertaining the order of full succession of the events which attended the production of the stratified rocks of the earth.

(3) The chronological features which fossils possess are not of a special character as such, but they are among those upon which their biological classification is based, all of which features have resulted from both progressive and differential evolution.

(4) The average rate of progressive evolution for the different branches or divisions of both the animal and vegetable kingdoms has not been the same for each in all parts of the world, nor the same for all in any one part of the world, during all the time they have coexisted.

(5) The rate of differential evolution among the forms constituting certain divisions of the animal and vegetable kingdoms was greater than that among those constituting other divisions; and it was greater for some of the members of a given division under certain conditions than it was for other members of the same division under other conditions.

(6) The succession of gradual mutations, in the development of the leading classificatory features which characterise certain groups of fossil forms, was not necessarily concurrent with consecutive portions of time.

(7) The progress of secular extinction of species and other divisions of the animal and vegetable kingdoms, including the types which specially characterise the various stages and sub-stages of the geological scale, was accelerated by adverse changes of environing conditions, and were retarded by a continuance of congenial conditions. The final consummation of the extinction of the types was naturally often, and perhaps usually, caused by catastrophic changes of conditions which occurred within the limited areas to which they were reduced by approaching secular extinction.

(8) The geographical distribution of species within the time-limits of the stages and sub-stages of the geological scale, and consequently that of the distinguishing types which the species constitute, has been effected by natural means. Such means included not only locomotory and mechanical dispersion within those time-limits from one original centre which was then the terminus of an evolutional line, but, at least in the same cases, survival in various regions by separate evolutional lines from the faunas of preceding stages and sub-stages was also included.

(9) The animal and vegetable life of each stage of the geological scale was in the aggregate different as to its forms from that of all others, and each stage and sub-stage was further specially characterised by certain generic, and also more general, types or peculiar groups of species. These types, however, were not necessarily confined within absolute time-limits.

(10) Although movements and displacements of the earth's crust have from time to time occurred over large portions of its surface, arresting sedimentation or changing its character and causing great destruction of life, there has never been a universal catastrophe of that kind. On the contrary, during all the time that disastrous conditions prevailed in any given area, conditions congenial to the existence and perpetuity of life prevailed in other and greater areas.

The second of the two sets of propositions show that certain of the views held by the early geologists, notably those which assumed the universally sharp definition of all the divisions of the geological scale, were radically wrong. Still, it is evident to every one who is familiar with modern geological literature that those views have continued to exert an adverse influence upon the biological branch of geological investigation long after they have been formally rejected, even by those who continued to be influenced by them. The early geologists adopted methods of investigation which were consistent with their biological views, but it has been shown that from the present standpoint of biology certain of those views were so fundamentally wrong that the methods which were based upon them are quite out of place in modern investigation. Still, those methods of our energetic predecessors have come down to the present time with such force and with such evidence of the general correctness of the scale which they had established by them, that it has been difficult for their successors to adopt the modification of methods which has been necessitated by the great subsequent revolution in biological thought and methods of investigation.

The facts which have been stated show that, while the scale which the early geologists established is a wonderful production of human reasoning and the best possible general standard which can be adopted before a comparatively full investigation of the geology of the whole earth has been made, it is not, and cannot be except in a general way, of universal applicability. That is, while the respective stages and sub-stages of the scale are recognisable only by means of their characteristic fossil remains, it has been shown that any of those characteristic forms are so liable to range from one stage or sub-stage to another, that it is impossible to sharply define the limits of stages, and often impossible to distinguish sub-stages in one part of the world as they are known in another part.

(*To be continued.*)

SCIENTIFIC SERIALS.

Bulletin de l'Académie des Sciences de St. Pétersbourg, Ve série, t. ii. No. 2, February 1895.—We notice in the proceedings of the meetings, that the full account of Baron Toll's observations in the New Siberia Islands will soon be published by the Academy. In the meantime the explorer has visited Switzerland in order to study glacier ice, and has found there further proofs, supported by A. Forel, in favour of the masses of ice which he has found in New Siberia (buried under clays containing fossil stems of *Alnus fruticosa* fifteen feet long), really being remains of the ice-sheet which covered the islands during the glacial period.—The yearly report of the Academy, which contains, among other matters, the obituaries of L. Schrenck, A. Middendorff, I. Schmalhausen, and P. Tschebycheff, whom the Academy has lost during the last year.—The positions of 140 stars of the star cluster 20 Vulpeculæ, according to measurements taken from photographic plates, by A. Donner and O. Backlund (in German). The measurements were taken on two plates, one of which had been exposed for twenty minutes only, and the other for one hour, and the accord between the two is most satisfactory, the average difference being 0·00s. in right ascension, and 0″·02 in declination, while the difference between the measurements on the photographic plates, and the direct measurements of Schultz, attains on the average −0·040s. in R.A. and −0″·55 in D.—On the differential equation $dy/dx = 1 + R(x)/y$, by N. Sonin.—On a new entoptric phenomenon, by S. Chirieff.—Note on the last mathematic conversation with P. L. Tschebycheff, about his rule for finding the approximate length of a cord, and the means of extending the method to curves of double flexure (all three in Russian).—The ephemeride of the planet (108) Hecuba, by A. Kondratieff.

Vol. ii. No. 3, March 1895.—Yearly reports of the Philological Section of the Academy, and of the committees: for the Baer premium, which was awarded this year to the Tomsk Professor Dogel, for his researches into the histology of the nervous system, and to Prof. Danilevsky for researches into the comparative study of parasites in blood, and the Lomonosov premium, which was awarded to A. Kaminsky for his work on the yearly march and geographical distribution of moisture in the Russian empire in 1871–90.—On the Perseids observed in Russia in 1894 (in French), by Th. Bredikhine. The observations were made by several observers at Odessa and at Kieff. It must be remarked that the observers have had difficulty in observing the meteors, the course of which made a sharp angle with the direction of the vertical line; and this circumstance is probably not without some influence upon the determination of the radiant point. The meteors observed on July 24, 26, and 27, seem to belong to a meteoric stream other than the Perseids. Combining the results of this year's observations (which are given in full in thirteen tables) with the observations of the preceding year, and calculating the elements for each of the radiants, the author sees in them a confirmation of the theoretical results he arrived at in his paper on the Perseids of 1893; the values of the inclination (i) of the centres of radiation—with the exception of the three first, which are somewhat uncertain—are all below the value of i for the comet of 1866. The average value of i before the epoch (August 10·5) is 60°, while after that time it is only 56°; but this decrease cannot be considered as quite real, on account of the said uncertainty in i for July 24–27. An inspection of the charts shows that a condensation of the radiation is taking place towards the epoch which falls on the night of the 10th to the 11th, as seen from the observations made in Italy by P. Denza. The arithmetical average of the coordinates of the three chief radiants of August 10 are $\alpha = 48° 48'$, and $\delta = 56° 30'$, we have: $l = 63° 32'$, $b = 36° 51'$, $i = 64°{\cdot}8$, $s = 72°{\cdot}8$, and $V = +34°{\cdot}4$. The value of i corresponds to the radiant of the comet of 1866. Considerable variations appear in the elements Ω and π; the perihelium is displaced in the direction of the orbital motion of the meteors. In a subsequent memoir the author proposes to take up the theory of the subject, and to evaluate the secular variations of the generating orbit of the comet, and of some of its derived orbits.—On the best means of representing a surface of revolution on a plane, a mathematical treatment of the subject, in Russian, by A. A. Markoff.—On the limit values of integrals, by the same.—List of the works of P. L. Tschebycheff.—On the methods for correctly determining the absolute inclination by means of the induction inclinator, and the degree of exactitude lately obtained with this instrument at the Pavlovsk Observatory, by H. Wild (in French).—The non-periodical variations in the quantity of precipitation at St. Petersburg, by E. Heintz (in Russian, summary in French).—Ephemeride of the planet (209) Didon, by Mme. Eugénie Maximoff.—Determination of the magnitudes of the stars in the star cluster 20 Vulpeculæ, by Mme. Marie Shilow. The diameters were measured by the micrometer, and the corresponding magnitudes were calculated by means of Charlier's formula.—On one sum, a mathematical note (in Russian), by I. Ivanoff.

THE numbers of the *Journal of Botany* for May to July contain, besides mere technical papers, one on the genus

Argemone, by Dr. D. Prain, a description of a new species of *Bryopsis*, and of a peculiar mode of growth in another species, by Miss E. S. Barton; an account of fossil plant-remains in peat, by Mr. A. Gepp; and a description of a large number of new species of Orchidaceæ, by Mr. A. B. Rendle, from the plants brought by Mr. Scott Elliot from Tropical Africa.

SOCIETIES AND ACADEMIES.

LONDON.

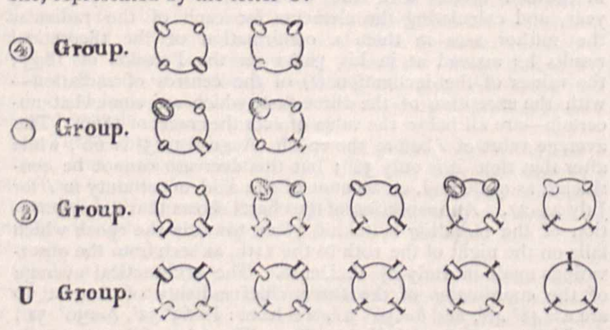
Royal Society, May 16.—"The Complete System of the Periods of a Hollow Vortex Ring." By H. C. Pocklington.

May 30.—"The Kinematics of Machines." By Prof. T. A. Hearson.

In this paper it is shown that all machine movements, however complex, are derived from the association together of some of a comparatively limited number of kinds of simple motions, which take place between consecutive directly connected pieces.

Certain geometrical laws are enunciated, from which are derived the conditions necessary for the association of those motions together in one machine. It is shown that those laws preclude the existence of certain combinations of motions. By attaching to each kind of motion a suggestive symbol a method of expressing the constitution of a machine movement by a simple formula is proposed, whereby similarities and differences between machines may be exhibited at a glance.

The author commences by considering a mechanism, consisting of four bars united in one continuous linkage by four pins which have parallel axes. By imagining the length of the links to undergo variation from zero to infinity, it is shown that this mechanism is representative of all the simple plane mechanisms, and, by imagining other variations to occur, it is shown to be representative of still further classes of mechanisms, in which the parts do not move in or parallel to one plane. In this the relative motions of consecutive pieces are either turning, when one piece revolves completely around relatively to the other, the representative symbol being the letter O, or swinging, when one piece turns through a limited angle relatively to the adjoining one, represented by the letter U.



The first law enunciated, which governs the association of the O and U motions, is founded on the geometrical fact that the sum of the four angles of the quadrilateral is constant. After a complete revolution the angle between the bars is considered to have been increased or diminished by 2π.

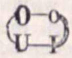
From this it is impossible for only one motion to be turning and the other three swinging, otherwise the sum of the four angles would increase or decrease by 2π each revolution.

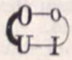
The second law, which governs the association of the motions, has to do with the proportions between the length of the links necessary to permit of complete turning. This is founded on the fact that one side of a triangle cannot be greater than the sum of the other two. From these two laws together it is shown that it is impossible to have two Os alternating with two Us.

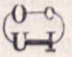
Next it is pointed out how the U motion may be provided for by constructing a circular slotway in one piece, and shaping the other piece to fit the slotway, so that by imagining the radius of curvature of the slotway to be indefinitely increased a relative movement of reciprocating sliding motion, represented by the symbolical letter I, will be substituted for the swinging motion U. A slide being conceived to be a swing through a zero angle about an infinitely distant centre, the previously mentioned laws will apply to associations containing I motions, and it will follow that a combination of three slides and one swing is precluded by the first law.

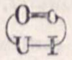
By the application of the governing laws 14 distinct combinations are found to be possible, and only 14. They are exhibited by the following formulæ, in which a large O associated with a small o signifies that in one case adjacent links turn relatively to one another so as to continuously increase the angle between them, and in the other to continuously diminish the angle. The double ◎ signifies that two complete revolutions accompany one complete to-and-fro swing or slide.

Applying Reuleaux's principle of "Inversion" it will be seen that 32, and only 32, distinct machine movements can be derived from the above 14 mechanisms. Those from the same mechanism are distinguished from one another in the formula by using a thick line for the frame link. For example,

 signifies a machine movement like that employed in the crank-and-connecting-rod engine.

 is exemplified in the oscillating engine much used in paddle-wheel steamers.

 is found in Stannah's pendulum pump, and

 quadrupled is the movement adopted by Rigg in the design of his high speed engine.

The author next discusses the relation of cams and spur-wheel mechanisms to the foregoing kinematic chains, showing that they are the result of the suppression of one of the previous four links and the amalgamation of the two adjoining simple motions into one more complex. A comparison is also made with belt gearing, and expressive formulæ suggested.

The author then passes to the consideration of machines the parts of which do not move parallel to one plane.

The first 13 of the previously mentioned mechanisms have their counterpart in mechanisms the parts of which move parallel to the surface of a sphere. Hooke's joint is the best known example. The 14th consisting of 3 slides cannot be adapted to a sphere but it can to a cylinder, and from it are derived 4 possible screw mechanisms.

The remaining mechanisms consist of those in which the axes of the turning and swinging motions neither meet nor are parallel. They include the motion which occurs at a ball-and-socket joint. The method of classification according to the proposed scheme is summarised as follows:—

All simple machine movements may be ranged in four divisions, viz.:—

(1) Consisting of plane mechanisms, in which the pieces move in or parallel to the surface of a plane.

(2) Spherical mechanisms, in which the pieces move in or parallel to the surface of a sphere.

(3) Cylindrical mechanisms, in which the pieces move in or parallel to the surface of a cylinder.

(4) The remainder, to which the name conoidical mechanisms is given, in which the axes of the swinging and turning motions neither meet nor are parallel.

The mechanisms in each of these divisions are classed in two subdivisions.

Subdivision S, with surface contact of consecutive links.

Subdivision P, with point contact of consecutive links.

The mechanisms in each of the eight subdivisions are still further subdivided into combinations. The combinations of 1_s, 2_s, and 3_s, are exhaustively enumerated, and it is suggested that an extension of the methods of applying the geometrical laws would lead to the preparation of an exhaustive list of the possible combinations in the other subdivisions. The combinations are still further subdivided into inversions according to Reuleaux's principle of the inversion of a machine.

Lastly, the author proceeds to show how the foregoing considerations assist in the analysis of compound mechanisms. It is assumed that practically all compound mechanisms contain a continuous mechanism A, of not more than four links, from which definiteness of relative motion of all the other links is derived. Any two links of A in their exact length, or longer or shorter, may be adopted to form with two new links a second mechanism B, and any two of A or B, or one of A and one of B, may be adopted to form with two still further added links a third mechanism C, and so on. In this way a definiteness of relative motion of many links in a compound mechanism is derived. The notation lends itself to a clear exhibition of the manner in which two or more simple mechanisms are associated together, and the compound mechanism built up.

June 20.—"The Influence of the Cerebral Cortex on the Larynx." By Dr. J. S. Risien Russell.

The author found the condition of the peripheral laryngeal apparatus has practically no effect on the result obtained from the central nervous mechanism, for abduction or adduction of the vocal cords resulted on excitation of the appropriate area of the cerebral cortex, irrespective of whether abduction or adduction was obtained on excitation of the recurrent laryngeal nerves in the same animal. No evidence of unilateral representation of the movements of the vocal cords in the cerebral cortex was obtained, although this point was tested in various ways. Nor was it found possible to inhibit the abductor muscles by excitation of the cortical centre of their antagonists the adductors. It was found that both in the dog and cat there existed a focus, excitation of which resulted in adduction of the vocal cords, and another near to this, stimulation of which resulted in abduction of the cords. While in the cat it was possible to differentiate these movements without any preliminary measures being adopted, it was only after the adductor fibres of one recurrent laryngeal nerve had been divided transversely that it first became possible to evoke abduction of the vocal cords on excitation of the cortex, though in subsequent experiments it was sometimes possible to evoke this movement on excitation of the cortex of the dog without adopting this preliminary measure. The other effect on the cords, which it was as a rule found most difficult to differentiate from that of abduction, was acceleration of their movements. It was further found that on the anterior composite gyrus, below the abductor centre, there existed a focus, excitation of which resulted in a clonic adductor effect on the cords, in which the cords were first brought into a position of moderate adduction, and then there was added rapid short to-and-fro excursions. On passing within the confines of Spencer's area for arrest of respiration, it was found that in the peripheral parts of this area there existed three foci, excitation of which affected the cords in different ways. The most anterior was responsible for arrest of the cords in adduction, *i.e.* in the expiratory stage of their excursions; excitation of the focus behind this, and corresponding, probably, to Horsley and Semon's abductor centre in the cat, was followed by arrest of the cords in abduction, *i.e.* their inspiratory position; while the most posterior focus, which is situated at about the junction of the anterior composite and anterior sylvian convolutions, resulted in intensification combined with acceleration of the movements of the cords when stimulated. Excitation of Spencer's chief focus for arrest of respiration on the olfactory lobe, resulted in arrest of the cords in the position they occupy during expiration in the dog, and in the position they occupy during inspiration in the cat.

Physical Society, June 28.—Dr. Gladstone, Vice-President, in the chair.—Mr. Bowden read a note on an electro-magnetic effect. A long glass tube containing mercury, and fitted with a small stand-pipe to indicate hydrostatic pressure, is passed between the poles of an electro-magnet. On passing a current of about 30 amperes through the mercury in this tube, the stand-pipe being turned so as to indicate the pressure either perpendicular or parallel to the lines of force of the field of the electro-magnet, movements of the mercury in the stand-pipe take place. When the stand-pipe is perpendicular to the lines of force of the field, the mercury rises or falls according to the direction of the current. When the stand-pipe, however, is parallel to the lines of force, the mercury always *rises*, whatever the direction of the current. Prof. S. P. Thompson said there appeared to be three unexplained effects, one proportional to the current and the field, and reversible; another, independent of the direction of the current, or of the field; and a third, which only occurred while the current was changing in strength. In addition there may be a fourth effect, which up to now has not been noticed. The motion of the mercury column in Fig. 1 of the paper was in the opposite direction to that of the drag on a conductor carrying the current. An apparent *rise* in pressure might be due to a decrease in the density of the mercury due to the heat developed by the current. Mr. Blakesley asked if the author had noticed any changes in level in the mercury reservoirs at the ends of the tube. The author, in his reply, said the reservoirs at the ends were so large that no changes of level were appreciable.—Mr. Rhodes read a paper on the armature reaction in a single phase alternating current machine. In this paper the author gives the investigations that were the subject of a verbal addendum to a paper read before the Society on a previous occasion. He investigates the lag or lead of the E.M.F.s over the current, and applies the results to examine whether the field excitation of the generator or the motor is strengthened or weakened by the reaction of the armature currents. Mr. Tunzelmann expressed a hope that the author would amplify parts of his paper. Mr. Blakesley said the conclusion of the author that "either of two alternate current machines may be driven as a motor by the other, irrespective of their relative E.M.F.s," is not invariably correct. The facts of the case were these: the E.M.F. of the motor may exceed that of the other machine to a certain extent; but that E.M.F., multiplied by the cosine of the angle of electric lag, must yield a product not greater than the E.M.F. of the generator; *i.e.* using Mr. Rhodes' symbols $e \cos \theta$ must not be greater than E. Mr. Blakesley gave a geometrical proof of this, but the same proposition had been given by him some ten years ago in the course of investigating the subject generally. This was at a time when Dr. John Hopkinson was, with less than his usual perspicuity, teaching that synchronous alternate current machines could not be run in series with stability, both doing work. Referring to the author's diagrams, Mr. Blakesley said that in a problem involving so many elements as that under consideration, it was impossible with the limited dimensions of space to represent the results with the complete generality of a formula. Some elements had to be taken as the independent, others as the dependent variables. The author had considered the power transmitted to the motor, the E.M.F. of the generator and the angle of electric lag as independent. The E.M.F. of the motor was dependent. In Mr. Blakesley's original diagrams the E.M.F.s were both considered independent as well as the electric lag, and the powers applied or transmitted as dependent variables. In any case the formulæ properly derived from such diagrams became perfectly general, and it did not appear to him that the change of method indicated could properly be called a new theory on the subject. As a matter of fact, diagrams based on the independence of the E.M.F.s and the electric lag would furnish a better means of discussing the question of the stability of the motion than Mr. Rhodes' plan, and this might account for the entire omission from the paper of this important matter. Prof. S. P. Thompson said it was impossible to discuss the question of stability till the subject of armature reaction had been thoroughly investigated. The terms lag and lead had been used by Mr. Rhodes in a consistent manner; but this was not always done, and he recommended that the phase of the current which was common to both generator and motor be taken as the standard. The author, in his reply, said he agreed with Mr. Blakesley that there was a limit to the extent to which the motor might be excited, and this upper limit could easily be obtained from the figure given in the paper. The question of armature reaction was, however, most important, as it might excite the field two or three times more than the original excitation. Since motors were designed to do a certain amount of work, and not the work to fit the motor, it was most natural to take the output of the motor as fixed.—Mr. Shelford Bidwell read a paper on the electrical properties of selenium. The author has continued his investigations on this subject, and has come to the following conclusions: (1) The conductivity of crystalline Se appears to depend principally on the impurities which it contains in the form of metallic selenides. It may be that the selenides conduct electrolytically, and that the influence of light in increasing the conductivity is to be attributed to its property of facilitating the combination of Se with metals in contact with it. (2) A Se cell having platinum electrodes, and made with Se to which about 3 per cent. of cuprous selenide has been added is, even though unannealed, greatly superior both in conductivity and sensitiveness to a similar cell made with ordinary Se and annealed for several hours. (3) Red Se in contact with copper or brass, is quickly darkened by the action of light, owing, it is suggested, to the formation of a selenide. (4) Crystalline Se is porous and absorbs moisture from the air, and it is this moisture that causes the polarisation of Se after the passage of a current. (5) The presence of moisture is not essential to sensitiveness, but appears to be in a slight degree favourable to it. (6) If cuprous selenide is made the kathode in an electrolytic cell, and a strip of platinum the anode in water, red Se mixed with detached particles of the selenide is deposited in the water. (7) The photo-electric currents sometimes set up when light falls upon Se, are dependent upon the presence of moisture, and are no doubt of voltaic origin. (8) Perfectly dry Se is below platinum in the thermo-electric series. Prof. Minchin (communicated) suggested that the selenium "cell" should be called a selenium "resistance." A grid having one terminal made of aluminium and the other of copper, might form a true cell, and might generate an E.M.F. when light fell on it. He (Prof. Minchin) would like to know if the author had tried any such cell in which light simply and solely generated an E.M.F. He could not agree that chemical action must necessarily follow the action of light in a cell. For, take the case of the oldest photo-electric cell—the thermopile—what chemical action can we show here for all the energy of the incident heat. Chemical action due to light may, or may not, occur according to the nature of the cell. Mr. Appleyard asked whether the author had submitted these selenium resistances to the action of electric oscillations. Prof. Minchin's "impulsion" cells were greatly influenced by electric oscillations. The great variation in the resistance with time of the author's cells pointed rather to an effect of contact between the selenium and the electrodes, than to an elementary change in the structure or composition. He (Mr. Appleyard) had recently tried to crystallise a supersaturated solution of sodium sulphate by electric oscillations, as well as by direct sparks, and by currents of several amperes, but no crystals could be induced to form. Change of contact, rather than change of structure, appeared to him to be the most promising direction in which to look for an adequate theory of selenium resistances. Prof. Ramsey said the quantity of Se liberated in the electrolytic experiment was much too great to be accounted for by oxygen dissolved in the water. The study of Se was very interesting, for this substance was on the borderland between those bodies in which the electric conduction was metallic, and those in which it was known to be electrolytic. The author, in his reply, said he agreed that the name "selenium cell" was not an appropriate one. He had not tried the effect of electric oscillations.—The Society then adjourned till the autumn.

PARIS.

Academy of Sciences, July 1.—M. Marey in the chair.—The President announced the decease of Prof. Huxley, Correspondant of the Anatomy and Zoology Section.—On photographs of the moon and new objects discovered by means of them, by MM. Lœwy and Puiseux.—On an extensive class of linear partial differential equations, of which all the integrals are analytical, by M. Émile Picard.—Laws of extinction of a simple wave on the high seas, by M. J. Boussinesq. The coefficient of extinction (with the distance) of a simple wave is inversely proportional to the fifth power of its demiperiod T.—On the estimation of minute quantities of arsenic, by M. Ad. Carnot. The arsenic is separated in the usual manner as sulphide, this is dissolved in free ammonia and treated with silver nitrate and hydrogen peroxide. The solution is then precipitated by bismuth nitrate, followed by ammonia, the accompanying bismuth hydrate is dissolved out by nitric acid ($\frac{1}{15}$ nitric acid of sp. gr. 1·33), and, finally, the bismuth arsenate is dried at 110° and weighed.—Truffles (Terfâs) from Morocco and Sardinia, by M. Ad. Chatin.—Comparison of the heating of the muscles in the cases of positive and negative work, by M. A. Chauveau. During negative work, descent or lowering, the temperature of the muscles concerned was raised to a notably less degree than during corresponding positive work, ascent or raising.—Contribution to the study of arable soil. Quantities of air and water contained in clods of earth, by M. P. P. Dehérain.—On the products of oxidation of benzylidenecamphor and benzylcamphor. Nitrosate or nitronitrite of benzylidenecamphor, by M. A. Haller.—A new instrument (tachéograph) serving to survey and trace directly from the earth's surface, by M. Schrader.—On curves traced on a surface, of which the osculating sphere is tangential at each point to the surface, by M. E. Cosserat.—On linear equations to the derived partials, by M. Étienne Delassus.—On the integration of ordinary differential equations, by M. Alf. Guldberg.—On the propagation of sound in a cylindrical tube, by MM. J. Violle and Th. Vautier.—On the apparent attractions and repulsions of electrified conductors in a dielectric fluid, by M. Gouy. The apparent forces exercised between conductors with given charges in a liquid dielectric result: (1) from their mutual attractions and repulsions, the same as in a vacuum; (2) from the hydrostatic pressure produced by the force which attracts the dielectric in the sense where the intensity of the field increases most rapidly.—New method of measurement of electric capacities based on the sensitiveness of the skin, by M. H. Bordier.—On the solubility of superfused liquids, by M. Louis Bruner. The author finds that superfused sodium thiosulphate is much more soluble in alcohol than the corresponding solid compound.—On the specific heat of superfused salts, by M. Louis Bruner. The curve of specific heats at different temperatures for sodium thiosulphate shows a maximum near the point of fusion, 48° C.—On paratungstic acid, by M. L. A. Hallopeau.—On the estimation of alumina in phosphates, by M. Henri Lasne. A method of precipitation of pure aluminium phosphate is described, which avoids the complications introduced by the use of molybdate or citrate in estimating alumina. The precipitation is accomplished by the use of ammonium thiosulphate.—On sodammonium, by M. de Forcrand. A thermo-chemical study.—On the phosphoric esters of allyl alcohol, allylphosphoric acid, by M. J. Cavalier.—Preparation and conductibility of new methyl alkylcyanacetates, by M. J. Guinchant.—Verification of Tschermak's law relative to plagioclases, and a new process of orientation and of diagnosis of felspars in thin plates, by M. A. Michel-Lévy.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Matriculation Directory, No. xviii., June, 1895 (University Correspondence College).—Allen's Naturalist's Library. A Hand-book to the Game Birds, Vol. 1: W. R. Ogilvie-Grant (Allen).—Missouri Botanical Garden, Sixth Annual Report (St. Louis, Mo.).—Iowa Geological Survey, Vol. 3, 2nd Annual Report, 1893 (Des Moines).—An Analysis of Astronomical Motion: Dr. H. Pratt (Norman).—Report of the International Meteorological Congress held at Chicago, Ill., August 21-24, 1893, Part 2 (Washington).—Ice-Bound on Kolguev: A. Trevor-Battye (Constable).—Wild England of To-day: C. J. Cornish (Seeley).—Thirteenth Annual Report of the Fishery Board for Scotland. Part 1. General Report (Edinburgh).

PAMPHLETS.—Report on the Loss of Gold in the Reduction of Auriferous Veinstone in Victoria: H. Rosales (Melbourne).—Royal Gardens, Kew, Hand-list of Herbaceous Plants (Eyre).—Great Eastern Railway Company's Tourist Guide to the Continent (30 Fleet Street).

SERIALS.—Geological Magazine, July (Dulau).—Scribner's Magazine, July (Low).—Jahrbuch der K.K. Geologischen Reichsanstalt, Jahrgang 1894, xliv. Band, 2, 3, and 4 Heft (Wien).—L'Anthropologie, Tome vi. No. 3 (Paris).—Science Progress, July (Scientific Press, Ltd.).—Proceedings of the Bath Natural History and Antiquarian Field Club, Vol. 8, No. 2 (Bath).—Annals of Scottish Natural History, July (Edinburgh, Douglas).—Journal of the Sanitary Institute, July (Stanford).—Himmel und Erde, July (Berlin).—Blackwood's Magazine, July (Blackwood).—Transactions of the Leicester Literary and Philosophical Society, April (Leicester).—Mind, July (Williams).—Botanische Jahrbücher, Zwanzigster Band, 5 Heft (Leipzig).

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