THURSDAY, FEBRUARY 13, 1896.

POPULAR GEOLOGY.

Open-Air Studies: an Introduction to Geology Out-of-doors. With illustrations. By Prof. Grenville A. J. Cole, M.R.I.A., F.G.S., &c. Pp. viii + 322. (London: Griffin and Co., Limited, 1895.)

PROF. GRENVILLE COLE has already won himself wide repute as a writer on geological science by his book, entitled "Aids in Practical Geology" (Griffin and Co.). There Prof. Cole treated the branches of mineralogy, petrology, and palæontology, dwelling more particularly on methods of work in laboratory and museum. This recent publication, "Open-Air Studies," breathes another atmosphere, lighter, gayer, and for the popular mind more enticing. The Professor leads his readers, like a class of students, into one countryside after another, makes use in imagination of bicycle and railway on the low ground, ascends boldly into glacier-swept regions, and roams at will across moorland and highland Everywhere he directs their powers of observation, drills great facts into them by presenting particular aspects again and again as occasion offers. Now and then the Professor calls a halt to recapitulate, set in order, or give more careful demonstration of stratigraphical difficulties.

The "Open-Air Studies" are ten in number. The author gives the key-note in his preface: "In our walks abroad we may be struck by this or that detail, seemingly trivial in itself, which finally leads up to some one of the vexed problems of the globe." The first chapter gives some preliminary teaching on "The Materials of the Earth," the rock-forming minerals being carefully examined. It is not until chapter ii. that the author takes his pen into the "Open Air." We are told: "We must imagine that we wake up on a fine clear morning at the foot of some of our wild mountain-masses in the British Isles" (p. 28), and we are guided up a swirling stream, past pools and waterfalls, to "a mountain hollow." On our way, our professorial guide bids us do many simple things. We have to "dig for a little while with a walking-stick," that "we may be able to see the tiny streamlet flowing"; we have to note that the "loose blocks, half buried in the grass, are made of the same materials as the cliffs"; to watch how, if some one kicks a stone, "it goes bounding down across the grass, it leaps over one or two of the little cliffs, and so descends until it rests on some lower ledge of the hill-side" (p. 30). Then we put our fingers into pot-holes, and move the pebbles in them, and we watch the stream washing down its load of mud and pebble, and listen to its rattling stones.

In this manner the pleasant little object-lesson is continued, and the attentive follower is made to observe for himself the forces of denudation at work on a mountain-side. Arrived at the mountain-tarn, the Professor is beguiled by the endless questions which rise to our lips into delivering quite a learned lecture on nature's tools—cloudland; wind, dew, and rain; frost, ice, and snow; stream, glacier, and avalanche. In the midst of it, we find ourselves frequently transported to glaciated Alpine valleys. Cirques are described and explained, an illustration is given of a snow-filled cirque on the Matterhorn, and applied to the past history of our own mountain-hollows.

From the tenour of this chapter, which is a fair type, it will be seen that Prof. Cole limits the subject-matter of his new book to that contained in any ordinary elementary text-book of geology. Familiar truths are re-set, and any claim to originality or success rests entirely upon the workmanship and brilliancy of that re-setting.

If we rightly understand Prof. Cole's method, it is the antithesis of the analytic method practised by a text-book writer. The latter passes, as it were, the diffuse light of knowledge through his own prism-acting mind, and it appears on his manuscript as a fully-resolved spectrum in which every band of colour is clearly presented to the student. Prof. Cole's method, on the other hand, is to call upon each student to make his own mind the prism. Observations, massed together with all the local details of nature, are thrown upon the student's mind, which may or may not be fit to act on its own account as a clear prism. Theoretically the system is laudable, but decidedly unstable. The author himself succeeds in carrying it out only through about one-half of the 313 pages of "Open-Air Studies." Certainly half the chapters—i., vi., vii., viii., and x.—are really text-book studies, and these are, moreover, the most concise and complete in their treatment.

To proceed with the actual contents, chap. iii. is called "Down the Valley." Its central theme may be thus expressed, in the words of the author: "The story of all streams is alike—excavation at one end and deposition at the other; and the area of deposition extends slowly back up the valley as time goes on" (p. 80). A typical Tyrolese valley is made the main basis of observation. The cutting of deep gorges by torrential side-streams, the spreading-out of talus-cones at the mouth of tributary valleys, the "creep" of a hillside downwards, landslips, the occasional damming up of the stream and formation of a lake, the resulting wide agricultural flats where these become later silted up or drained—these and other features are described with copious reference to famous scenes and incidents in the Alps.

The river Rhine is studied between Chur and the Lake of Constance, to illustrate alluvial deposits and the variations which take place in the course of a river. A "delta" is defined, examples given, and brought home to us by the remark that "an enterprising child with a spade can soon produce a shallow lake, and can observe how it becomes filled and obliterated by the encroachment of a delta at its upper end" (p. 86).

The main discussion of deltas, however, falls into the following chapter, "Along the Shore." There, too, the first lessons on sedimentary deposits are given. The abrasion of a coast-line by the action of the waves, leads up to an explanation of Sir Andrew Ramsay's term "plain of marine denudation." The author then goes on to show how the worn-away material is reconstructed into different kinds of strata bedded beneath sea, lake, and river. Even deep-sea deposits of siliceous organisms, the growth of coral-reefs, and the formation of oolites find a place here, although the chapter begins and ends with the description of a pretty fishing-village, low cliffs, and a coastguard station!

Chapter v., "Across the Plains," is probably the most successful in the book. Written in the same style as those just described, it is distinctly more realistic and dignified in its tone. The subject-matter is also more coherent, and its treatment—for the scope of the book—exhaustive. Were all ten chapters equal to this one, we would say Prof. Cole had been most successful in carrying out a set purpose pleasantly and beneficially for cultured readers who travel at home and abroad. But unfortunately the high-water level is seldom reached in the book.

Chap. vi. dips into the lore of "Dead Volcanoes," starting with the highland of Auvergne, and returning to our own islands. Chap. vii. is a careful study of "A Granite Highland," and generally of igneous rocks. "The Annals of the Earth" (chap. viii.) gives a general account of the stratigraphical succession. Chap. ix., "The Surrey Hills," is a very happy combination of the art of seeing and describing beauty of landscape, along with the practical demonstration of the principles of stratigraphy. It resounds with the stroke of the geologist's hammer, and the enthusiasm of a true guide and teacher. Chap. x., "The Folds of the Mountains," will be to most readers difficult, as it deals with the hard subject of the making of mountain-chains. The Alps are mainly considered, the writing is admirably clear, and the doctrines taught are in accord with all the newest researches.

It remains to add how often passages occur throughout the book which remind us of the artistic feeling for nature displayed by the author in a previous publication, "The Gypsy Road." Every small detail in a scene is touched with a sympathetic, kindly pen, that reminds one of the lingering brush of a Constable. Take his description of Fenland:

"From Cambridge northward to the estuary of the Wash, there are forty-five miles of level ground. . . . Between the scattered villages lie areas of black peat, covered with coarse grass, and dug into here and there for fuel. The roads are carried along the crests of broad embankments, with dark, little drainage-cuts on either side of them, crossed by bridges to the fields. A few trees cluster round the old farm-houses, protecting them from the winds that sweep across the fenland steadily for weeks together. . . . The sky is usually full of great cumulus-clouds, dark grey below and silvery white above, where the sunlight strikes through them in long shafts across the grey-green plain. A church-tower or a windmill is visible ten miles away, when touched on by these sudden gleams; then it sinks back again into the great gloom of the horizon" (p. 121).

Taken as a whole, the book fails, in so far as it constantly mixes together two distinct styles of writing, the picturesque and the didactic. It is at the same time eminently readable, and will be warmly hailed by many lovers of geology out-of-doors. A solid groundwork is formed by the five chapters referred to above as partaking of the text-book method. The others are somewhat of a skim-swallow type, too rapid in their course, too overladen with detail and local colouring, to be adapted either to the serious wants of a student or to the slow apprehension of a complete novice in nature's fields. The book will appeal most to the dilettante student, or to the tourist who has already had abundant opportunity of observing, who delights in finding his own dim perceptions vividly mirrored by a trained scientific mind, and in seeing them marshalled towards the solution of "vexed problems of the globe."

MARIA M. OGILVIE.

PRIMITIVE PICTURE-WRITING.

The Beginnings of Writing. By Walter James Hoffman, M.D. With an Introduction by Prof. Frederick Starr. Pp. xiv + 209. (London: Macmillan and Co., 1895.)

THERE is no more fascinating subject for study than the development of the art of writing through its various stages, from the first rough pictures drawn by primitive man to the alphabets in use among the civilised nations of the present day. But the historic systems of writing go back to a remote antiquity, and although we can trace some of them back many hundreds of years, the question of their first origin is one that is beset with many difficulties. It is at this point, however, that the anthropologist comes to the antiquary's help, for he shows that the mind of savage man always works along the same general lines of development. All primitive races, whether in China, Central India, Western Asia, Europe, North Africa, or North America, have used the same rude means to record their thoughts and actions, scratching on their rocks or weapons rough pictures of their possessions and pursuits. The methods used by hunters of the Palæolithic age to record a successful hunting expedition resemble those which the North American Indian now employs. From these rude pictures the savage passes, by a natural law of development, to the representation of ideas, expressing motion or condition by means of gesture-signs. Then, after certain pictures have become associated with certain words, he begins to use them to express their sound but not their meaning; in fact, he begins to write phonetically. His pictures, as he writes more rapidly, change to signs, which become more and more simplified, and finally his system of syllabic writing he develops still further till he reaches the most perfect phonetic system of writing, the alphabet.

Such in brief are the main lines of development which all races follow who work out for themselves the art of writing, and it is this attractive subject that Mr. Hoffman has selected for his book. In the brief preface in which Prof. Starr introduces him and his book to the reader, we learn that he has for some years been engaged both in field-work and study among the Indian tribes of North America, and from his book itself we find that his studies have resulted in a most interesting collection of the various methods of writing employed by these primitive races. It is true that many of his examples are already well known from other publications, but the collection does not aim at being original, for the book belongs to an educational series, where original work would be out of place. What was wanted, and what in the main Mr. Hoffman has given us, was a representative collection of the various methods of writing employed by the North American Indians. These are here classified, described clearly and illustrated with numerous small cuts; and for this part of his work we have nothing but praise. But why did not Mr. Hoffman confine his book to the subject in which he is in a sense a specialist? Why did he seek to extend the aim of his volume so that it might apply to the beginnings of writing all the world over?

Yet such an extension Mr. Hoffman has attempted in the course of about 200 *octavo* pages printed in large type. The result was inevitable. The lion's share of the little space at his disposal falls naturally enough to the North American Indians with whose pictographs and writing he is most familiar; but, in consequence of this mistaken aim at universality, the book is peppered here and there with Egyptian hieroglyphs, once or twice a cuneiform character crops up, and we have even come across a Christian symbol. In many places, therefore, the work is patchy, and the reader is in some danger of bewilderment. Moreover, this is not the only danger to which Mr. Hoffman exposes his readers, by rashly leaving the ground he knows; for his studies have not been sufficiently extended to enable him to act as a trustworthy guide elsewhere. He is certainly wise in making considerable use of "The Alphabet," by Isaac Taylor, but some statements made twelve years ago naturally now-a-days need revision. For instance, the dates he quotes at the beginning of p. 184 must now be considerably altered in view of M. de Sarzec's recent discoveries at Telloh in Southern Babylonia. But perhaps the most misleading portions of the book are those passages in which he refers to the so-called "Hittite" inscriptions. Here Mr. Hoffman's authorities are not so trustworthy as Mr. Taylor, and the result is disastrous. From his description of these hieroglyphs, the reader would certainly infer that no doubt existed as to their interpretation, but no impression could be more erroneous. The four or five systems of interpretation that have been proposed, and the last of which appeared less than two years ago in the *Zeitschrift* of the German Oriental Society, differ totally from one another, and are mutually exclusive; that is to say, each interpreter has employed a different method and system of interpretation, and, although they all work on the same inscriptions, the translations they have produced do not agree at all. Of course Mr. Hoffman is at liberty to select one of these systems and to say it appears to him to be correct, but to take the results of one of them, and to describe them as though they were universally adopted without reference to the controversy which still rages, is surely misleading in the highest degree. Moreover, on p. 158 occurs this rather puzzling sentence: "In the Akkadian, *a* signifies 'water' . . . and is represented by the inverted triangle, the prototype in Hittite being the vase or *olla*." If we take this sentence literally, we can only infer that Mr. Hoffman considers the earliest cuneiform character to be the descendant of the Hittite hieroglyphs. We think, however, that he cannot seriously hold this view, and that the sentence referred to does not rightly express his meaning.

It is not necessary, however, to dwell at greater length on the blemishes which mar an otherwise excellent book. They might, indeed, have been avoided by the exercise of a little discretion, and the book would have been none the less interesting if its contents had been confined to the beginnings of writing among the tribes of North America. Even as it is, the greater part is both interesting and instructive, and the excellent print and numerous diagrams add considerably to its attractiveness.

OUR BOOK SHELF.

Elements of Geometry. By Prof. George C. Edwards. Pp. vii + 293. (London: Macmillan and Co., 1895.)

PROF. EDWARDS has forsaken the general sequence of parts in the study of the elements of geometry as propounded by Euclid, and has given us a book almost on a new plan. It must have been felt by most people, when in their early school-days they commenced to read Euclid's Elements, that much of the first few pages was almost unintelligible; and while no one would take exception to the rigidity of the methods there used, yet it would be exceedingly difficult for the average beginner to comprehend many of the definitions from the text alone without further *viva voce* explanation.

The substance of Euclid's Elements may be condensed into a very small fraction of its present dimensions for all practical purposes without losing any of the rigidity of argument or usefulness in application, and in this way a student may understand the proportionality of the sides of similar triangles without previously wading through four or five books. Prof. Edwards has arranged his book in this condensed form, and at the same time has presented his subject in a manner which attracts the attention of the reader by its simplicity and usefulness. One feature of this book is the great number of diagrams and figures which it contains; and where more than one figure is required with any one proposition, they are all carefully and separately given. The author does not overload the student with a mass of definitions at the commencement, but introduces them with explanations, when required, in the context. The usual mathematical conventions are introduced very early into the subject-matter, such as signs to denote directions; and the usual contractions are freely used, a table of them being given at the beginning for reference.

The first three chapters deal with propositions which relate to triangles and parallel lines; chapter iv. deals with the properties of circles; and chapter v. with areas, and the proportional relations of similar figures. This is one of the most important sections of the book, and includes some useful results which are generally obtained from books on trigonometry. Immediately following we find a chapter devoted to chords and tangents, and another to polygons and limits, including finding the value of π. Then follow over thirty pages of problem questions, with hints to some of their solutions, and innumerable figures to illustrate them where necessary. No pains appear to have been spared to make the whole as clear and intelligible as possible.

We then find a few chapters on solid geometry, which include matter dealing with surfaces, volumes, intersection of surfaces, and spherical triangles. The last chapter contains some useful propositions in conic sections.

Throughout the book there are a great number of examples, notes, suggestions and warnings to the reader, which should be of especial value to those who have not pursued the study of geometry to any extent previously. Altogether the book is a valuable one for any educational institution, and exceptionally so to technical students.

Elementary Mensuration. By F. H. Stevens, M.A. Pp. xii + 243. (London: Macmillan and Co., 1895.)

Mensuration. By the Rev. A. Dawson Clarke, M.A. Pp. vi + 88. (London: Rivington, Percival, and Co., 1895.)

MR. STEVENS' book is divided into two parts, the first only requiring a knowledge of Euclid's first book, and the use of algebraic symbols, in order to understand it; while the second part necessitates a much fuller acquaintance with geometry and algebra, and a slight knowledge of trigonometry. The book is not technical, but contains a thorough treatment of those principles upon which all calculations of dimensions depend, and therefore it will be valuable as an introduction to works concerned with special practical applications of the rules described and exemplified. Moreover, it should play a useful part in schools, by illustrating the concrete applications of abstract geometrical principles. The large number of original examples will be found of great assistance by teachers, and the questions, selected from papers set by the principal examining bodies, will prove of service as tests of the students' capabilities in working out mensuration problems.

Of a less detailed character is the Rev. Dawson Clarke's primer, intended "for the use of schools, and Woolwich, Sandhurst, and Home Civil Service candidates." The book is a collection of rules and formulæ, with examples to explain their use, and numerous exercises selected from various examination papers. It particularly appeals to students who learn the rules of mensuration in order to utilise their knowledge in the examination-rooms of the Civil Service Commissioners; but it is, also, a concise text-book which other students will find serviceable.

Physical Measurements. By Frank C. Weedon. Pp. 232. (London: G. Gill and Sons, 1895.)

THIS volume is another help towards the establishment of rational methods of instruction in elementary science. It is a laboratory manual of practical physics for organised science schools under the Department of Science and Art, and other secondary schools. Of the educational value of the course contained in the book, there can be no doubt; for the experiments (which are of a character suited to beginners) follow a natural order, and are such as will develop the faculties of observation, investigation, and common sense; in fact, they will lead the student to think as well as learn. The book is divided into three sections, dealing respectively with measuring and weighing, relative densities, and experimental mechanics. Experiments on these matters elucidate the fundamental principles which form the basis of a scientific education. The knowledge cannot be labelled "Sound, Light and Heat," or "Magnetism and Electricity," and therefore superficial critics, and syllabus-bound teachers, think it is not Physics. We are of the opinion, however, that experimental work in measuring and weighing, constitutes the foundations of physics. The student who is able to weigh and measure carefully, and to observe and think accurately, knows more of the realities of physical investigation than if he had spent a dozen years in learning scraps of information about other people's contributions to knowledge.

LETTERS TO THE EDITOR.

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

The New Actinic Rays.

A BRIEF account of some experiments which I have been making in my laboratory at Blythswood, in connection with the new photographic rays, may, I hope, be of interest to the readers of NATURE.

Three or four years ago I constructed a very powerful Wimshurst electrical machine. It has 128 plates, three feet in diameter, and is driven by an electric motor of about 1½ horse-power. With this machine, which was specially built for quantity, I can obtain a torrent of sparks a foot and a half or two feet long: and it occurred to me to try to obtain photographs, after the manner of Röntgen, but without the intervention of a vacuum tube.

A thick sheet of lead was placed upright between the poles of the electric machine, as a screen, and was connected to the ground, the two poles being insulated. A sensitive dry plate was put into the camera dark slide, with a metallic object to be photographed (a steel washer with holes in it), and this was connected, by a wire which passed out of the dark slide, to the ground. The whole was wrapped up in four folds of a black velvet focussing cloth, and was put, in some cases between the negative pole and the lead screen, and in other cases between the positive pole and the lead screen, the plane of the slide being perpendicular to the line of discharge. In all cases good strong negatives were obtained with exposures of about twenty minutes. The machine was arranged to give a silent brush discharge during the experiments.

I next tried similar experiments with the dark slide containing the sensitive plate quite out of the line of discharge, and with the plane of the plate parallel to the line of discharge, and obtained equally good results. It seems, therefore, that the vacuum tube is not essential to the production of the Röntgen rays. With reference to this, however, I am not so sure, as I think I may have been deceived by using isochromatic plates—at all events I am engaged in further experiments either to confirm or the contrary. BLYTHSWOOD.

Blythswood, Renfrew, February 10.

WITH reference to Mr. Porter's letter regarding the amount of electric energy and exposure required for obtaining photographs by the Röntgen method, I may mention that against his photograph taken with a 3-inch coil and four minutes' exposure, I can instance a successful human foot that shows the bones very distinctly almost up to the ankle-joint, in taking which I used a 10-inch coil working at about half power without Leyden jars, and for which fifty-five seconds' exposure proved ample.

For living physiological subjects, it is very important to shorten the exposure as much as possible, and to attain a minimum in this respect, very high vacua and considerable E.M.F. are requisite.

Again, for an extensive subject, a large tube placed at a considerable distance from the subject is required, and more electric energy is needed for this than for a small subject, for which a smaller tube in closer proximity will suffice.

66 Victoria Street, S.W. A. A. C. SWINTON.

HAVING made some experiments on the lines laid down by Mr. Gifford, of Chard, I think the two enclosed photographs will prove of interest, as showing perhaps that Mr. Gifford's method of dispensing with a Crookes' tube introduces elements of another character. Both these negatives were taken without a tube, using the discharge from the terminal of a small Tesla transformer. In each case a metal plate was placed behind the film in communication with the other terminal of the coil. Under these conditions a stream of "discharge" passes from one terminal through the photographic film.

The interesting point is that not only does the outline of the coin come out, but also the impression. And that in the case of the florin the coin was placed *behind* the film. The same sparking appearance as described by Mr. Gifford is evident.

From the fact that it is immaterial on which side of the photographic film the coin is placed, it is evident, I think, that we have here to do with a "contact" phenomenon, and not with Röntgen's rays at all. SYDNEY D. ROWLAND.

38 Wimpole Street, W., February 2.

"The Astronomical Theory of the Glacial Period."

As it was my two letters which initiated the interesting and not unfruitful discussion now going on in your pages on the above subject, I think it right to say a few words in reply.

The object of my letters was to point out (perhaps I did it in somewhat too heated language) that Sir Robert Ball, whose personal and official distinction give his words exceptional weight, had in his work entitled "The Cause of an Ice Age" given fresh currency to a discredited theory, and further that when this had been pointed out, he had refused to take any notice of his critics, and continued to publish his book.

In his letter to you, Sir R. Ball (if I do not misunderstand him) entirely breaks away from the position maintained in his book, and gives up the case there argued, definitely and completely. While Prof. Darwin, who had given the book the advantage of his friendly recommendation and countenance, tells us he is now reluctantly compelled to take the other side.

So far as I know, there does not now remain a single mathematician or astronomer who favours a purely astronomical theory of an Ice Age; a theory which, as Arago, Humboldt and Croll, all urged long ago, is quite inadequate to explain the climatic effects required. Every one, as far as I know, now agrees with the American astronomer Meech, who subjected the astronomical theory to a most searching analysis, as far back as 1857, that "the causes of notable geological changes must be other than the relative position of the sun and earth under their present laws of motion." It is with this sentence that I close my own analysis of the problem in chapter ix. of my "Glacial Nightmare."

As I understand, Sir R. Ball in surrendering his old view, which was *that astronomical causes by themselves* are sufficient to produce an Ice Age, falls back upon a modification of Croll's meteorological argument. While, however, he no longer relies on the adequacy of astronomical causes alone as competent to produce an Ice Age, he does not admit the conclusiveness of Mr. Culverwell's argument, but bids us remember that the world cannot be cut up into a number of parallel zones shut off from each other by solid partitions, each one of which can be treated as a separate climatic region, but that the climate of every zone is very largely indeed the result of heat brought in or carried away by air and water from or to other zones. No one disputes this. It is in fact an elementary postulate of meteorology, and applies as much to Sir R. Ball's arbitrary zone termed a hemisphere as to any other.

What we want Sir R. Ball to do is not to rest content with this barren postulate, but to apply it as Croll applied his postulates, and to prove that, granting the greatest possible alteration of the relative length of the seasons due to eccentricity, &c., which, as Mr. Culverwell has shown, will by itself tend to shift the climate of each zone about five degrees, how is this going to affect the circulation of the air and of ocean currents sufficiently to constitute an Ice Age? This was the problem Croll virtually set himself to analyse by a minute and ingenious investigation.

Croll's arguments have been riddled through and through by several writers, and in this behalf I may perhaps venture to again refer to a minute dissection of them in a chapter, headed "Transcendental Meteorology," in the work already cited, namely, the "Glacial Nightmare," and which I have been told by some eminent physicists is unanswerable. I can, at all events, say it has not been answered.

If Dr. Ball can discover some method of curing the radical defects of Croll's arguments, he will have made us a valuable present. Meanwhile, if I do not entirely misunderstand his present position, it is more clear than ever that he owes it to us all to withdraw his "Cause of an Ice Age" from circulation, for it has not only been condemned by its distinguished and formerly friendly critic, but has been actually condemned by its own author. HENRY H. HOWORTH.

Athenæum Club, January 30.

The Positions of Retinal Images.

PROF. KULPE, in his "Outlines of Psychology" (translation by Prof. Titchener), sets out with much effectiveness the argument in favour of believing that the visual perception of extended surface is an original datum of consciousness attached to the extended retinal surface (and no more to be explained than why the sensation red feels the way it does, and not otherwise); and he also shows conclusively that the peculiarity of nerve-excitation by which right- and left-ness and up- and down-ness are distinguished, is of peripheral (and not of central) origin; by adducing the facts of metamorphopsia, that is, the cases in which a portion of the retina has become detached by a wound, and has afterwards grown on again, and in which vision is correspondingly inverted—exactly as when a piece of the skin of the forehead has been grafted upon the nose, say, and upon touching it we seem, for a long time afterwards, to be touching the forehead. He thus attaches himself to the innate-space-sensation theory of James and Sumpf. But his effort to show that the out- and in-sensation is fundamentally dependent upon the different shape of the image cast upon the two retinas by an object, carries less conviction with it. This is, of course, an essential element of the sensation when the object looked at is so complex as to consist of two points at a given distance from each other. But when it consists of a single bright point only, we are still perfectly able to determine its position in depth (if it is looked at with two eyes), and the sensation-element which enables us to do this is plainly more *fundamental* than the other. To say the least, it is something which ought not to be overlooked.

It is plain that in this case the only criterion which is left us (granting, what is the case, that the localisation can be effected with certainty with two eyes, but only vaguely and indefinitely with one) is *the distance apart of the double images*; it is that which we estimate, unconsciously of course, in spite of the fact that one image is in one eye and the other in the other, and it is that which we translate, without difficulty, into a feeling of depth. But there is always an ambiguity; for every point, O (Fig. 1), without the horopter-circle, which casts images upon the retina at the points *r* and *l*, there is a congruent point, O', within the horopter-circle, which casts images upon the corresponding retinal points, *l'* and *r'*, and which, therefore, gives images which are at the same distance apart. We have no difficulty in sensation in distinguishing between a bright point at O and one at O', but how can this be effected? There is still a difference in sensation between the two cases. The nasal half of each retina gives distinctly brighter images than the temporal half; in the case of the object O, which gives the two images, *r* and *l*, the remoter one is the brighter, while in the case of the object O', which gives the images *l'* and *r'*, it is the nearer one which is the brighter. A bright image of the object, which seems to us to be the thing itself, is attended by a somewhat fainter secondary self, whose presence we are absolutely unconscious of, in our non-scientific lives, as an image,[1] but which we evaluate with the utmost nicety as a sign of the distance away of the real object,

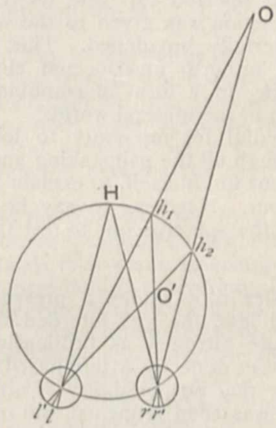

FIG. 1.

and which has, moreover, a different significance according as it stands nearer to, or further from, the fixation point than the image which we regard as the object itself. This explanation may seem at first to vary much in the air, but its correctness has been demonstrated by Schön in a very ingenious manner (*Arch. f. Ophthalm.*, xxii. and xxiv.). His experiment has been unaccountably overlooked by all the makers of text-books, as far as I have seen, but it is of critical importance. He arranges a series of screens with openings in them in such a way that *two different* bright objects are seen, one by the right eye only, in the line h_2 O produced, and the other by the left eye only, the line h_1 O produced. The *positions* of the double images now correspond equally well for an object at O or at O'; and the person experimented upon thinks he sees an object now at O and now at O', exactly in accordance with the way in which the relative brightness of the objects beyond the screens is made to vary. When the image which falls at *r* is brighter than that which falls at *l*, the object is seen at O; when the image which falls at *l* is brighter than that which falls at *r* (sufficiently brighter, of course, to counteract the relative efficiency of the different halves of the retina) the object is seen at O'. It is therefore demonstrated that it is the relative brightness of the images which is the determining factor in enabling us to localise objects in one or other of the two congruent worlds without and within the horopter-circle. I have myself repeated the experiment with perfect success. C. LADD FRANKLIN.

Baltimore, January 13.

[1] There are many people who cannot bring the secondary image into consciousness, no matter how hard they try, when it falls at any distance from the fovea.

THE STORY OF HELIUM.[1]

CHAPTER II.

MY next chapter breaks new ground. I take you right away from the celestial regions, we leave the sun for a time, and I land you at Washington. I take you to a room in the geological department there, and I introduce you to one of the officials at work—a certain Dr. Hillebrand.

He is engaged upon the chemical examination of specimens of the mineral uraninite from various localities. The time was the year 1888.

He deals with a little crystal such as I have in my hand, he puts it in a vessel containing some sulphuric acid and water. He finds that he gets bubbles, bubbles of gas produced out of the crystal by means of the sulphuric acid. Well, he collects this gas, gets a quantity of it, tries to find out what it is, and he comes to the conclusion that it is nitrogen, and he publishes a statement that this gas obtained from the mineral uraninite consists of nitrogen.

This result was new. He thus writes about it:—

"In consequence of a certain observation" [the one I have just referred to] "and its results, an entirely new direction was given to the work, and its scope wonderfully broadened. This was the discovery of a hitherto unsuspected element in uraninite, existing in a form of combination not before observed in the mineral world."

It is not needful for my story to follow Dr. Hillebrand through all the painstaking and patient labour he cut out for himself, to explain this anomalous behaviour. Needless to say he did not omit to employ the spectroscope to test the nature of the new gas.

He writes[2]:—

"In a Geissler tube under a pressure of 10 millimetres and less, the gas afforded the fluted spectrum of pure nitrogen as brilliantly and as completely as was done by a purchased nitrogen tube. In order that no possibility of error might exist, the tube was then reopened and repeatedly filled with hydrogen, and evacuated till only the hydrogen lines were visible. When now filled with the gas and again evacuated, the nitrogen spectrum appeared as brilliantly as before, with the three bright hydrogen lines added."

On this paragraph I may remark that it has long been known that gases like nitrogen give us quite distinct spectra at different temperatures—one fluted, another containing lines. Which of these we shall see in a tube will depend upon the pressure of the gas and the electric current used. The fluted spectrum of nitrogen is very bright and full of beautiful detail in the yellow part of the spectrum; the line spectrum, on the other hand, is almost bare in that region.

Note well *that it so happened* that the pressure and electric conditions employed by Dr. Hillebrand enabled him to see the fluted spectrum.

Dr. Hillebrand concludes his paper by pointing out that—

"The interest in the matter is not confined merely to a solution of the composition of this one mineral; it is broader than that, and the question arises, May not nitrogen be a constituent of other species in a form hitherto unsuspected and unrecognisable by our ordinary chemical manipulations? And, if so, other problems are suggested which it is not now in order to discuss."

[1] Continued from p. 322.
[2] "On the Occurrence of Nitrogen in Uraninite,' *Bulletin*, No. 78, U.S. Geol. Survey, 1889-90, p. 55.

CHAPTER III.

Now, I have another part of the story to bring before you.

Following the recognised practice of story-tellers, I have to change the scene; instead of dealing with the sun, I must take you still further afield and consider some points connected with distant suns, which we call stars, and the nebulæ which are so often associated with those distant suns. A star appears to us as a point, but a nebula appears to us as a surface, and the result is that the method of investigating these two classes of heavenly bodies, to get at the facts I have to bring before you, is somewhat different.

Let us deal, in the first instance, with the method of photographing the spectrum of a star which appears to us as a point. The best way of doing that is to use an instrument represented in this diagram, which consists of

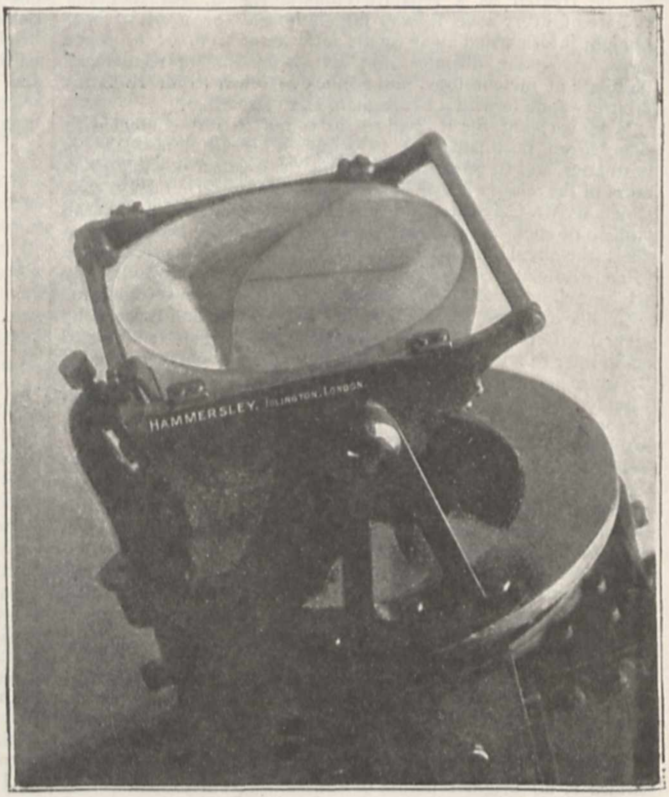

FIG. 8.—The objective prism.

a telescope with an object-glass at one end in front of which a prism of large dimensions is fixed, through which the light has to pass before it enters the telescope at all.

Hence on the photographic plate, instead of the point formed by a star under ordinary conditions, owing to the tearing asunder by the prism of the different coloured rays of which the white light of the star is composed, we get a fine line, along which is represented the complete radiation, or complete system of hieroglyphics, from red to violet.

Another method is to employ a spectroscope of the ordinary construction in connection with a large reflector. The light from the star, which is grasped by the large mirror, is thrown by a diagonal mirror inside the tube on to the slit. In the case of the nebulæ, of course allowing first one part and then another to fall upon the slit of the spectroscope, we are enabled to determine what light-notes build up its light, and whether the light-notes from every part of the nebula are absolutely the same, or whether there are differences of chemical constitution in different parts. In this way it was found many years ago that both in the light of stars and of nebulæ we get very considerable varieties of spectra. Let me prove this point by referring to some recent observations.

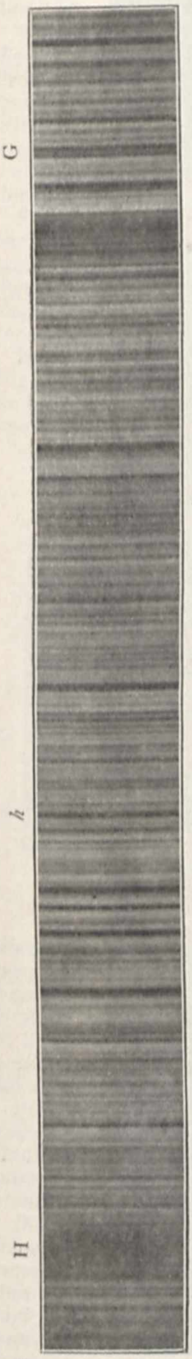

FIG. 9.—Spectrum of Arcturus between G and K.

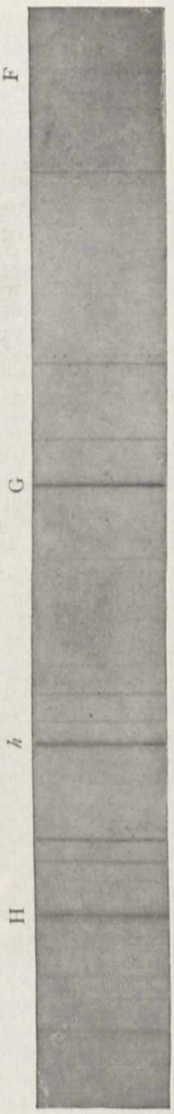

FIG. 10.—The spectrum of Bellatrix between H and F

The original negative of the nebula of Orion, taken at my Observatory at Westgate-on-Sea in 1890, contains fifty-six lines, and of course by determining, as we have been able to do, the wave-lengths—the positions of these lines in the spectrum—we can determine the exact light-notes represented, and therefore the substances which produce them. Now I have to tell you that in this spectrum of the nebula of Orion we get lines of unknown origin exactly coinciding with those unknown lines which I have already referred to as having been seen in the sun's atmosphere. Mark it well, that some of the unknown lines in that atmosphere, those that we have not been able to see in our laboratories, are identical in position with some of the unknown lines in the nebula of Orion, the line D^3 being one case in point.

Then, I have next to say that in the spectra of many stars we have these same lines, which we have so far classified as unknown for the reason above stated, that in our laboratories we have not been able to get any lines which correspond with them.

I call attention to the spectra of some stars which have been carefully photographed and measured. You will see that the progress of this branch of science lately has been so considerable that any statement made with regard to the positions of lines, and therefore the chemical origins of them, may be made with a considerable amount of certainty as depending upon very accurate work.

It forms no part of my present purpose to indicate the various classes in which the stars have been classified by different observers according to their spectra, but some of the more salient differences must be pointed out. Thus we have stars with many lines in their spectra, others with comparatively few. I will take the many-lined stars first.

The diagram (Fig. 9) represents the spectrum of Arcturus, a star the spectrum of which closely resembles that of the sun. In α Cygni we have another star with many lines, but here we note, when we leave the hydrogen on one side and deal with the other stronger lines, that there is little relation between the solar spectrum and these lines.

I next come to the stars with few lines: these are well represented by many of the chief stars in the Constellation of Orion. Bellatrix is given as an example (Fig. 10).

Some astronomers hold the view that all the stars were hotter when they were first formed, than they ever have been afterwards. I have attempted to prove not only that this view is unphilosophic, but, further, that the spectroscopic facts indicate that we can arrange the stars along one line, provided the hottest stars are supposed to occupy the middle of it. In other words, that stars begin cool, get hotter, and then cool down finally.

In this way we are enabled to trace the chemical changes from one star to another, and that has been done with an expenditure of considerable labour.

CHAPTER IV.

Now I go to another chapter of my story. I ask you to accompany me in your mind's eye to another eclipse. The importance of eclipses in the abstract I think you will grant, from some of the results which I have already referred to. A method which was first employed by Respighi and myself during the eclipse of 1871, was employed on a large scale and with great effect during the eclipse of 1893. The light proceeding from the luminous ring round the dark moon was made to give us a series of rings, representing each bright line seen by the ordinary method, on a photographic plate. The observers this time were stationed in West Africa and in Brazil. The African station was up one of the rivers, not very far away from the town of Bathurst.

As a matter of fact, the very same instrument which was previously referred to as used for obtaining photographs of the stars was sent here, in order that photographs of the eclipse of the sun might be taken on exactly the same scale as the photographs of the stars had been. We next come to the Brazilian station. Here again the instruments were somewhat similar. I have another view of the

Brazilian station, and it affords me very great pleasure to bring this before you, because it enables me in this public way, as President of the Vesey Club, to tender thanks to Sir Benjamin Stone for the help he gave the Brazilian party.

We next pass to the results which were obtained by Messrs. Fowler and Shackleton, who were in charge of the instruments at the two stations. The diagrams will indicate the kind of celestial hieroglyph—to come back

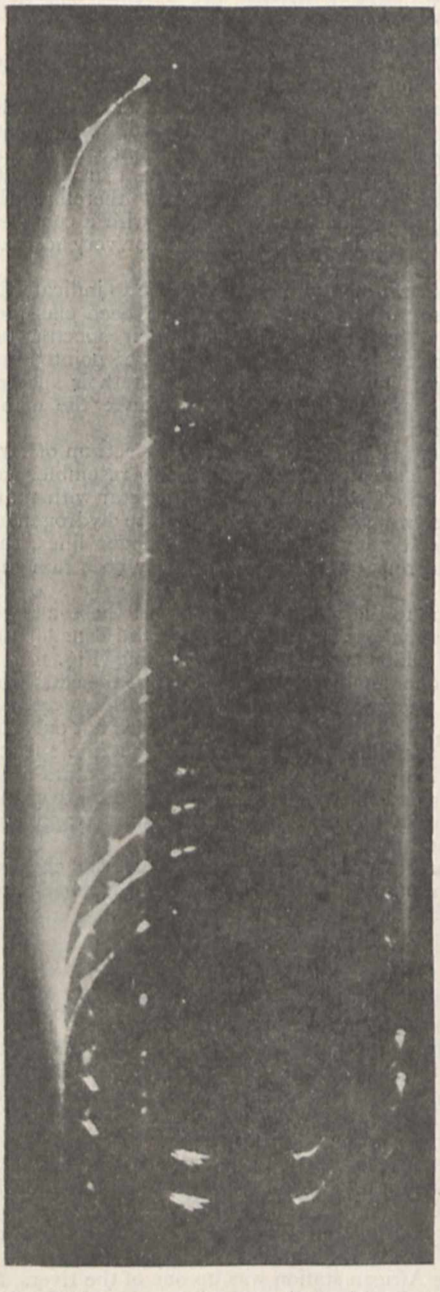

FIG. 11.—Untouched reproduction of photograph (African station) taken very shortly after the commencement of totality, the exposure being "instantaneous." At this phase of the eclipse a considerable arc of the chromosphere was visible, and its spectrum is therefore shown in addition to the spectrum of the higher reaches of some of the large prominences extending beyond the moon's limb. It will be seen that at H and K there are long arcs of chromosphere and prominences, the absent portions being of course obscured by the moon. One very small prominence is especially rich in lines.

to the old image—we have to deal with when this method is employed.

We get more or less complete rings when we are dealing with an extended arc of the chromosphere, or lines of dots when any small part of it is being subjected to a disturbance which increases the temperature and, possibly, the numbers of the different vapours present.

The efficiency of this method of work with the dispersion employed, turns out to be very marvellous, and in securing such valuable and permanent records as these, you will acknowledge that we have done very much better than if we had contented ourselves with the style of observations that I have referred to as having been made in 1871.

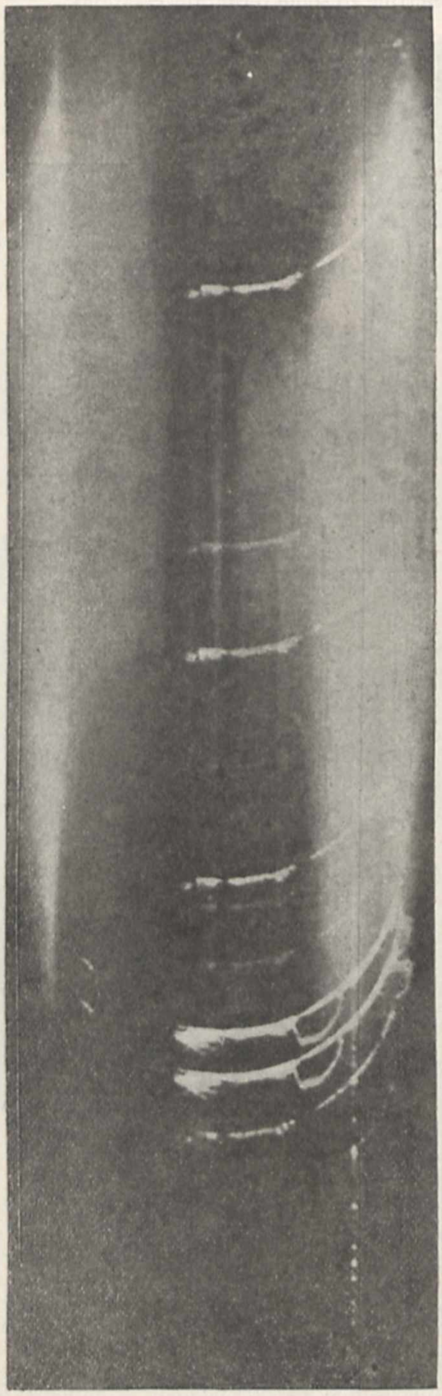

FIG. 12.—Photograph 21 (African station), taken shortly before the end of totality. A portion of the chromosphere on the other edge of the dark moon is now visible in addition to numerous prominences. It will be seen that one of the smallest prominences is rich in lines, and closely resembles that which appears in Fig. 11.

And now the plot of my story begins to thicken. On examining these eclipse records, we find that we have to do exactly with those unknown lines which had already been photographed in the stars and in the nebulæ.

As was to be expected we, of course, deal with the lines recorded in the first observations of the solar disturbances, and chronicled in that table of Prof. Young's, to which I have already called attention; but the important thing, the unexpected thing, is the marvellously close connection between eclipse- and star-spectrum photographs.

CHAPTER V.

Again I recall you from the heavens to the earth; the time is the beginning of the present year.

You remember that last year was made memorable by the announcement of the discovery by Lord Rayleigh and Prof. Ramsay of a new gas called argon, and you know that the discovery was brought about chiefly in the first instance by the very accurate observations of Lord Rayleigh, who found that when he was determining the weight of air in a globe of a certain capacity, the weight depended upon the source from which he got the nitrogen.

From the nitrogen from atmospheric air he obtained one weight, and from that obtained by certain chemical processes he obtained another, and ultimately it was found that there was an unknown element which produced these results, these various changes in the weight; and as a consequence we had the discovery of argon.

It struck Mr. Miers, of the British Museum, that it might be desirable to draw attention to the nitrogen which we have seen Dr. Hillebrand in 1888 obtaining from his crystal of uraninite; his observations, of course, were more in the mind of Mr. Miers than in the minds of the pure chemists. He therefore communicated with Prof. Ramsay, who lost no time, because it was very interesting to study every possible source of nitrogen and see what its behaviour was in regard to the quantity of argon that it produced, and in the relation generally of the gas to the argon which was produced from it.

Prof. Ramsay treated uraninite in exactly the same way that Dr. Hillebrand had done in 1888. The gas obtained as Dr. Hillebrand had obtained it was eventually submitted to a spectroscopic test, following Dr. Hillebrand's example. But here a noteworthy thing comes in.

It so happened that the pressure and electrical conditions employed by Prof. Ramsay were so different from those used by Dr. Hillebrand that, although nitrogen was undoubtedly present, the fluted spectrum which, as I have previously stated, floods the yellow part of the spectrum with luminous details, was absent. But still there was *something* there.

Judge of Prof. Ramsay's surprise when he found that he got a bright yellow line; that was the chief thing, and *not* the strong suggestion of the spectrum of nitrogen. Careful measurements indicated that the twenty-six-year-old helium had at last been run to earth, D^3 was at last visible in a laboratory. Prof. Ramsay was good enough to send specimens of the tubes containing this gas round to other people, and he sent one of them to me.

I received Prof. Ramsay's tube on March 28, but as it was not suitable for the experiments I wished to make, in his absence I obtained some gas for myself by a different method with which I need not trouble you. From March 30 onwards my assistants and myself had a very exciting time. One by one the unknown lines I had observed in the sun in 1868 were found to belong to the gas I was distilling from bröggerite, not only D^3 but 4923, 5017, 4471 (Lorenzoni's *f*) 6677 (the BC of Fig. 7), referred to previously, and many other solar lines, were all caught in a few weeks.

But this was by no means all. The solar observations had been made by eye, and referred therefore to the less refrangible part of the spectrum, but I had obtained and studied hundreds of stellar photographs, so I at once proceeded to photograph the gas and compare its more refrangible lines with stellar lines.

Here, if possible, the result was still more marvellous. In the few-lined stars, by May 6, I had caught nearly all the most important lines at the first casts of the spectroscopic net. Fig. 13, which includes some later results, will give an idea of the tremendous revelation which had been made as to the chemistry of some of the stages of star-life.

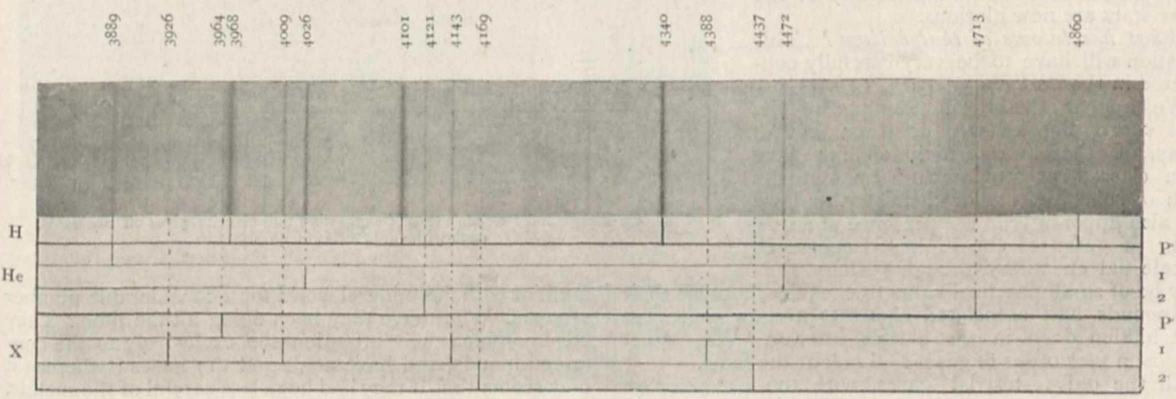

FIG. 13.—The spectrum of Bellatrix showing the lines of hydrogen and those which have been traced to the gas obtained from minerals.

These results enabled us at once to understand how it was that the "unknown lines" had been seen both in the sun's chromosphere and some nebulæ and stars. The gas obtained from the minerals made its appearance in the various heavenly bodies in which the conditions of the highest temperatures were present, and the more the work goes on we find that this gas is really the origin of most, but certainly not of *all*, of the unknown lines which have been teasing astronomical workers for the last quarter of a century.

A great deal of work has been done upon these gases from other points of view than those which affect my story, and perhaps I may be allowed just to refer for a minute or two to one of the results which have been obtained by myself.

It is perfectly obvious that the gas as obtained from uraninite is a mixture of several gases; that the gas which gives the yellow line has not yet been isolated, but is always mixed up with other gases which give other lines.

In May I communicated to the Royal Society some experiments which indicated in a most conclusive manner the fact that the lines D^3 and 667, to deal with two only for the sake of simplicity, were not produced by the same gas, and that 667 seemed to be a compound gas of which D^3 represents one of the constituents.

Some little time after, Profs. Runge and Paschen, from an entirely different standpoint, arrived at exactly the same conclusion. They recognised two gases, one represented by D^3, the other by 667, and further they showed that the lines might be arranged into six similar and beautifully rhythmic series, a principal and first and second series for each gas. These are indicated in the diagram of Bellatrix on p. 345, and in Fig. 14; He = helium is the gas which contains D^3, the other gas I so far call "gas X."[1]

This result is, however, more important from the chemical than from the astronomical point of view *at present*.

A word in conclusion referring to the occurrence of this gas in terrestrial minerals.

We are brought face to face with one question, which ought to influence many lines of work for many years to come. I have already suggested to you that we really now can talk with something like certainty and definiteness about hot stars and cooler stars, and that in the hottest stars we know of, the atmospheres of those stars consist almost entirely of hydrogen and helium.

But see what a little trace of helium we have in this small planet of ours, which undoubtedly was once a sun, which undoubtedly once had an atmosphere just as glorious in its hydrogen and its helium as any of the other stars are now glorious.

What has become of that helium? This question will have to be very carefully considered in the next few years.

We appear to be in presence of the *vera causa*, not of two or three, but of many of the lines which so far have been classed as "unknown" by students both of solar and stellar chemistry, and we are also apparently in the presence of a new order of gases of the highest importance to celestial chemistry, though perhaps they may be of small practical value to chemists, because their compounds and associated elements are for the most part hidden deep in the earth's interior. Why do I suggest a *new* order of gases? Look at the facts.

All the old terrestrial gases, with the exception of hydrogen, are spectroscopically invisible in the sun and stars—though they doubtless exist there—and these new gases, scarcely yet glimpsed, have already supplied us with many points of contact between our own planet and the hottest part of our central luminary that we can get at, and stars like Bellatrix.

The work certainly is full of hope for the future, not only in relation to the possibility of more closely correlating celestial and terrestrial phenomena, but since it indicates that terrestrial chemistry, founded on low density surface products in which non-solar gases largely enter, is capable of almost infinite expansion when the actions and reactions of the new order of gases, almost, it may be said, of paramount importance in certain stages of stellar evolution, shall have been completely studied.

I have some other results to refer to, but it is quite sufficient, I think, to leave my story as I have told it to you without going back on any of the characters, or without dealing in any greater detail with the *dénouement* of the plot.

But the story has a moral. The more we can study the different branches of science in their relation to each other, the better for the progress of all the sciences. Another point is, that in the study of nature we behave in a very foolish way if we think there is anything unimportant which comes under our eyes. *If it had so happened* that Dr. Hillebrand had seen the line spectrum of nitrogen in 1888, we should have saved all these seven years of waiting for this terrestrial source of helium; and I may add, further, that argon would have been discovered as well in the first hour's work. In science, results of the first importance depend upon the minute examination of so-called residual phenomena; it is too much the general tendency, of scientific work on a large scale, to think too much of those results which may have a practical importance.

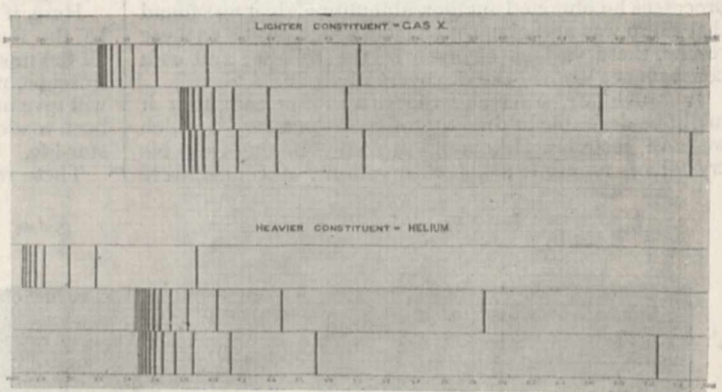

FIG. 14.—Runge and Paschen's results suggesting that cleveite gives off two gases, each with three series of lines.

Geologists, natural philosophers generally, have been familiar with the mineral world for a considerable number of years. What have they been doing all the time? They have confined their attention too exclusively to the contained metals, and have neglected the gases; whereas it now seems that if they had been less careful of the metals, and had studied the gases, it would have been very much better for our nineteenth century knowledge. And that is the moral of my simple story.

J. NORMAN LOCKYER.

[1] In the many comparisons I had to make, I soon found the inconvenience of not having a name for the gas which gave 667, 501, and other lines. When, therefore, Profs. Runge and Paschen, who had endorsed my results, and had extended them, called upon me, I thought it right to suggest to them that, sinking the priority of my own results, we should all three combine in suggesting a name. Prof. Runge (under date October 20) wrote me, "the inference that there are two gases is a spectroscopical one, being based on the investigation of the 'series.' Now, though we think this basis quite sound, we must own that the conclusion rests on induction. . . . For this reason we do not want to give a name to 'gas X.'" I have so far suggested no name, though Orionium and Asterium have been in my mind.

SCIENCE TEACHING IN SECONDARY SCHOOLS.

THE School Syllabus of Chemistry and Physics, which has just been put forward by a Committee of the Incorporated Association of Headmasters, is an attempt to indicate to those who are dissatisfied with the ordinary course of qualitative analysis, the lines on which the practical study of science may be made more profitable.

It is a great misfortune that in constructing a syllabus for use in schools, one has constantly to keep in mind its relation to the examination fiend. A short notice upon this syllabus, in this journal on Jan. 16 (p. 262), contained the observation that the teaching in schools is governed entirely by the examinations; and nothing could be more sadly true, nor a greater hindrance to a more rational system of teaching.

A syllabus may be perfect of its kind, but school authorities do not ask "How will it develop certain powers?" but "How will it examine?" and they base their estimation of it upon the answer to this question; while the relative value of the two in the eyes of parents may be judged from the fact that many a parent who grumbles at having to pay £6, £7, or £8 a year for his son's education, willingly pays a 30*s.* or 40*s.* fee for the examination which tests it. As a matter of fact, if £7 be a fair sum to pay annually for a boy's education, an examination at the same rate would be rather overpaid with half-a-crown.

Our whole system of examinations rests upon a wrong basis; it assumes that the value of a boy's education depends upon how much he has learnt, whereas the true criterion is how he has learnt it. The climax of absurdity is reached in those examinations which test the knowledge of a language, as French or German, by the knowledge a pupil has of one specified book; it is true that some of our examining bodies have attempted to meet this difficulty by setting books of an extremely uninteresting character, in order that the pupil's attention, not being able to find interest in the subject-matter, may be solely occupied with the language, but that is a digression.

Those who have taught science in schools have long been dissatisfied with the ordinary course, and have felt the necessity of a change; but the question, "What change?" has hitherto been asked in vain. In answering this question, it is important to bear in mind that we do not want to train boys to be chemists or physicists; that is the part of the technical institute, and a system of instruction which may be excellent for producing trained chemists may be extremely unsuitable for developing the latent powers of children.

The chief principles which it is hoped to introduce by means of this syllabus are as follows.

The insistance at the outset of *measurement* as the chief factor in scientific work, and as the basis of reasoning. It is not too much to say that all scientific reasoning is the outcome of measurement, but measurement has been conspicuously absent hitherto from our school courses, and the balance, which is often stated to be the instrument of precision of chemistry, has never yet had its due place in the chemical teaching. The mental realisation of the value of numbers, and what a theologian would call an "experimental faith" in arithmetic, are important results from this kind of work. The figures in an arithmetic book, however they may profess to deal with concrete substances, represent to a boy's mind only abstract ideas, which, by means of skilful juggling, may be made to produce certain "answers," but the answers convey to him no meaning. A good instance of this is found in his view of decimals; any average boy will tell you that a mistake in the first or second decimal place causes his sum to be wrong, but the misplacement of a decimal point is not worth considering; in practice the former error is a trifle, the latter causes a grotesque absurdity.

Another point of value in the syllabus is that it teaches a boy to perform experiments with a definite purpose, and to suggest experiments himself—to put, in fact, questions to nature. Herein lies its great superiority to qualitative analysis, which, as practised in most schools, is scarcely superior to the setting of puzzles. Any boy who has worked intelligently through this course can scarcely fail to have a much better idea of the problems which natural science has to attack, and of the reasoning which is brought to bear upon them, than one who has obtained a greater amount of knowledge from text-books and lectures.

A valuable lesson which may be learnt from this syllabus is that of writing down in good English prose a systematic account of any experiment done, and of the conclusions to be drawn from it. The lesson, in fact, becomes largely a literary one. The faculty of doing this is extremely rare amongst boys leaving school, because it has hitherto never been cultivated, and the ordinary courses of analysis, whether quantitative or qualitative, rather discourage it than otherwise. Professors who continue our boys' education in higher colleges complain, with reason, of its absence, and, once acquired, it cannot fail to be of great advantage in every walk of life.

A great advantage to which I, as a schoolmaster, can bear witness, is the influence of this teaching upon the other work of the school; it has caused masters who teach other subjects to appreciate the value of teaching practically, and of reducing text-books to a secondary place. No consummation could be more devoutly to be wished especially in view of the present colossal output of worthless text-books, which boil down every difficulty and compress all knowledge into a few pages; and the general value of which is well instanced by the advertisement of a French grammar now before me, which states that several school masters and mistresses have given very successful lessons out of this book, *without any previous knowledge of the language.*

I have omitted some of the more obvious advantages of this syllabus, the importance of a definite connection between lectures and laboratory work, the training in manipulation, and the development of step-by-step reasoning, and I pass on to consider some of the objections which have been urged against this syllabus.

Although there have not been wanting objectors to the syllabus, especially the chemistry part of it, I have found it difficult to induce them to formulate objections definitely; and when they have done so, many of the objections raised appear to me to be positive advantages. Amongst these I class the following statements.

That it requires closer attention and more work on the part of the teacher, that it necessitates small classes, that it is unsuitable for examination.

The only objections I have ever heard which appear to me to require answering are the three following:

(1) That many of the experiments are unsuitable for boys; one experiment which has been specially mentioned in this connection, and which, I own, startled me before I tried it, is the production on burning hydrogen of sufficient water to estimate its physical constants. The best answer to this is, "Try it." If a boy can be taught to burn a jet of hydrogen at all, nothing is easier than that he should burn it under a retort kept cool by a stream of water running in at the tubulure and out at the neck; and if eight or ten couples are doing this, it is surprising what a large amount of water can be collected in half an hour. A little experience will enable any teacher to simplify the experiments to the capacity of his class.

(2) That the results of experiments are often so far from accurate as to be worthless. This objection proceeds from an insufficient appreciation of the aims of the work. It is true that a schoolboy will not obtain Stas's numbers, but he will obtain numbers which show a remarkable concordance when the average of all the experiments done by various members of the class are taken, and which will enforce upon his mind the law of definite proportions.

A boy may find that H and O combine in the proportion of 1 : 7·3, that chalk contains 42·8 per cent. of CO_2, or that phosphorus and oxygen combine in the proportion of 100 : 113; but when eight or ten couples have obtained similar figures, the lessons of the definiteness of the reaction, and the importance of careful quantitative work, may be learnt as thoroughly as from more accurate results.

(3) Is it not a waste of time for a boy to laboriously work out a fictitious discovery when he could learn it in five minutes by being told it, or by reading a book? *Yes*, if committing facts to memory be the desired end; but *No*, if the end be to form habits of inquiry and of thought, to understand scientific reasoning, and to prepare the mind for dealing with problems where the text-book is not available.

If I may be allowed to speak of my own experience as a teacher, I have now taught on these principles for some time, and I can positively say that I believe this system to be infinitely superior to the ordinary method of lecturing on the non-metallic elements and on physics, and setting qualitative analyses as practical work. The only examination of which I had experience lately is the London University Matriculation, which has recently introduced practical questions; my boys have always gone up with confidence, and found these distinctly easy, a matter in which others with whom they compared notes have not agreed with them. I have never had a failure in chemistry.

At the same time, I must insist upon the point that I do not believe that this kind of knowledge can be properly tested by examination; the true criterion of the success of this syllabus must be the opinion of those professors who carry on the scientific education of our boys after they have left us. I have received a large number of letters from various professors, in which they complain bitterly of the present preparation of most of their students, and state that they believe such changes as those contemplated by this syllabus would be of the greatest value. One of these I feel justified in quoting.

"I am a very strong advocate for change in the method of teaching science in schools. The method that usually prevails is, in my opinion, worse than barren. Not only is no satisfactory foundation laid for future teaching, but bad and slovenly habits of mind and manipulation are formed, with the result that a large portion of my work and that of my colleagues consists in the attempt (too often futile) to eradicate these habits."

The syllabus, if it has a fault, is that it is too long for an elementary course. Several of the Committee were of opinion that a course on mechanics, to lead up to the construction and use of a balance, would be sufficient; but it was pointed out that the Science and Art Department would not accept any syllabus which did not include a complete course of statics. This will explain why the syllabus includes an amount of statics out of all proportion to the other subjects; it is a sacrifice to the examination fiend; personally I should leave this work for a more advanced course.

I can, in conclusion, heartily recommend to all science teachers to try this syllabus with their classes of beginners, not following it slavishly, but adopting those points which appeal to their experience.

CHARLES M. STUART.

NOTES.

LORD KELVIN reaches this year his jubilee as Professor of Natural Philosophy in the University of Glasgow. The event will be recognised by a joint celebration, in which the City, University, and students will take part, on June 15 and 16. It is anticipated that delegates and addresses will be sent from numerous home and foreign Universities to express the esteem in which the distinguished investigator is held.

THE date fixed for the next "meeting for discussion" at the Royal Society is April 23, when the subject will be "Colour Photography," and the discussion will be opened by Prof. Lippmann.

THE Bakerian Lecture will be delivered before the Royal Society on the 20th inst., the lecturer for the year being Prof. Roberts-Austen, and the subject, "The Diffusion of Metals." The Croonian Lecture will be given, probably on March 12, by Dr. A. D. Waller, who has chosen for his subject, "Electrical Changes in Isolated Nerve."

THE Odessa correspondent of the *Times* says that the Russian Government will send a special scientific expedition to observe the total eclipse of the sun on August 9. The expedition will be in charge of three astronomers from the Nikolas Observatory at Pulkova, and leaves Odessa in May, by one of the cruisers belonging to the Russian Volunteer Fleet Committee, for Vladivostok, whence it will go near the mouth of the river Amour for observations. The Committee has agreed with the Government to convey the party from Odessa to Vladivostok and back again to Odessa free of charge.

A COMMITTEE was formed in Paris, in December last, to obtain the means for erecting a monument to Pasteur, by international subscriptions. In the furtherance of this resolution a circular has been widely distributed, appealing for funds, and asking for the organisation of local committees, so that the monument in Paris shall be worthy of the man whose labours against disease and death it will commemorate. The French Comité de Patronage includes the President of the Republic, many of the Ministry, and a large number of distinguished scientific men; while the Commission which is organising the memorial is composed of members of the Council of the Pasteur Institute. It is to be hoped that a generous response will be made to the committee's appeal for subscriptions and assistance.

A CIRCULAR has recently been issued to draw the attention of biological professors and lecturers to a course of instruction in Marine Biology which has been organised in connection with the Plymouth Laboratory of the Marine Biological Association. This course, which is intended to be supplementary to the ordinary academical courses in Comparative Anatomy, will be conducted during the forthcoming Easter vacation between March 23 and April 24, and will be superintended by Mr. W. Garstang, Fellow and Lecturer of Lincoln College, Oxford, and formerly naturalist on the staff of the Plymouth Laboratory. England has undoubtedly been behind the times in the lack of any organised arrangements by which biological students could be enabled to enjoy the various advantages which a well-equipped marine laboratory affords for the study of some of the more fascinating aspects of animal life; and it is to be hoped that this new departure in biological education in England will meet with the success and encouragement which it deserves.

JUDGING from a Reuter's telegram, a meteorite which fell at Madrid on Monday produced exceptionally striking effects. Reports of remarkable meteoritic falls are comparatively rare, so we give a fairly full transcription of the telegram:—"At half-past nine this morning an aerolite of considerable size exploded in the atmosphere above Madrid. The phenomenon was accompanied by a vivid glare of blinding light, followed by a loud report, which caused a general panic among the people. All the buildings in the city were shaken, and many windows were smashed by the concussion. The sky was clear and the sun was shining brightly at the time of the explosion, all that was visible in the heavens being a white cloud bordered with red, which was travelling eastward at a great rate, leaving behind it a train of fine, light dust. The panic was general throughout the city. Many shopkeepers closed their establishments. Not only were buildings shaken, but at least one house is known to have collapsed. At the United States Legation a partition wall fell in, and many of the windows were shattered, but no other damage was done to the building. A great many houses throughout the city sustained similar damage, partition walls being shaken down by the force of the explosion. The excitement in the city, and especially in the suburbs, has not yet calmed down. The explosion was heard over a distance of several kilometres from Madrid. At Guadalajara, a town about forty-six miles from the capital, the explosion was very strongly felt." The following official communication has been issued from the Madrid Observatory:—"At 9.29 this morning a strong light was observed proceeding from a small cloud moving from the south-west to the north-east. A minute and a half later a terrific report, followed by several others of less intensity, occurred, accompanied by a shaking of the ground and of buildings. A red-tinted cloud was for a long time visible in the east. The directors of the Observatory suppose that the phenomenon was caused by an aerolite. Owing to the time that elapsed between the observation of the explosion and the report, the aerolite must have burst at a great distance from the earth." The note adds that no traces of the meteorite have yet been found. Nevertheless, it is reported that several small pieces of greyish stone, still warm, have been picked up in Madrid.

GREAT interest has been aroused in France by the discoveries of Prof. Röntgen, and experiments have been at once made confirming and extending the original results. Last week's number of the *Comptes rendus* contained further work by M. Jean Perrin on the properties of the kathode rays (see *ante*, p. 298), showing that they are not identical with the Röntgen rays. The current issue (February 3) contains papers on this subject by MM. L. Benoist and D. Hurmuzescu, A. Nodon, V. Chabaud, and G. Moreau. The first of these gives an account of a most remarkable property of the rays, that of completely and rapidly discharging an insulated charged electroscope, the effect being produced more rapidly if the charge borne by the gold leaves is a negative one. This phenomenon is not in any way interfered with by an aluminium screen, although the latter protects the electroscope perfectly from both light and electricity. The note by M. Nodon further emphasises the difference between these rays and the ultra-violet rays. An ordinary dry plate wrapped in several thicknesses of blackened paper was exposed at a distance of 40 c.m. from a powerful arc lamp (20 amperes) for fifteen minutes; on developing, the plate was unaffected, although under the same conditions the Röntgen rays produced a very marked result. The latter also were found to pass with equal facility through differently coloured screens of the same material. The transparency of metals to the X-rays is the subject of the communication by M. Chabaud. Fourteen metals and alloys were examined by the photographic method, the results being substantially identical with those published by Prof. Röntgen in his original paper; results which were obtained very simply by the use of a phosphorescent screen. M. Chabaud makes no reference to these earlier experiments on the same subject. The two metals most opaque to these rays are those of the highest atomic weights, platinum and mercury, the most transparent being that with the lowest atomic weight, aluminium. M. Moreau dispenses with the Crookes' tube altogether, and uses the brush discharge of an induction coil; the curious result being obtained, that if the box containing the plate be placed normally to the brush, there is no effect, whilst placed parallel to the brush, strong, clear negatives are produced. No explanation of these experiments is attempted.

THE same number of the *Comptes rendus* also contains a second note by M. G. Le Bon, on photography with "dark light." By placing a sensitive plate under a negative, covered with a metallic plate 0·5 mm. in thickness, and exposing to the light of a lamp, good images are obtained on development, especially if a piece of lead is bent back over the frame, so that the whole printing frame is in a sort of metallic box. M. G. H. Niewenglowski mentions that these results can be obtained without any lamp at all, and hence suggests that they must be due to luminous energy stored up in the negative. But further experiments by M. Le Bon have completely eliminated this source of error, as the same results are obtained with negatives which have been previously submitted to blank experiments in the dark. From the point of view of Maxwell's theory of light, rays which can pass through 0·5 mm. of copper must differ essentially from ordinary light, and M. Le Bon proposes to next examine within what limits these dark rays submit to the laws of refraction, and how far they are affected by a magnetic field.

IT is announced that Dr. Kitasato, of Japan, has succeeded in inoculating for leprosy, and that the disease is curable.

THE formal presentation of the portrait of Mr. W. Carruthers, F.R.S., to the Linnean Society, was made at its meeting on February 6, by Sir W. H. Flower, on behalf of the subscribers.

THE Annual Congress of the French Association for the Advancement of Science will be held this year at Tunis, between the 1st and 11th of April. The Botanical Society of France will also hold its Extraordinary Session at the same place, and about the same time.

A COURSE of six lectures to working men, on "Fermentation," by Dr. W. P. Wynne, will be given in the Lecture Theatre of the Museum of Practical Geology, Jermyn Street, commencing on Monday, February 24.

A CONGRESS of Natural History and Scientific Societies of the South-East of England will be held at Tunbridge Wells on Saturday, April 25, under the presidency of the Rev. T. R. R. Stebbing.

THE practical extinction of the buffalo, or American bison, is very forcibly shown by an article recently published, which states that 300 dols. is now refused for a good buffalo robe, such as could at one time early in the seventies have been bought for a dollar, or even less.

WE are requested to state that the arrangements are now complete for lighting in the evening the Southern Galleries of the South Kensington Museum on the west side of Exhibition Road, which contain the collections of machinery and naval models. These Galleries will be open free to the public from February 17 on three evenings a week—Mondays, Tuesdays, and Saturdays, till 10 p.m.—in the same manner as the main building.

ANOTHER of the wonderful potentialities of the Niagara water-power is its application to the new process of Blumenberg for the manufacture of chlorate of potash by the action of the direct electric current upon tanks of potassium chloride. The electric current is at a remarkably low voltage. Important by-products are also obtained. Operations will begin next July. The Alkali Union, which controls the world's output of chlorate of potash, has reduced the price from 17 to 10 cents a pound, in anticipation of this undertaking; but the new manufacturers will probably undersell them in all the markets of the world.

THE new illuminant, acetylene, seems to be notable for its efficiency and the cheapness of its production by the electrical manufacture of calcic carbide, from which it is evolved by the mere application of water. Calcic carbide is already made at Spray, North Carolina, at a cost of 20 dols. per ton, by the alternating electric current passed through a mixture of powdered coke and lime. Works have been erected at Niagara which will produce the calcic carbide at 10 dols. a ton, beginning about the middle of this month. One ton of the carbide yields 11,000 cubic feet of acetylene gas, which has many times the illuminating power of common illuminating gas. The light, moreover, is clear and "solid"—that is, it has not the unilluminated centre of an ordinary gas-light. Acetylene gas is readily solidified, and may be conveniently distributed in cylinders, like carbon dioxide and other gases.

THE Geographical Survey of the United States has issued the report of a party of department geologists who recently made an extended investigation and survey of the Cripple Creek region. The mining district is thirty miles west of Colorado Springs, and seventy-five miles south of Denver. It covers an area of six miles in length by five and a half miles in width, between the two main ranges of the Rocky Mountains in Colorado. As long ago as 1874 it was thought that a big gold vein had been discovered near Mount Pisgah; and the second mining excitement was in 1884; but during these two periods not one of the really valuable ore bodies which are worked to-day was discovered. Rich ore was discovered in 1890, and the rush of miners began in 1892. The first gold in the district was produced in the latter part of 1891, and since that year the output has steadily increased, being variously estimated at 5,000,000 dols. to 7,000,000 dols. The ores of the Cripple Creek district are almost exclusively gold ores, with seldom silver enough to be important. The gold occurs both in a coarse and a fine condition, sometimes in particles so small as to be invisible to the naked eye, and at other times in plates or spongy masses from an eighth to a quarter of an inch in diameter. It is rarely clear and bright, and is generally coated with a thin rusty film of a yellowish-brown compound.

A GENERAL meeting of the Members of the Federated Institution of Mining Engineers will be held in Sheffield on Wednesday, February 19. Arrangements have been made for visits to cutlery and silverplate works and the technical schools in Sheffield on February 19, and to collieries and ironworks on the following day. The following papers are down for reading: "The Eastern Limits of the Midland Coal-field," by Prof. Ed. Hull, F.R.S.; "Electric Welding," by Mr. T. Scott-Anderson; "The Vibromotor as applied to the Screening of Small-coal," by Mr. Emerson Bainbridge, M.P.; "The Matabele Gold-fields," by Mr. F. G. Shaw; "Photography in the Technology of Explosives," by Mr. Alfred Siersch; "Lead and Lap of Winding and other Engines," by Mr. Hargrave Walters; "The Elliott Coal-washer," by Mr. J. Platt.

THE *Comptes rendus* for February 3 contain a paper by M. Maurain on the measurement of the energy dissipated in iron due to hysteresis. The amount of heat generated in a sample of iron when subjected to an alternating magnetic field is measured by enclosing the iron inside a cylindrical glass reservoir furnished with a capillary tube. This cylinder is filled with alcohol, and placed within a long magnetising coil. The reservoir being carefully protected from the heat generated by the passage of the current through the magnetising coil, the quantity of heat developed in the iron is deduced from the amount of the expansion of the alcohol. The development of heat due to Foucault currents is almost entirely prevented by using a bundle of very fine iron wires. The curve which gives, as a function of the time, the position of the surface of the liquid in the capillary tube during an experiment, consists of a straight line until the losses of heat due to radiation and conduction through the walls of the reservoir become appreciable when the line becomes curved. A preliminary experiment having indicated the form of this curve; in the final measurement care is taken that the observation does not extend beyond the straight part of the curve. The author finds that the quantity of heat developed during one cycle diminishes as the rapidity of the alternations increases. This diminution, however, becomes smaller and smaller as the frequency increases, and seems to be practically independent of the magnitude of the magnetising force. The author considers that the diminution observed is probably due to the screening action of the outside layers of the iron, and some experiments on which he is now engaged seem to favour this view.

THE number of the *Bulletin of Miscellaneous Information* of the Royal Gardens, Kew, for January, contains some interesting particulars of the effects on the Gardens of the long and severe frosts of the early months of 1895. These were in some respects different from what might have been anticipated from previous experience. Thus, while some herbaceous plants and shrubs reputed to be nearly hardy have perished, the laurustinus, the bay, and the arbutus escaped almost unharmed, as also did the evergreen oaks. "Alpines" suffered severely, though of course the cold was not so intense as the ordinary winter temperature to which many of them are exposed in their native habitat; but there they are protected through the winter by a covering of snow. The bamboos in the open ground were but little injured, while the pampas grass was largely killed. Of our native plants, the ling and the gorse were greatly damaged. Perhaps the greatest destruction was among the bulbs, of which very large numbers were entirely killed, especially the narcissi and hyacinths. In the same journal the number of visitors to the Gardens during 1895 is estimated at 1,407,369. This is about 30,000 more than the number for 1894, but somewhat less than the average numbers for the last ten years.

WE have received from Mr. J. Eliot, F.R.S., Meteorological Reporter to the Government of India, a preliminary discussion of certain oscillatory changes of pressure of long period and short period in India, being part ii. vol. vi. of *Indian Meteorological Memoirs*. The author points out that the discussion of pressure conditions in India requires that the observations should be of a high standard of accuracy, and for many years the greatest attention has been paid by the India Meteorological Department to the methods of observation, and to the critical comparison of the data. The first part of the paper deals with the normal conditions of pressure, and shows that the ordinary seasonal changes are very regular and uniform, while the abnormal conditions, such as are associated with the occurrence of storms, are, as a rule, very slightly marked. The largest regular oscillation is the annual change; the readings are highest in January, and fall until June, after which they rise until the end of the year. The amplitude of this change is smallest in Southern India, and increases with latitude. The smallness of the daily pressure changes is remarkable; only about five per cent. of the changes exceed a tenth of an inch. The second part of the paper deals in great detail with oscillations of pressure differing from the annual and diurnal changes, and which appear to be associated with atmospheric movements common to the whole of Southern Asia and the adjacent seas. In the long-period oscillations, the transfer of air across the equatorial belt tends during one half of the year to give an accumulation of air and high pressure over Central and Southern Asia, whilst in the other half of the year the air is drained away. The short-period oscillations are less regular than either the diurnal or annual, and approximately average four days in length. They appear to be common to the whole Indian area, and occur almost simultaneously; they are apparently not due to the transmission of waves in a horizontal direction, but possibly to waves of rarefaction and condensation, transmitted slowly upwards and downwards in the atmosphere, caused by variations of temperature. Although the discussion is only preliminary to a future paper, it teems with instructive matter which will well repay careful study.

A GOOD portrait of Lord Kelvin, reproduced by collotype, is presented as a supplement to *Industries and Iron* of February 7.

THE newly-established Société de Spéléologie of Paris has sent us the first three numbers of its quarterly journal, *Spelunca*. Accounts of caverns in various parts of the world are gathered together in the journal, several of them being illustrated by maps and other figures.

THE annual report of the New South Wales Railway Commissioners for 1894–95—being the final one for the original term of their appointment—contains, in addition to the ordinary very detailed statements, a full account of the extensive alterations and improvements made in the railway system since 1888. The report is illustrated by maps, photographs, and diagrams; and the statistical tables are admirably compiled.

THE January number of the *American Geologist* contains an account of the late Prof. J. D. Dana, with a portrait and bibliography. Mr. Warren Upham discusses the flow of glacial ice, with especial reference to the views of Messrs. Fletcher and Deeley, mentioned in these columns about a year ago. A description of cases of the flotation of sand, by Mr. F. W. Simonds, and an essay on the use of Palæontology as "The Timepiece of Geology," by Prof. Claypole, are among the other contents.

THE *Annuaire* of the Municipal Observatory of Montsouris for this year has just come to hand; the observations—meteorological, chemical, and bacteriological—tabulated and discussed in it, refer to the year 1894. Another *Annuaire* which we have received is that of the Belgium Académie des Sciences, des Lettres, et des Beaux-Arts. A full notice of the life and mathematical works of Eugéne Charles Catalan, with a portrait, appears in this annual of the Brussels Academy.

WITH the motto *Magnus magnes ipse est globus terrestris*, the new international quarterly journal, *Terrestrial Magnetism*, the forthcoming publication of which was announced in these columns a few weeks ago, has made its appearance. The articles in the journal are: "On electric currents induced by rotating magnets, and their application to some phenomena of terrestrial magnetism," by Prof. A. Schuster; "Die vertheilung des Erdmagnetischen Potentials in Bezug auf Beliebige Durchmesser der Erde," by Dr. A. Schmidt; and "Halley's Earliest Equal Variation Chart," by Dr. L. A. Bauer, the map being reproduced in facsimile. There are also letters, notes, and reviews dealing with terrestrial magnetism and cognate subjects. The journal is published under the auspices of the Ryerson Physical Laboratory, University of Chicago.

WE have had upon our table for some time a copy of Napier's celebrated and rare work on the construction of logarithms—"Mirifica Logarithmorum Canonis Constructio"—reprinted in facsimile by M. A. Hermann, 8 Rue de la Sorbonne, Paris. Napier's discovery of logarithms was announced in his "Mirifici Logarithmorum Canonis Descriptio," published in 1614; but the explanation of the method by which the logarithms were calculated appeared in the "Constructio," a posthumous work not issued until 1620. To this work, which M. Hermann has now reprinted, Henry Briggs, who was one of the first to recognise the value of logarithms, appended some notes. The reprint will be valued by all students of the history of mathematics.

THE Geological Society of London has just published a list of the geological literature added to its library during the year 1895. As compared with the only previous issue in this form, we note that while the price remains unaltered, the volume has swollen to nearly threefold—an increase only partly accounted for by the fact that the present list represents a whole year's additions as against a half-year's in the previous one. The omission of maps from the list, to which we drew attention when the previous list was published, has been repaired; maps are entered under the names of the authors responsible for them in the general list, but there are cross-references under "Maps" in the subject-index. The publication will be found a most useful one by all geologists.

THREE papers were read at the recent meeting of the Institution of Mechanical Engineers, viz.: "Telemeters and Range-Finders for Naval and other Purposes," by Profs. Barr and Stroud; "Calculation of Horse-power for Marine Propulsion," by Lieut.-Colonel Thomas English; "Notes on Steam Superheating," by Mr. William H. Patchell. The paper by Profs. Barr and Stroud was confined to a description of two instruments, viz. (1) the range-finder which is now in use in the navies of this and many other countries; and (2) a small hand instrument, identical in principle with that for naval use, but much more portable and much simpler in its details. Lieut.-Colonel English's paper called attention to a method of calculating, from the results of a single sea-trial of one ship, the horse-power necessary to propel another ship, of the same type, at any required speed. By this method it becomes practicable, with the ordinary appliances of a shipyard, to approximate closely to results which could otherwise be obtained only by the use of the refined apparatus of a model tank. Mr. Patchell described a number of superheaters, and gave the results of tests with them.

THE additions to the Zoological Society's Gardens during the past week include a Klippspringer (*Oreotragus saltatrix*, ♂) from Nubia, presented by Commander Alfred Paget, R.N.; an Indian Wolf (*Canis pallipes*, ♂) from India, presented by Mr. Duncan Darroch; a Barn Owl (*Strix flammea*), British, presented by Mr. Bernard R. White; a Gould's Monitor (*Varanus gouldi*) from Australia, presented by Mr. Arthur R. H. W. Leach; a Sharp-nosed Crocodile (*Crocodilus acutus*) from Jamaica, presented by Mr. Arthur P. Cohen; a West African Love Bird (*Agapornis pullaria*) from West Africa, presented by Mrs. Roberts; an Alpine Marmot (*Arctomys marmotta*), European, a Yellow-headed Conure (*Conurus jendaya*) from South-east Brazil, deposited; a Crested Grebe (*Podiceps cristatus*), a Curlew (*Numenius arquata*), British, purchased; a Malaccan Parrakeet (*Palæornis longicauda*) from Malacca, received in exchange; two Rufous Rat Kangaroos (*Hypsiprymnus rufescens*, ♂ ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE ASTROPHOTOGRAPHIC CATALOGUE.—At the last meeting of the Royal Astronomical Society, the Astronomer Royal gave some particulars relating to the progress at Greenwich of the international photographic star catalogue. A special staff for dealing with this work has been organised under Mr. Hollis, and already 130 of the plates taken for the catalogue have been measured. It is estimated that about 180 plates can be measured, and 160 of them reduced in the course of a year, so that at this rate the section allotted to Greenwich, comprising about 150,000 stars, will be completed in five or six years. Assuming that the other sixteen co-operating observatories are proceeding equally well, the world will soon be in possession of a colossal catalogue, comprising between two and three million stars.

ORBIT OF α CENTAURI.—Dr. Doberck, of the Hong Kong Observatory, has revised the elements of the orbit of α Centauri, which he determined in 1877, with the following results (*Ast. Nach.*, No. 3330).

☊	...	$25^\circ\ 25'$	T	...	1876·02
λ	...	$52^\circ\ 53'$	e	...	0·51184
γ	...	$79^\circ\ 14'$	P	...	79·123 years
μ	...	$4^\circ{\cdot}54987$	a	...	$18''{\cdot}450$

The sum of the masses, as determined from these elements, is 2·3780 times the mass of the sun. Taking the parallax as $0''{\cdot}75$ the major semi-axis is 24·60 times the earth's distance from the sun, so that the distances between the components are about the same as those of the outer planets from the sun in our own system. If the diameters of the stars are not very different from that of our sun, each would appear from the other as a mere star to unaided vision, the distance being too great to show a disc. An ephemeris up to 1920 is given in the paper, as well as a comparison of observations with the places calculated from the adopted orbit.

COMETS OF SHORT PERIOD.—From an interesting article on comets of short period, by Mr. W. E. Plummer (*Knowledge*, February), we extract the following table, indicating the comets which may with reasonable certainty be expected to reappear.

They are arranged in the order of the mean distances from the sun.

Name.	Period in years.	Date of last Perihelion passage.	Approximate date of next return.
Encke	3·303	1895, Feb. 4	1898, May 26
Tempel	5·211	1894, April 23	1899, July 10
Tempel-Swift ...	5·534	1891, Nov. 14	1897, May 28
Winnecke	5·818	1892, June 30	1898, April 25
Finlay	6·627	1893, July 12	1900, Feb. 26
D'Arrest	6·691	1890, Sept. 17	1897, May 27
Wolf	6·821	1891, Sept. 3	1898, June 30
Faye	7·566	1896, March 19	Now visible.

The mean distances of the comets from the sun range from 2·218 to 3·854, but the aphelion distances do not vary so greatly in proportion—a fact which suggests the controlling influence of Jupiter. It is remarkable that such a small number of regularly returning comets seem to be permanently attached to our system.

EFFECT OF SPOTS ON SUN'S DIAMETER.—Observations of the sun's diameter, made in the latter half of last year by J. Sykora, of the Charkow Observatory, have led to a result which may be of considerable importance if established by further investigations (*Ast. Nach.*, No. 3330). The observations were made with a 6-inch refractor by projecting the image of the sun together with that of the micrometer wires. The diameter measured in the direction of the points of appearance or disappearance of spot groups was found in the great majority of cases to be greater than the diameters in neighbouring parts of the sun as measured on the same days. Some of the results are as follows, the first column giving the diameter in the direction of spot groups, and the other two showing adjacent diameters:

	m. s.	m. s.	m. s.
June 22 ...	2 8·62 ...	2 8·38 ...	2 7·97
July 5 ...	8·37 ...	8·04 ...	8·21
,, 12 ...	8·30 ...	8·27 ...	8·27
Sept. 5 ...	8·52 ...	8·25 ...	8·44
,, 9 ...	8·41 ...	8·29 ...	8·36

It is concluded that although the spots themselves may be depressions, they produce an elevation of the surface of the sun in the regions where they are formed.

THE SPECULATIVE METHOD IN ENTOMOLOGY.

THE annual general meeting of the Entomological Society of London was held on January 15, the President, Prof. R. Meldola, F.R.S., being in the chair. After referring to the affairs of the Society and to the great literary activity of English entomologists during the past year, the President called attention to Mr. Oswald Latter's discovery of the secretion of potassium hydroxide by *Dicranura vinula*, &c., and to Mr. F. Gowland Hopkins's researches on the pigments of Pierine butterflies. The address then proceeded as follows:—

The association of chemistry and biology in researches such as those to which I have drawn attention, has suggested a comparison between the methods of research in vogue in the two great departments of science of which these two subjects are respectively typical. All science necessarily begins with observation or experiment, *i.e.* with ascertained facts, and it is perhaps unnecessary to assert that no mere collection of facts can constitute a science. We begin to be scientific when we compare and coördinate our facts with a view to arriving at generalisations on which to base hypotheses or to make guesses at the principles underlying the facts. Having formed the hypothesis we then proceed to test its accuracy by seeing how far it enables us to explain or to discover new facts, and if it fails to do this to our satisfaction we conclude that our guess has been a bad one and requires modification or replacing by a better one, *i.e.* by one more in harmony with the facts. I take it that the course of progress is the same in so far as these fundamental methods are concerned in both departments of science, the physical and the biological. It is possibly a matter of individual opinion as to how large a body of facts should be accumulated before we attempt to draw any general conclusions. There can be no doubt that the requirements of one branch of science cannot be measured by those of another branch to which it has no near relationship. But however large the number of facts, and however cautious or conservative the worker may be, it is an established doctrine taught by the whole history of science, that real progress only begins when we go to seek for facts armed with at least the suggestion of a principle if not with a complete theory based on facts already accumulated by observation or experiment. This is the whole difference between scientific observation or experiment and mere random or haphazard observation. A naturalist of the old school, William Swainson, writing in 1834,[1] speaks of the "observance of nature, without making any attempt to generalise the facts so acquired," as "a mere amusement, fascinating indeed, and even useful, but totally disconnected with the objects of philosophic science." Now I venture to think that entomology in this country has been retarded in its development for want of a little more of this "philosophic science"; by an unwillingness on the part of our most active workers to give rein to the imagination—by an overcautiousness which is damping to the speculative faculty. There are no doubt many present who will not agree with this view, but I claim indulgence while I state my case in its support. It will, I think, be conceded that we have passed beyond the mere fact-collecting stage. It appears to me that in entomology we have arrived at a state where we are suffering from a plethora of facts; if we are not in a position to explain everything connected with the development, life-histories, instincts, classification and distribution of insects as a class of animals, we are at any rate in a position, speaking paradoxically, to know what we want to know, and I do not see how we are going to advance unless a more generous use is made of hypothesis as a scientific guide. It is this point which I desire to urge and to show that there is no real danger in boldly facing what the late Dr. Romanes aptly calls the bugbear speculation.

In the first place, with respect to the physical sciences, there is abundant justification for the view which I am advocating. We have there long ceased to collect random facts; observations and experiments are suggested by hypothesis. That prince among experimental philosophers, Michael Faraday, was wont to say: "Let us encourage ourselves by a little more imagination prior to experiment." The state of affairs is well summed up in one of the latest works on chemistry in which the author, in introducing the fundamental principles of modern investigation says:

"The history of the exact sciences teaches us that we may discover new laws of nature in two essentially different ways, one of which may be designated as the empirical, the other as the theoretical. Thus in one way by suitable observations, one collects abundant material . . . and then by a repeated and purely empirical grouping of the data so obtained, he seeks to approach the desired goal. . . . The second way, on the other hand, leads from suggested conceptions regarding the nature of certain phenomena, through pure speculation to new information, the correctness of which must be determined by a subsequent research."[2] One other recent utterance by my colleague, Dr. W. M. Hicks, the President of Section A at the last Ipswich meeting of the British Association, will serve to give us a glimpse into the spirit of progress in pure physics: "By our imagination, experience, intuition, we form theories; we deduce the consequences of these theories on phenomena which come within the range of our senses, and reject or modify and try again. It is a slow and laborious process. The wreckage of rejected theories is appalling; but a knowledge of what actually goes on behind what we can see or feel is surely, if slowly, being attained. It is the rejected theories which have been the necessary steps towards formulating others nearer the truth."[3]

And now let us consider how far these methods, recognised as valid in the physical sciences, are applicable to the biological sciences, of which entomology constitutes a branch. Of course, I am not claiming for our subject the position of an exact science, and to suppose that it could be advanced by purely deductive methods would be absurd. But I am endeavouring to hold the balance between a more liberal use of the speculative method, on the one hand, and the deadening influence of refusing to speculate at all, on the other hand. I am putting forward a plea for an increased use of the imagination, because I hold that

1 'Preliminary Discourse on the Study of Natural Science," p. 51.
2 "Theoretical Chemistry," by Walter Nernst, translation by Prof. Palmer, 1895, p. 2.
3 Address to the Mathematical and Physical Section of the British Association, Ipswich 1895.

the time has arrived when this may—nay, must be allowed, if our science, with its immense wealth of raw material, is to take that rank to which it is entitled among the departments of modern biology. If, as is undoubtedly the case, the speculative method has been found fruitful in other fields of natural history, it behoves us as co-workers in the great battle for truth to re-examine our weapons—to ask ourselves seriously whether the time and energy of our most active workers is being utilised in the best way for the advancement of knowledge.

To many it may appear that the use of hypothesis as a guide to investigation is so obvious, that no special advocacy is required. All I have to say, in this case, is to express the earnest wish that the Fellows of this Society who hold such a view may be numerous—the more numerous the better. I will venture to remind you, however, that my predecessor in this chair has stated, with respect to this method of handling entomological problems:

"I feel, however, for myself, and I think that others must also feel, that however great and important is the knowledge which we may ultimately attain, by endeavouring to discover the laws which govern the development, variation, and distribution of insects, the knowledge we have of the actual facts is in many cases quite insufficient to bring such speculations to a definite end. I also feel that the number of persons whose talents are sufficiently great to enable them to steer a straight course through the numerous difficulties, contradictions, and doubts which constantly surround such inquiries is very limited" (*Proc. Ent. Soc.* 1893, p. xlvi.).

I am sure Mr. Elwes will not ascribe any personal motive to me in making use of this passage, as representing the views of what may be called the conservative school of entomologists. I feel only too acutely the truth of his remark that many agree with him in this opinion; at the same time I am sanguine enough to believe that there are many who do not, and on behalf of this constituency I have felt it a duty to urge a claim for the speculative method, not as displacing the older method of collecting and recording facts altogether, but as a stimulus to more systematic investigation, rendered imperative by the general advance of biological science. For my own part, I believe that the time has gone by when every attempt at discovering natural law in the organic world by the aid of entomological observations, is to be met by this prevalent cry of *non possumus*.

If we turn to results as a measure of the value of methods, it will, I imagine, be conceded that we can show good cause in favour of theorising. I may be permitted to draw some illustrations from the Lepidoptera, the only order to which I can lay claim to some slight special knowlege, and in which our former President is a recognised authority. In the following remarks I desire most emphatically to dissociate myself from controversial matters, because my sole aim in this address is to clear the atmosphere for the more healthy use of the speculative faculty by our younger and rising workers. I wish it to be understood that in speaking of any particular hypothesis, I am not now raising the question of its soundness or unsoundness—that is, logically, a distinct issue—but I am simply adducing the hypothesis in order to illustrate the results of its introduction into modern scientific thought. I begin with the phenomena of mimicry and protective resemblance among butterflies and moths as first explained by our late distinguished Fellow and past President, Henry Walter Bates, in his memorable paper of 1861, which was followed by the well-known memoirs of Wallace and Trimen on the same subject. It will be remembered by all who are familiar with the history of the subject, that this was the first application of the theory of natural selection of Darwin and Wallace to explain a new set of phenomena. It was a speculation evolved by Bates, not when collecting in the Amazon Valley, as is generally supposed, but while looking over his specimens when he had reached London, and was pondering, at his own fireside, over the meaning of the remarkable superficial resemblances among the butterflies of different groups which he had brought home.[1]

The Batesian theory was fruitful; it carried with it the explanation of the resemblance between insects of distinct orders and of the assimilation of insects and other animals in colour and form to the objects among which they lived; it prompted further observation and experiment because more evidence was required as to the protected character of the insects which were copied; it raised the whole question of the existence of such protected species in nature, and the question has been answered so far in the affirmative, although there is still a large field for further experimental observation waiting to be explored. The facts have increased enormously since 1861, the search for new instances having been stimulated by the explanation suggested by Bates, and the systematist is now no longer in danger of being deceived by superficial resemblances.

The theory of Bates left unexplained the resemblance between species belonging to protected groups to which he had himself called attention in his original paper; an extension was required and was made by our Hon. Fellow, Fritz Müller in 1879, and as a result, whether this extension be considered valid or not—a point which I am not now raising—the systematist is now more fully alive to the superposition of external similarity upon structural resemblance due to true blood-relationship, as can be seen from the writings of Moore on the genus *Euplœa*, and of Wood-Mason and others on certain Papilionidæ. As another result of Fritz Müller's hypothesis, the question of inherited knowledge of edible and inedible species on the part of insect-eating creatures has likewise been raised, and has already led in the hands of Prof. Lloyd Morgan to some interesting experimental conclusions.

As the product of a theory we thus have a large body of real and tangible knowledge gleaned from nature! Mere casual observation would never have revealed the widespread existence of the phenomenon if the stimulus to look out for it had not come from the theoretical side.

It is not the bare record of the comparatively few cases of mimicry that constitutes the highest value of these classical memoirs—it is the speculation, the hypothesis, the suggested *cause* of the phenomenon that has given vitality to what would otherwise have been a disconnected and meaningless set of facts. But the consequences of the introduction of the theory of natural selection into the subject of insect colouration have not yet been exhausted. From the observation that the species which are mimicked are generally gaudily coloured and take no special means to hide themselves, it is but a step to the well-known theory of warning colours propounded by Wallace in 1867. That theory, in itself the outcome of a question raised by Darwin in connection with his theory of sexual selection, stimulated the experiments of the late Jenner Weir and of A. G. Butler, the striking observations of Thomas Belt in Nicaragua, the detailed researches of Weismann into the origin and meaning of the colours of caterpillars, and the later systematic series of experiments conducted by Poulton. Yet another example I will permit myself to make use of because it is one in which I have some personal interest. In considering the subject of adaptive colouration as explained by Bates and Wallace, a difficulty occurred in the case of species which are of variable colouring: I ventured to suggest, as far back as 1873, that this kind of colouring would be explicable by natural selection, if we supposed that this agency could confer a power of adaptability on the individual. At that time no mechanism could be conceived of by which such individual adaptability could be acquired, excepting the direct assimilation of the colouring-matter of food-plants in the case of caterpillars or other vegetable feeders. This, of course, carried with it the implication that natural selection could work on physiological processes if they were of use, just as well as upon any external morphological character. Stimulated by this hypothesis, other cases of variable colouring were sought for and found. The subject was later taken up by Prof. Poulton, who, for many years, conducted experiments and obtained results which are now familiar to all naturalists. The original speculation, that variable colouring was the result of an individual adaptability due to natural selection, implies that this faculty is of bionomic value. I am not now concerned with the validity or otherwise of this assumption; that is an issue on which opinion appears to be divided; although, I have no doubt in my own mind on the point, it is not necessary to state the case with any bias on the present occasion. Now the experiments of Poulton have shown that this colour variability is of very much more frequent occurrence than was ever dreamt of in 1873, and his facts have, in the main, been substantiated by the independent observations of many other experimenters. And it turns out also that the mechanism of the process is not even the simple assimilation of colouring-matter from the food-plant, excepting in the case of green caterpillars, in which it has been shown that chlorophyll in a modified form passes into the blood. The colour variability of caterpillars and pupæ in response to the external stimulus exerted by coloured surfaces, as established by these experiments, has brought us face to face with a fundamental

[1] I owe this statement to Mr. Bates himself, who has often made it to me.

problem in insect physiology, the solution of which we are anxiously awaiting. The mere possibility of being able to state the problem in its present form—apart from any question of the adaptive value of the colouration—is a step forwards; is an incentive to further experiment, and this is the legitimate end and aim of all scientific speculation.

Were I to attempt, however, to pass from what has already been accomplished to that which is yet awaiting investigation—to the questions which rise on all sides as pressing for solution, there would be no limit to this address. In view of the splendid opportunities afforded by insects for treatment as living organisms capable of revealing natural laws by skilled experimental research, is it not pardonable if we sometimes give way to the unphilosophic thought that the possession of chitinous exo-skeletons by these creatures, whereby they lend themselves so admirably for preservation as cabinet specimens, is an arrangement expressly designed for the retardation of entomological science? The scientific workers at living insects in this country are deplorably few as compared with those who devote themselves to cabinet entomology. The one great desideratum of modern biology is an experiment station where protracted observations can be carried on year after year on living animals, each set of experiments prompted by hypothesis and with the definite object of answering some particular question in relation to variability and inheritance, the nature of the action of the environment, the effect of selection, &c. This was a dream of the late Dr. Romanes; he has not lived to see it fulfilled, but if it should be realised in our time our entomologists will, I venture to hope, not be behind with suggested lines of work.

If by way of comparison we now turn to that branch of the subject in which the empirical method has hitherto almost exclusively been employed, viz. the taxonomy of this same order Lepidoptera, the results are most instructive. In view of the immense body of facts, the number of named species and the mass of published descriptive matter, I do not think I shall be wrong if I say that the best energies of the acutest workers have been concentrated on this subject from the middle of the last century down to the present time. A record of nearly a century and a half against the thirty odd years that have elapsed since the introduction of the theoretical method into the biological sciences. Is there any indication that all this work has brought us nearer the "definite end" to which it was and is directed—the natural classification of the Lepidoptera—to an extent commensurate with the number of workers and the time bestowed upon it? It is only quite recently that any decided advance has been made, and that through the work of Hampson, Comstock, Chapman, Meyrick, and others. It cannot be said that we have been waiting all these years for materials—for a few thousand new species is one of the best "collected" groups in the whole world of insects—in order that this sudden rush might be made. I take the view that we have been waiting rather for method than for additions to the lists of species; that we have hitherto too much disregarded the spirit of the speculative method in our taxonomic work, and that we have now happily found a band of workers who refuse to submit to the plea of inability because all the existing species of Lepidoptera have not been collected and named.

After advancing these arguments in favour of a more liberal use of the "scientific imagination" in connection with entomological subjects, I feel it incumbent upon me to define the position a little more fully in order to prevent misunderstanding. The conditions of speculation in the two great departments of natural science which have been under consideration are not exactly the same, and the differences in the method of treatment must not be lost sight of. If in the physical sciences there is, to use the expression of the late Prof. Stanley Jevons, "unbounded license of theorising," it is because we can appeal to nature so readily by the experimental method, and get our answer one way or the other, by imposing rigid conditions which are under our control. In the biological sciences this is not the case; all who are acquainted with experimental work in biology know how difficult it is, generally, to get definite answers to our questions—the conditions are vastly more complex when we come to deal with living organisms. I remember once remarking to the late Mr. Darwin how difficult it was to get nature to give a definite answer to a simple question, and he replied, with a flash of humour: "She will tell you a direct lie if she can." The practical result of this difference is that the speculation of an hour may take a lifetime for its verification. But I see no reason why, on these grounds, we should repress the spirit of speculation. If, as our former President says, it is given to few to be able to speculate with advantage—and in this I thoroughly agree with him—it is our paramount duty for the present and future welfare of our science, to give every man's honest thought our most serious attention, and to encourage the faculty whenever and wherever we find it, as the most precious means of advancing scientific knowledge. The "bugbear" is a very harmless animal if you look him boldly in the face, and if you treat him gently and put him into harness he will drive the chariot of science for you at a speed that will leave the empirical method far behind in the race for the knowledge of nature's ways.

The great service which the founders of the modern doctrine of evolution have rendered to science has, in my belief, been not only the particular theory of species transformation with which their names will ever be associated, but the importation into biology of the methods of the physical sciences. Writing to Wallace, in 1857, Darwin said: "I am a firm believer that without speculation there is no good and original observation" ("Life and Letters," vol. ii. p. 108). In the same letter he remarks: "You say that you have been somewhat surprised at no notice having been taken of your paper in the *Annals*. ["On the Law that has regulated the Introduction of New Species," *Ann. and Mag. Nat. Hist.*, 1855.] I cannot say that I am, for so very few naturalists care for anything beyond the mere descriptions of species." This statement of 1857 does not hold good in 1896; other methods of biological research have been introduced—the road to biological fame is no longer through the sole channel of technical systematic work, and we owe it to the writer of that letter more than to any other worker and thinker of our time, that the horizon has been extended on all sides.

The misapprehension to which my remarks may possibly give rise, and which I am most anxious to prevent, is that in urging the claim of the theoretical method I am introducing the danger of rash and promiscuous speculating by all kinds of dabblers in the subject. There is much justification for this attitude, but an analysis of the supposed danger will, I think, serve to show that it is not a very formidable one after all. It appears to me, moreover, that the advantage of giving an impetus to observation along preconceived lines far outweighs any passing danger arising from hasty speculation. It is notorious in the history of modern science that no single branch has escaped the efforts of well-intentioned, but quite irresponsible outsiders, to set our various houses in order for us. On critical examination it will be found, however, that none of these attempts, even when they have been lucky enough to forestall the conclusions arrived at by legitimate methods, have led to any practical issue in the way of observation or research. I am addressing my remarks on the present occasion to a Society composed more or less of experts; I am not inviting "the man in the street" to favour us with his views on this, that, or the other question, but I am asking the working entomologists among us to bear in mind that their studies may be directed so as to throw light on some of the broad biological problems of the day, if they will, as Faraday said, encourage themselves by a little more speculation. Judging from the part played by this method in the development of modern science, it is perhaps not going too far to say that it is better to have speculated erroneously than never to have speculated at all. Illustrations might be adduced showing that erroneous theories have often done good service to science, and that for this reason they have been temporarily retained, even when recognised as inadequate to meet the growing body of new evidence. This was the case, for example, with the old "fluid" theory of electricity. So also the "corpuscular" theory of light enabled Newton to develop optical science to a remarkable extent, although this theory is now among what Dr. Hicks calls the "wreckage."

Another source of danger in biological speculation to which I am also alive, is that we have the public eye upon us to an extent that is not experienced in other departments of science. I am bound to confess that I never could quite make out why this should be the case. It is possible to speculate about the constitution of matter, the degradation of energy, the age of the solar system, and other great problems of the universe, with any degree of dogmatism without exciting public discussion. But as soon as ever an effort is made to explain something in the living world, no matter how modestly, the speculator is forthwith treated as though he had thrown down a public challenge. Perhaps it is for this reason that biology is more subject to unauthorised and unscientific intrusion; because it gives opportunity for the pure *littérateur* to pose as a theorist. The speculations of the physicist or chemist are, moreover, generally expressed in a symbolical language which is not understood by the public at large, and their ideas, however revolutionary, thus escape newspaper and magazine notoriety. As far as my reading extends, I am inclined to believe that even in the case of the purely literary treatment of biological problems by writers who are not experts, the danger of overweighting the science with hypothesis is much exaggerated. Writers of this class are often capable of taking a wider and more philosophic grasp of a problem than a pure specialist, and ideas of lasting value have sometimes emanated from such sources. I imagine that nobody will dispute that Mr. Herbert Spencer's writings have largely influenced the public mind—whether we agree with the details of his doctrines or not—in accepting the broad principle of evolution, although this profound thinker lays no claim to an expert knowledge of any branch of natural history. But every working naturalist can ascertain for himself the credentials of any particular writer: my remarks are simply offered with the object of claiming more consideration for such writers, as a class, on the part of practical workers. The philosophic faculty is quite as powerful an agent in the advancement of science as the gift of acquiring new knowledge by observation and experiment. It is not often that the faculties are combined in one individual.

The general conclusion to which these considerations point is that the biological theorist, by virtue of the complexity of the factors, the difficulty of experimental verification, and the tendency on the part of the public to mistake tentative hypotheses for established theories, should put forward his views with more explicit caution than is necessary in the case of the physical sciences, where experimental evidence is more easily obtainable, and where the self-constituted philosopher but rarely gets a hearing. All this amounts, however, to nothing more than a plea for caution, and not for total abstinence. To disallow speculation because a complete theory cannot be formed out of the existing materials, is simply to put a check upon legitimate advancement. I freely admit that it is possible to carry speculation to an unscientific extreme—to fritter away a plausible hypothesis by mere metaphysical discussion, or to bury a real and important issue under an incubus of verbiage. But this is not the legitimate use of the speculative method; it is an accident, which the scientific worker will know how to avoid, and which is contingent upon the present condition of biological investigation. We cannot test our speculations off-hand by a few crucial experiments, as in physical science, and in the meantime the logic-chopper may get hold of our idea and whittle it away. On these grounds, however, I again fail to see any reason for repressing speculation. It might as well be argued that because the action of fire, carried to an extreme, carbonises organic matter, we should therefore eat our food raw. The irresponsible manipulation of biological hypotheses by pure speculators does no real or permanent injury to the cause of science, and may indirectly do good by directing public attention to the work which is being carried on. I rather think the absence of public sympathy, in connection with theoretical research in chemistry and physics, exerts a depressing influence; the inventor of a new hypothesis in these subjects moves entirely in an atmosphere of his own creation, which even his colleagues seldom venture to penetrate. That biological speculations are more prone to such unauthorised treatment is no more a reason for refusing to speculate than the circumstance that generations of fact-collectors have wasted their time in amassing large stores of disconnected observations, which for want of system are practically of no avail to the scientific worker, is an argument in favour of repressing observation. It is possible to be quite as unscientific in the accumulation of facts as it is to become metaphysical by over-speculation; there is as much danger in one direction as there is in the other. Yet the most ardent advocate of the theoretical method has not taken it upon himself to declare that observation must cease until he has explained all the facts at present available. This, however, is practically the position taken up by those who refuse to recognise that existing knowledge is sufficient to enable considerable advance to be made by the legitimate use of the theoretical method.

One other point demands consideration, in conclusion. If latitude for the exercise of speculation is to be allowed, where, it may be asked, is the line to be drawn? How are we to distinguish between the cautious theoriser and the writer who permits himself "unbounded license?" These are questions to answer which requires nothing but an exercise of individual judgment. A sound speculation may emanate from the happy possessor of a philosophic mind although he may never have done any technical biological work. But this kind of speculator naturally fails to secure that hearing to which the practical worker is entitled. Although valuable generalisations may occasionally be given out by great thinkers, the expert biologist shows wisdom in giving his most serious attention only to those who are familiar with their data at first hand—who have themselves gleaned their information directly from nature. By such workers only can the true value of the evidence be fairly weighed and estimated. I should be very sorry if the remarks which I have ventured to offer in the course of this address were to be interpreted into a general public invitation to speculate on biological problems. But I do raise the question here as to the kind of biological work which is to be recognised as a fitting preparation for the exercise of the speculative faculty. It used formerly to be asserted that he only is worthy of attention who has done systematic, *i.e.* taxonomic, work. I do not know whether this view is still entertained by entomologists; if so, I feel bound to express my dissent. It has been pointed out that the great theorisers have all done such work—that Darwin monographed the Cirripedia, and Huxley the oceanic Hydrozoa, and it has been said that Wallace's and Bates's contributions in this field have been their biological salvation. I yield to nobody in my recognition of the value and importance of taxonomic work, but the possibilities of biological investigation have developed to such an extent since Darwin's time that I do not think this position can any longer be seriously maintained. It must be borne in mind that the illustrious author of the "Origin of Species" had none of the opportunities for systematic training in biology which any student can now avail himself of. To him the monographing of the Cirripedia was, as Huxley states in a communication to Francis Darwin, "a piece of critical self-discipline,"[1] and there can be no reasonable doubt that this value of systematic work will be generally conceded. That this kind of work gives the sole right to speculate at the present time is, however, quite another point. It might be argued with some show of reason that exclusive devotion to systematic work cripples the imaginative faculty.[2] The methods of attacking the problems connected with living organisms have been increased and improved from every side, and the anatomist and physiologist, the morphologist, the embryologist, the student of bionomics, have all an equal claim to contribute to biological theory. The particular problems relating to the transformation of species are no doubt best dealt with by those who, by systematic work, have acquired a true notion of what is meant by the term "species." But so far as entomology is concerned, it must be confessed that the greater part of our systematic work has emanated from cabinet entomologists, who know nothing of the species they describe as living organisms by direct observation, and to me it appears doubtful whether this kind of work does confer any special faculty of speculating with advantage on the species question. It seems rather that the "field-naturalist" in the old sense of the term has the advantage, and I may remind you in this connection that during the voyage of the *Beagle*, when Darwin began to make those observations on island life which afterwards led him to take up the question of species transformation, he was essentially a "field-naturalist," his systematic work on the Cirripedia not having been commenced till after his return. So also Wallace, at the time when he independently elaborated the theory of natural selection, was certainly not a systematist in this narrow sense. He has been good enough to favour me with his views on this point, in a letter dated December 31, 1895, in which he says: "I do not think species-describing is of any special use to the philosophical generaliser, but I do think the collecting, naming, and classifying some extensive group of organisms is of great use, is, in fact, almost essential to any thorough grasp of the whole subject of the evolution of species through variation and natural selection. I had described nothing when I wrote my papers on variation, &c. (except a few fishes and palms from the Amazon), but I had collected and made out species very largely, and had seen to

[1] "Life and Letters," vol. i. p. 348. Even in the days of my studentship, Huxley lectured on Natural History at the Royal School of Mines with the aid of diagrams and specimens only: practical work in the laboratory was unknown.

[2] See a letter from Darwin to Bates in 1861, "Life and Letters," vol. ii. p. 379.

some extent how curiously useful and protective their forms and colours often were, and all this was of great use to me."

I had hoped to be able to discuss some of the current problems which are before biologists, and towards the solution of which entomology might contribute largely. Such, for example, are Galton's and Weismann's views on the non-transmissibility of acquired characters, the *rôle* of what Mr. Bateson calls "discontinuous variation" in the origin of species, the recent efforts to throw light on the all-important subject of variability by the statistical methods introduced by Galton and now being worked at from the experimental side by Weldon, and from the mathematical side by Karl Pearson. I feel, however, that I have trespassed already too long upon your forbearance, and while again thanking you for the honourable position in which you have placed me, I can only express the hope that my special plea for a more liberal use of the speculative method among our working entomologists will not be regarded by those who hold different views as a breach of the privilege of that office to which by your courtesy I have been elected. Should there be any who entertain this opinion, I beg them to make a liberal discount for personality, and they will find that the ultimate motive has been to promote the best interests of our science.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The Waynflete Professor of Mineralogy (Mr. H. A. Miers), who has been absent during the early part of the term through illness, announces a course of lectures on Elementary Crystallography.

In a Congregation held last week, the proposal that a sum not exceeding £150 per annum for three years from October 1, 1894, should be applied out of the Common University Fund in maintaining a scholarship to be held by a student at the Marine Biological Station at Naples, was agreed to, *nemine contradicente.*

In a Congregation to be held on February 18, a form of statute amending the provisions of a statute made for the administration of the Lichfield Trust for Clinical instruction, will be proposed. The object of the statute is to provide for the conduct of the Pathological Department at the Radcliffe Infirmary by the Regius Professor of Medicine, or a person appointed by him, and for giving instruction in Pathology in accordance with the Regulations of the Board of the Faculty of Medicine.

The interest of the University is at present absorbed in the resolutions respecting the admission of women to the degree of B.A., which are to be submitted to Congregation on March 3. The first resolution, which proposes that women shall, under certain conditions, be admitted to the degree of B.A., will if carried tend to promote the study of every subject by women in Oxford, and therefore has an ultimate bearing on scientific studies. At the same time it will compel women to go through Responsions and the other examinations from which they are now exempt. There are some who think that this will be injurious to their interests. The most that can be said in the case of those who wish to read Natural Science is, that it will compel them to learn Latin and Greek either before they come to Oxford, or after they have come up. If the latter, they will find themselves obliged to keep four years residence, which most do not as things now are. If they know enough Latin and Greek to pass Responsions before coming into residence, their case will not be altered, for a woman competing for honours in one of the final subjects in the Honour School of Natural Science always passes the preliminary examinations required by the statutes in the case of men. It is not proposed that the strict B.A. course should be obligatory on all women students. Those who do not wish to take up Latin and Greek, but wish to read Natural Science or another subject such as History, will be allowed to do so under existing regulations, and so may escape Responsions; but they will also have to forego the distinction of the degree.

CAMBRIDGE.—St. John's College has made arrangements for the admission of post-graduate students desiring to pursue a course of advanced study or research under the new regulations of the University. Until the statutes now before the Privy Council are approved, candidates for admission are required to present a letter of recommendation from the Professor or other teacher under whom they propose to work in Cambridge. Further particulars may be learned on application to one of the tutors of the College.

Mr. J. N. Langley, F.R.S., Lecturer in Physiology, has been approved for the degree of Doctor of Science.

T.R.H. the Grand Duke of Hesse and Prince Henry of Prussia have presented to the Museum of Zoology the skeleton of a wild boar.

The following have been appointed Electors to the undermentioned professorships: Chemistry, Prof. Thomson; Plumian (Astronomy), Sir G. G. Stokes; Anatomy, Downing (Medicine), Surgery, and Pathology, Prof. Foster; Botany and Physiology, Prof. Allbutt; Geology, Dr. Phear; Mineralogy, Prof. Liveing; Zoology, Mr. J. W. Clark; Experimental Physics, Prof. Clifton; Mechanism, Mr. Horace Darwin.

A MEETING was held at Cardiff last week to start a public subscription in aid of the erection of new buildings for the University College of South Wales. A sum of £20,000 is required to meet the conditional grants made by the Treasury and the Drapers' Company. Contributions amounting to £13,000 were promised at the meeting. Lord Windsor, who presided, will contribute £2500, and a substantial sum has also been promised by Lord Tredegar. It is expected that £30,000 will be raised. Mr. Alfred Thomas, M.P., contributes £1000, and Mr. John Cory a like amount.

Science announces the following gifts to education in America: The University of Pennsylvania has received a gift of 5000 dols. from Mr. Charles M. Swain, and of 5000 dols. anonymously, the money to be used without restrictions. The will of the late Martin Brimmer, of Boston, to take effect on the death of his wife, bequeaths 50,000 dols. to Harvard University. Ground has been broken for the first of the four buildings of the new biological school of the University of Chicago, which is to be erected with part of the 1,000,000 dols. recently given by Miss Culver. It is proposed to erect special buildings for zoology, botany, anatomy, and physiology, instead of one biological building, as planned before the receipt of Miss Culver's gift.

PRINCIPALS of Technical Schools and others who assist in deciding the character of instruction in chemistry, would do well to take to mind the lesson contained in the following extract, referring to the work of the Chemical Department, from the programme just received from the Central Technical College: "The object aimed at in this part [first year] of the course will be to encourage habits of accuracy and thoughtfulness, and to teach the art of experimenting with a logical purpose rather than to impress mere facts. . . . As soon as students have acquired the necessary proficiency as analysts and sufficient skill in preparing pure substances, they will be encouraged to undertake an original investigation, in order that they may learn to apply their knowledge, as well as develop their powers of observation and reasoning: and thus become fitted to solve problems which are continually presenting themselves in practice, and to improve and advance the industry with which they may be connected. The importance to students of thus devoting themselves, sooner or later, to the higher branches of chemistry cannot be too strongly insisted on; in no other way is it possible for them to acquire the breadth of view and the power of grappling with new problems, as they arise in practice, which are required of the technical chemist."

DR. H. E. ARMSTRONG has been for some time trying to instil a little scientific spirit into the School Board for London. In an address recently delivered at the Borough Polytechnic Institute, and printed in full in the *Technical World*, he described the excellent results attained by the introduction of the scheme of instruction in scientific method, drawn up by a Committee of the British Association. The Board has every reason to be proud of what its science demonstrators have done to promote the reformed methods of science instruction, of which Dr. Armstrong is the most active exponent. The methods have been proved to be practicable, and the results obtained by following them are most satisfactory. It remains for the School Board to recognise this by extending to all its schools in the metropolis (girls' as well as boys' schools) the teaching which has been so successfully carried on in one of its districts. If that were done, a great advance in education would be assured. Those who are engaged in the work of technical education are agreed, as Dr. Armstrong pointed out, that it is all but impossible at the present time to give true technical education in this country, owing to the extraordinary defective condition of our preliminary school training. But if children in elementary schools were taught to appreciate the main principles of scientific method, it would be possible for them afterwards to properly avail themselves of the higher training which is offered to them, and which alone can render them competent as industrial and domestic workers. It is to be hoped, therefore, that the School Board will see its way to extending the work of scientific education begun under its auspices six years ago.

In a preliminary report recently prepared for the Technical Education Board of the London County Council, Dr. C. W. Kimmins gives the following statistics to show the progress that has been made, especially in the teaching of physics and chemistry, in the secondary schools assisted by the Board.

	1893-4.	1894-5.	1895-6.
Number of pupils receiving theoretical instruction in physics	1867	1899	2266
Number of pupils doing practical work in physics	215	433	1576
Number of pupils receiving theoretical instruction in chemistry	2091	2287	2647
Number of pupils doing practical work in chemistry	630	1101	1814
Percentage of those receiving theoretical instruction in physics, taking practical work in this subject	11·5	22·9	69·5
Percentage of those receiving theoretical instruction in chemistry, taking practical work in this subject	30·1	48·1	68·5

Dr. Kimmins points out that the statistics show that there has been a general advance in the number receiving instruction in experimental science at these schools, and that the proportion doing individual practical work has increased to a far greater extent. He reports that the general introduction of practical teaching in elementary physics is producing excellent results. A marked improvement is also to be noticed in the teaching of chemistry; the practical work is of a much more rational kind, and bears a closer relation to the class teaching. Qualitative analysis is rapidly ceasing to occupy the important position it has held in the laboratory in former years.

SCIENTIFIC SERIALS.

THE *Journal of Botany* commenced its enlarged issue with the present year, and the two numbers already published indicate that its editor will have no difficulty in filling its pages with matter of value to the English botanist. An interesting paper, by Mr. E. A. L. Batters, describes several new British seaweeds, including two new genera, *Colaconema* and *Trailliella*, both belonging to the *Florideæ*. Mr. J. H. Burkill contributes a paper on the variation in the number of parts of the flower of *Parnassia palustris*. Mr. A. H. Praeger proposes a division of Ireland into botanical districts, accompanying his paper by a map. There are a number of other papers on various departments of descriptive botany. The plates illustrate two new forms of British pond-weed described by Mr. A. Fryer, and new African plants described by Mr. A. B. Rendle and Mr. E. G. Baker.

Bulletin of the American Mathematical Society, vol. ii. No. 3, December 1895.—Prof. F. Morley, in a notice of Gundelfinger's Vorlesungen aus der Analytischen Geometrie des Kegelschnitte, classes it with two other recent analytic works on conic sections, for which one is very thankful; the other two are the works by the late Prof. Casey and Miss Scott. He states the plan of Gundelfinger's treatise to be to systematically develop the theory by means of homogeneous coordinates, while bringing out the fact that the elementary (x, y) system is merely a case to which we can descend when so minded. This latter may seem a minor point; pedagogically it is not so, and it is certainly not well explained in many books. The development of the theory is really analytic, though one feels that the analysis is under the control of a masterly geometric insight. Prof. Morley's review is a long one, and enters into many details of the work which has been edited by Dr. Dingeldey. Short notes follow, viz. on divergent series, by Prof. A. Chessin, and a simple proof of a fundamental theorem of substitution groups, and several applications of the theorem, by Dr. G. A. Miller.—Dr. James Pierpont contributes an interesting note on an undemonstrated theorem of the *Disquisitiones Arithmeticæ*. This ends with two theorems relating to the construction of a polygon of n sides by a series of *rational* conics, *i.e.* conics whose coefficients are rational in the current domain of rationality, and gives in three rows the polygons, constructible by rule and compass, known to the Greeks (twenty cases); then the polygons of this class discovered by Gauss (five cases); and, in the last row, the *additional* polygons which can be constructed when *rational* conics can be employed (thirty-five cases). The table is limited to constructible regular polygons of sides $\gtreqless$ 100.—Notes and new publications close the Number.

Bollettino della Società Sismologica Italiana, vol. i. 1895, No. 7.—Ernesto von Rebeur-Paschwitz, by A. C.—The first instant of the great earthquake-shock of May 18, 1895, noted in Arcetri (Florence), by A. Abetti.—On the Florentine seismic centre, by M. Baratta. A topographical discussion of the three principal earthquakes felt in the neighbourhood of Florence in the present century, those of 1812, 1887, and 1895. The centres of the meizoseismal zones, though very near one another, are not quite coincident; but this, it is suggested, may be due to a variation in the depth of focus, or in the intensity of the original disturbance.—Notices of earthquakes felt in Italy (May-June 1895), by M. Baratta. The most important are the Florentine earthquake of May 18, the Spoleto earthquake of May 20, and the Rovigo earthquake of May 25.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, January 16.—Mr. A. G. Vernon Harcourt, President, in the chair.—The following papers were read:—The acetylene theory of luminosity, by V. B. Lewes. The adverse criticism of the acetylene theory of luminosity by Smithells does not affect the considerations upon which the theory is based; these are (1) that the unsaturated hydrocarbons in the inner region of the flame are largely converted into acetylene before luminosity commences; (2) that pure acetylene develops luminosity when flowing through a heated tube; (3) that the temperature necessary to decompose acetylene with evolution of light does not raise to incandescence the liberated carbon; and (4) that in luminous hydrocarbon flames of sufficiently high temperature, the luminosity varies directly with the amount of acetylene present at the point where luminosity commences.—The action of sodium alcoholate on certain aromatic amides, by J. B. Cohen and W. H. Archdeacon. Many of the aromatic amides form addition compounds with sodium methor eth-oxide; thus, acetanilide yields a substance of the composition PhNHAc, MeONa.—Note on the electrical conductivity of formanilide and thioformanilide, by T. Ewan.—The action of sugar on ammoniacal silver nitrate, by J. Henderson. A definite factor can be assigned expressing the action of glucose, lævulose, and galactose on ammoniacal silver nitrate under standard conditions, but no such factor can be obtained in the case of lactose or maltose, owing to secondary reactions. Cane-sugar, starch, and dextrin do not act on the ammoniacal solution under the standard conditions.—Solution and diffusion of certain metals in mercury, by W. J. Humphreys.—On some of the ethereal salts of active and inactive monobenzoyl, dibenzoyl, diphenylacetyl, and dipropionyl glyceric acids, by P. Frankland and J. MacGregor. The physical properties of these salts have been determined. and the relation between the rotatory power and the constitution of glyceric acid derivatives is discussed.—On the rotation of optically active compounds in organic solvents, by P. Frankland and R. H. Pickard. As a result of cryoscopic and rotatory power determinations of methyl dibenzoylglycerate and ethyl diacetylglycerate in various solvents, the authors find that when the substance has a low molecular weight, the specific rotation is high, and *vice versâ*; the bearings of these results are discussed.—Note on the action of hydrogen chloride on ethyl alcohol, by J. C. Cain.—Transformation of the alkylammonium cyanates into the corresponding ureas, by J. Walker and J. R. Appleyard. Measurements of the rates of transformation of the alkylammonium cyanates into ureas, and *vice versâ*, indicate that the cyanates are dissociated into two ions in aqueous solution. On certain phenylthiocarbamates, by H. L. Snape.—The available potash in soils, by T. B. Wood.

Linnean Society, January 16.—Mr. C. B. Clarke, F.R.S., President, in the chair.—Messrs. O. V. Aplin and William Cole were elected Fellows of the Society.—On behalf of Mr. G. H. Adcock, of Geelong, Victoria, Mr. A. B. Rendle exhibited and made remarks upon some photographs of *Hakea grammatophylla*, F. Muell, a little-known species of the Proteaceæ, of local distribution in South Australia.—Mr. G. F. Scott Elliot exhibited specimens of bark cloth from Uganda and the shores of Lake Tanganika, and gave an account of the mode of its preparation from the bark cloth fig, and of the fleshy Euphorbias and Acacias of British East Africa, illustrating his remarks with lantern slides from photographs taken by himself. Mr. Elliot remarked that the native cloth manufactured on the shores of the Tanganika was made on the same sort of rough loom which he had seen employed near Sierra Leone, and that as the Tanganika is ethnologically and botanically part of the west coast, it was interesting to find that the methods employed in countries so far apart were so similar in detail. A discussion followed, in which Messrs. Rendle, Holmes, T. Christy, and W. Carruthers took part.—On behalf of Mr. W. R. Ogilvie Grant, Mr. Harting exhibited some land shells and eggs and skins of two rare Petrels from the Salvage Islands, lying between the Canaries and Madeira. These islands were stated to be of volcanic origin faced with steep rocks from 100 feet to 300 feet in height, and covered with loose sandy soil, the vegetation consisting chiefly of the wild tomato *Lycopersicum esculentum*, the ice-plant *Mesembryanthemum crystallinum*, *Asparagus scoparius*, and *Cistanche lutea*. Amongst the shells collected were *Helix ustulata*, peculiar to the Salvage Islands, *H. pisana*, *H. Macandrewi*, *H. polymorpha*, *Rumina decollata*, *Littorina striata*, *Cerithium rupestre*, and *Nassa conspersa*. *Helix paupercula* was said to furnish the chief food of the Tarantula spider (*Lycosa maderiana*), and entire shells of *Helix pisana* had been found in the stomach of a Kestrel hawk shot on one of the islands. The Petrels exhibited with their eggs were *Pelagodroma marina*, and *Oceanodroma cryptoleucura*, which were found nesting in burrows after the manner of the Shearwater (*Puffinus kuhli*), of which great numbers were also breeding there. Mr. Howard Saunders offered some critical remarks on these birds, referring chiefly to what was known of their geographical distribution.—Mr. George Murray exhibited full-grown complete specimens of some giant Laminarians from the Pacific, *Nereocystis*, *Egregia*, and *Macrocystis*, and some very fine specimens of *Postelsia*, collected by Mr. W. E. Shaw on the coast of California. He made some remarks on the distribution of Californian *Laminarieæ*, and illustrated some points in the structure of their reproductive organs.—A paper was then read, by Prof. T. Rupert Jones, F.R.S., and Mr. Frederick Chapman, on the relations of the fistulose *Polymorphianæ* and the *Ramulinæ*, with the view of showing the existing evidence for or against the suggestion that several specimens referred to the latter of these two sub-families may really belong to the former.

Geological Society, January 22.—Dr. Henry Woodward, F.R.S., President, in the chair.—Mr. W. W. Watts, in the absence of Prof. Lapworth, called attention to three specimens of sandstone and limestone containing specimens of some species of *Hyolithes*, which Prof. Lapworth had found in the higher part of the Cambrian quartzite at Nuneaton in Warwickshire.—The following communications were read:—On the Speeton series in Yorkshire and Lincolnshire, by G. W. Lamplugh. Further work on the Speeton section, while extending the knowledge of the palæontological details, had fully sustained the results of the author's previous investigations. The rapid attenuation and final disappearance of the Speeton series in a westerly direction in Yorkshire was discussed, and though the available evidence was held to be insufficient to demonstrate the exact conditions, it was stated that, contrary to the accepted view, the lower zones were probably the first to die out and were overstepped or overlapped by the higher divisions, since at Knapton, fourteen miles inland, only the upper zones of the coast-section can be proved to occur. In Mid-Lincolnshire all the palæontological zones of Speeton were identified and traced, the presence of the leading zonal types of the cephalopoda readily establishing the general correlation proposed by Prof. A. Pavlow and the author. The President said that it was hardly possible when mapping in the field to do more than follow those petrological changes in the character of beds over any given area which are patent to the observer. The point discussed by the author was that the life-line did not follow the line of the same sedimentation, but life-forms may transgress, and did transgress, over sediments of different character when they happened to be accumulated at the same time. It was hoped, however, that the case propounded by the author was exceptional, and that, as a rule, the sediments and the fossils followed one another on the same lines.—On some Podophthalmous Crustaceans from the Cretaceous formation of Vancouver and Queen Charlotte Islands, by Dr. Henry Woodward, F.R.S.—On a fossil octopus, *Calais Newboldi* (J. de C. Sby., MS.), from the Cretaceous of the Lebanon, by Dr. Henry Woodward, F.R.S.—On transported boulder clay, by the Rev. Edwin Hill. The "mid-Glacial" sands of the cliffs between Yarmouth and Lowestoft are overlain at Corton by chalky boulder clay. But farther north than Corton some masses of the same clay occur in the interior of the cliffs, surrounded by the sands in undisturbed stratification, but passing into them by strings and patches such as suggest the melting off of enveloping ice. They had probably been floated and dropped there. The observations suggest that chalky boulder clay was being manufactured in one locality simultaneously with "mid-Glacial" sands in another.

Mineralogical Society, February 4.—W. W. Watts in the chair.—Mr. L. J. Spencer gave an account of some of the results he had obtained in the course of an examination of various massive and fibrous forms of calcite and aragonite.—Mr. F. Rutley read a paper relative to associated globular and rhombohedral forms of rhodochrosite and chalybite from Cornwall.—Mr. G. T. Prior described the microscopic characters of certain rocks, allied to Monchiquite, collected by Mr. Ridley in Fernando Noronha, Brazil.—Mr. W. J. Pope explained a method of determining the optic axial angle for the case where the faces of the investigated plate are oblique to a bisectrix, and demonstrated the phosphorescence of saccharin crystals on fracture.

Zoological Society, February 4.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—Mr. G. A. Boulenger, F.R.S., read a report on the second portion of the reptiles and batrachians collected by Dr. A. Donaldson Smith during his recent expedition to Lake Rudolph, the first portion having been already described. In the present report forty-two species of reptiles and five of batrachians were catalogued—of which two lizards were described as new, under the names *Agama smithi* and *A. lionotus*.—Dr. A. Günther read a report on the collection of fishes made by Dr. Donaldson Smith during his expedition to Lake Rudolph. From Lakes Rudolph and Stephanie examples of eight species of fishes had been obtained. Of these, five were species also found in the Nile-basin, and mostly of wide distribution in Africa; while one (*Distichodus rudolphi*) was new to science. Two other species were also described as new, and named *Clarias smithi* and *Synodontis smithi*, after their discover.—Mr. Martin Jacoby offered some remarks on the system of coloration and punctuation in the beetles of the genus *Calligrapha* of the family Chrysomelidæ.—Mr. F. E. Beddard, F.R.S., read a paper on the oblique septa in Passerine and other birds, in which he pointed out a new character of Passerine birds.—A second paper, by Mr. Beddard, contained a note upon the syrinx and the ambiens muscle of an African stork (*Dissura episcopus*), and comprised some remarks upon the classification of the Herodiones.—A communication from Mr. R. Lydekker, F.R.S., contained a note on the mode of progression of the sea-otter.—A communication from Dr. St. George Mivart, F.R.S., contained a description of the hyoid bones of *Nestor meridionalis* and *Nanodes discolor*.

PARIS.

Academy of Sciences, February 3.—M. A. Cornu in the chair.—Notice was received from the Minister of Public Instruction of the approval, by the President of the Republic, of the election of M. Rouché.—On the equilibrium of an ellipsoidal envelope, by M. L. Lecornu. The problem of a flexible inextensible surface submitted to a given system of forces gives rise to a system of partial linear equations, the integration of which, in general, is not possible. The particular case, however, of an ellipsoidal membrane, which is of considerable practical value on account of its application to the theory of aerostats, can be dealt with by the use of elliptical coordinates, and the results of the integration are given.—The measurement of a section of the Paris base line, with the apparatus of Jäderin, by M. d'Abbadie. By the use of wires of steel and of bronze, of known coefficients of expansion, a base line can be measured at the rate of 2500 metres *per diem*, as against 400 metres when bars are used as the standards of length.—Solar observations made at the observatory of the Roman College during the second half of 1895, by M. P. Tacchini.—On the complete solutions of the equation

$$x_1 \tan^{-1}\frac{1}{\kappa_1} + x_2 \tan^{-1}\frac{1}{\kappa_2} + \ldots + x_n \tan^{-1}\frac{1}{\kappa_n} = k \cdot \frac{\pi}{4},$$

by M. C. Störmer. A continuation of a note presented to the preceding meeting of the Academy.—On the energy dissipated in magnetisation, by M. Maurain. An attempt to measure the energy dissipated by iron and steel wires in a closed magnetic cycle when the variations in the strength of the field are very rapid (see p. 350).—Resistance of thin metallic sheets, by M. Ed. Branly.—Observations on a recent note, by M. G. Le Bon, on the "dark light," by M. G. H. Niewenglowski. A repetition of M. Le Bon's experiment, carried out in complete darkness, still gave a similar result, showing that the image is due to stored-up luminous energy.—Photography with dark light, by M. Gustave Le Bon. In further experiments made on this subject especial care has been taken to eliminate the possible influence of heat, and of light stored up in the plates.—New properties of the X-rays, by MM. L. Benoist and D. Hurmuzescu. The X-rays discharge a gold-leaf electroscope, and this offers a ready method for examining the permeability of various substances to these rays.—Experiments on the Röntgen rays, by M. A. Nodon. These rays are clearly distinguishable from the ultra-violet rays by the fect that the latter, obtained from a powerful arc lamp, fail entirely to affect a sensitive plate protected with several thicknesses of blackened paper. The Röntgen rays readily affect the plate under these conditions.—Transparency of metals for the X-rays, note by M. V. Chabaud. In sheets of 0·2 mm. thickness, platinum and mercury alone are perfectly opaque, while lead, zinc, copper, tin, steel, gold, silver, and aluminium are more or less transparent. In sheets of ·01 mm. thickness, platinum also ceases to be perfectly opaque.—The photography of metallic objects through opaque bodies, by means of the brush of an induction coil, without a Crookes' tube, by M. G. Moreau.—On the acid fluorides, by MM. Meslans and F. Girardet. The method employed is to act on the corresponding chloride with the fluoride of either arsenic, antimony, silver, or zinc. The fluorides of propionyl and of benzoyl were prepared and their properties examined.—Method of preparation of acid fluorides, by M. A. Colson. The acid anhydrides, treated with hydrofluoric acid, give the corresponding acid fluoride and acid. The chlorides of acetyl and propionyl are very easily obtained in this way.—On a hydride of lithium, by M. Guntz. Lithium, at a low red heat, absorbs about seventeen times its volume of hydrogen without any change of appearance; at about a red heat further absorption commences, and on cooling the lithium is seen to be covered with a layer of hydride. This was prepared in a pure state, and proved to be LiH.—The negative reaction and the centre of the retina, by M. Aug. Charpentier.—Researches on the embryonic nervous system of the Nauplius and of some larvæ of marine animals, by M. N. de Zograf.—On an Ophidian of the cretaceous earths of Portugal, by M. H. E. Sauvage.—Physiological researches on the respiration of fishes (*Ammodytes tobianus*), by M. J. B. Pieri. This fish was able to completely extract the oxygen from a solution of air in water, although it could not take out all the oxygen from a solution rich in the gas. Asphyxia is never instantaneous, even when the *Ammodytes* is introduced into water completely freed from dissolved oxygen. This fish can exist without inconvenience in water containing a considerable quantity of dissolved carbon dioxide.—Observations on the cephalic vesicle of insects of the family Muscides, by M. A. Laboulbène.—*Mucor* and *Trichoderma*, by M. Paul Vuillemin. Some remarks on a paper of M. J. Ray on the parasitism of a *Trichoderma* on a supposed new species of *Mucor*.—On the geological characters of the auriferous conglomerates of the Transvaal, by M. L. de Launay.—On the bed of eruptive and metamorphic rocks of the basin of Lavol, by M. D. P. Œhlert.—Petrographic study of the Albitophyres of the Laval basin, by M. Michel Lévy.—The effects of the solar displacements, considered by themselves, on the barometric pressures of the zone 10° to 30° N., by M. A. Poincaré.—On a meteor seen at Baleine on January 6, 1896, by M. Doumet-Adanson. This meteor, the appearance of which was noted to the Academy on January 13, was seen at Baleine at 5.7 p.m. (Paris time), passing horizontally, about 25° above the horizon.

BERLIN.

Meteorological Society, January 7.—Prof. Börnstein, President, in the chair.—Prof. Kremser spoke on the duration of sunshine over Europe, basing his remarks on the data available from the various stations. The mean duration increases from the north towards the south, being least in Scotland and greatest in Spain. There is also a distinct increase from the west eastwards. It is less on mountains than over open plains, except at very high stations which are frequently above the level of the clouds and mist. All stations show a yearly minimum in the winter solstice, and a maximum in the summer; the latter occurs as early as May in Scotland, in June over Germany, and in July over Spain. There is no such annual variation observable at the highest stations. The amplitude of the annual curve is less when based on the percentage of observed to possible duration of sunshine. The curve of daily variation rises sharply in the morning, is then steady for some time, and falls again sharply towards the evening. At high stations the daily maximum occurs in the afternoon.

AMSTERDAM.

Royal Academy of Sciences, November 30, 1895.—Prof. Van de Sande Bakhuyzen in the chair.—Prof. Engelmann treated the following subjects. (1) The influence of the pulse frequency upon the physiological conductive power of the ventricular muscle. (2) A means of rendering extra-polar electric impressions upon muscles and nerves impossible.—Prof. Lorentz read a paper on Poynting's theorem concerning the transfer of energy in the electromagnetic field, and on two general propositions in the theory of light. After showing how Poynting's theorem may serve to calculate the energy of a magnetised body and the development of heat due to magnetic hysteresis, the author discussed a more general formula, already used by Volterra. The application of this equation to the propagation light (homogeneous and of constant intensity) leads in the first place to a well-known law of reciprocity (viz. a relation between the vibrations at a point A, caused by a source of light at B, and the vibrations at B, produced by a source at A) and in the second place to a generalisation of "Huygens' principle." If, in a system of conducting or dielectric, isotropic, or anisotropic bodies, surrounded on all sides by the ether, a closed surface be arbitrarily chosen, so, however, that all sources of light are external to it, then a definitive distribution of sources of light over this surface may be indicated, which would give rise at all internal points to the same vibrations as are produced by the external sources.—Prof. Kamerlingh Onnes communicated Dr. Zeeman's further measurements on the absorption of electrical waves in different electrolytes. The results are: (1) the intensity of electrical vibrations (wave-length 6·5 m.) decreases to one-third of its original value when the vibrations pass a layer of a solution of sulphate of copper, 5 per cent. c.m. in thickness, the resistance being $3340 \cdot 10^{-10}$ that of mercury. (2) Different aqueous solutions of the same conductivity absorb vibrations of the same frequency in the same degree.—Dr. W. van Bemmelen has drawn the lines of equal secular variation of the magnetic declination for the period 1540–1880. The values of the yearly variations have been determined by measuring the inclination of the curves on his map, which shows the curves of the secular variation for 8 × 18 intersections of meridians and parallels (meeting of September 28).—The maps for 1780 and 1880 show that Bauer's isoclinal poles lie in the immediate vicinity of the lines of maximum variation; whilst the whole system of maps points out the fact that in the tropical zone these lines and the agonic lines accompany each other. The mean yearly rates of shifting of the poles, the agonic lines and the maximum lines, viz. 0°·194, 0°·184 and 0°·21, agree very closely.

December 28, 1895.—Prof. van de Sande Bakhuyzen in the chair.—Prof. W. Kapteyn gave a new treatment of a problem on *Analysis situs*.

GÖTTINGEN.

Royal Society of Sciences.—The third part of the *Nachrichten* (physico-mathematical series) for 1895 contains the following papers communicated to the Society:—

May 25.—On the development of *Dadocrinus gracilis* (von Buch) and *Holocrinus Wagneri* (Ben.), and their relation to other crinoids, by A. von Koenen.

June 15.—On the integration of the partial differential equation $\Delta u + k^2 u = 0$ on Riemann's surfaces, by A. Sommerfeld.—The hypsographic curve of the earth's crust and the relations of Romieux ,by Hermann Wagner.

July 6.—On certain regularities in the spectra of solid bodies, and on a new determination of the sun's temperature, by F. Paschen.

July 20.—Researches (ii.) from the Göttingen University Laboratory: (1) On new instances of isomerism and abnormal molecular refraction in certain cyclic ketones; (2) on pulegon, by O. Wallach.—Report on the scientific memoirs issued from the Göttingen University Pathological Institute in the session 1894-95, by J. Orth.

October 19.—(1) Contributions to the theory of algebraic numbers; (2) the unimodular substitutions in an algebraic *Zahlenkörper*, by A. Hurwitz.—On a geometrical representation of the ordinary development of a continued fraction, by F. Klein.—On the regions of discontinuity of the groups of real linear substitutions of a complex variable, by Robert Fricke.—On the foundations of the theory of "ideals," by Ph. Furtwängler.

November 2 (Commemoration-day).—On the "arithmetisation" of mathematics, by F. Klein.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, February 13.

Royal Society, at 4.30.—On the Behaviour of Argon and Helium when submitted to the Electric Discharge: Dr. J. N. Collie and Prof. Ramsay, F.R.S.—On the Generation of Longitudinal Waves in Ether: Lord Kelvin, F.R.S.—On the Discharge of Electricity produced by the Röntgen Rays, and the Effects produced by these Rays on Dielectrics through which they pass: Prof. J. J. Thomson, F.R.S.

Society of Arts, at 4.30.—Punjab Irrigation—Ancient and Modern: Sir James Broadwood Lyall, G.C.I.E., K.C.S.I.

Mathematical Society, at 8.—Geodesics on Quadrics, not of Revolution: Prof. Forsyth, F.R.S.—Solid Ellipsoidal Vortex: R. Hargreaves.—Potential of a Cyclide: A. L. Dixon.

Royal Institution, at 3.—Some Aspects of Modern Botany: Prof. H. Marshall Ward, F.R.S.

Institution of Electrical Engineers, at 8.—The Electric Wiring Question: F. Bathurst.—Concentric Wiring: Sam. Mavor.

Society of Antiquaries, at 8.30.

Gresham College (Basinghall Street, E.C.), at 6.—The Planet Jupiter: Rev. E. Ledger.

FRIDAY, February 14.

Royal Institution, at 9.—Fish Culture: J. J. Armistead.

Physical Society, at 5.—Annual General Meeting.—On the Determination of High Temperatures with the Meldometer: W. Ramsay and N. Eumorfopnilos.

Royal Astronomical Society, at 3.—Annual Meeting.

Institution of Civil Engineers, at 8.—The Construction of the Molong to Forbes Railway, New South Wales: Sydney Thow.

Malacological Society, at 8.—Annual Meeting.

Gresham College, at 6.—The Planet Jupiter: Rev. E. Ledger.

SUNDAY, February 16.

Sunday Lecture Society, at 4.—Water Skin: Douglas Carnegie.

MONDAY, February 17.

Society of Arts, at 8.—The Chemistry of certain Metals and their Compounds used in Buildings, and the Changes produced in them by Air, Moisture, and Noxious Gases, &c.: Prof. J. M. Thompson.

Victoria Institute, at 4.30.—China: Dr. Gordon.

TUESDAY, February 18.

Royal Institution, at 3.—External Covering of Plants and Animals: Prof. C. Stewart.

Society of Arts, at 8.—The Development of Electrical Traction Apparatus: H. F. Parshall.

Zoological Society, at 8.—On the Butterflies obtained in Arabia and Somaliland by Captain Chas. G. Nurse and Colonel J. W. Yerbury in 1894-95: Dr. Arthur G. Butler.—On Moths collected at Aden and in Somaliland: Lord Walsingham, F.R.S., and G. F. Hampson.—Observations on the Metallic Colours of the Trochilidæ and the Nectariniidæ (communicated by F. E. Beddard, F.R.S.): Miss Marion Newbigin.

Institution of Civil Engineers, at 8.—The Manufacture of Aluminium by Electrolysis; and the Plant at Niagara for its Extraction: Alfred Ephraim Hunt.

Royal Statistical Society, at 5.—Mental and Physical Conditions among 50,000 Children seen 1892-94, and the Methods of Studying Recorded Observations, with special reference to the Determination of the Causes of Mental Dulness and other Defects: Dr. Francis Warner.

Royal Photographic Society, at 8.

Pathological Society, at 8.30.

Royal Victoria Hall, at 8.30.—Flowers and their Insect Visitors: Dr. Kimmins.

WEDNESDAY, February 19.

Society of Arts, at 8.—Report of the Royal Commission on Secondary Education: H. Macan.

Royal Meteorological Society, at 7.30.—Report on the Phenological Observations for 1895: Edward Mawley, President.—Notes on the Recent unusually High Barometer Readings in the British Isles: Robert H. Scott, F.R.S.—Turner's Representations of Lightning: Richard Inwards.

Royal Microscopical Society, at 8.

Entomological Society, at 8.—Notes on Flower-Haunting Diptera: G. F. Scott-Elliot.—On the Nomenclature of the Geometridæ: A. Radcliffe-Grote.

THURSDAY, February 20.

Royal Society, at 4.30.—Bakerian Lecture—The Diffusion of Metals: Prof. Roberts-Austen, F.R.S.

Royal Institution, at 3.—Some Aspects of Modern Botany: Prof. Marshall Ward, F.R.S.

Linnean Society, at 8.—On Discoveries resulting from the Division of a Prothallus of a Variety of Scolopendrium vulgare: E. J. Lowe, F.R.S.

Chemical Society, at 8.—Origin of Colour: the Yellow 2:3 Hydroxynaphthoic Acid; Note on Etherification; The Relation of Pinene to Citrene: Prof. Armstrong, F.R.S.

London Institution, at 6.—My Voyage to Siberia: Captain Wiggins.

Numismatic Society, at 7.

Society of Antiquaries, at 8.30.

FRIDAY, February 21.

Geological Society, at 3.—Annual General Meeting.

Royal Institution, at 9.—The Past, Present, and Future Water Supply of London: Dr. E. Frankland, F.R.S.

Institution of Civil Engineers, at 8.

Quekett Microscopical Club, at 8.

SATURDAY, February 22.

Royal Institution, at 3.—Light: Lord Rayleigh, F.R.S.

Royal Botanic Society, at 3.45.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Books.—Cocoa, all about it: "Historicus" (Low).—Links in a Long Chain: Mrs. A. Bell (Philip).—Annuaire de l'Observatoire Municipal de Montsouris, 1896 (Paris, Gauthier-Villars).—Cours de Physique de l'École Polytechnique: Prof. M. Bouty, premier supplément (Paris, Gauthier-Villars).

Pamphlets.—Atlas des Isanomales et des Variations Séculaires du Magnétisme Terrestre: Lieut.-Géneral A. de Tillo (St. Pétersbourg).—Royal Gardens, Kew: Hand-List of Orchids cultivated in the Royal Gardens (Eyre).

Serials.—Scribner's Magazine, February (Low).—Transactions of the English Arboricultural Society. Vol. 3, Part 1 (Carlisle).—Geological Magazine, February (Dulau).—Engineering Magazine, February (Tucker).—Massachusetts Institute of Technology, Boston, Annual Catalogue, 1895-96 (Cambridge, Mass.).—American Journal of Science, February (New Haven).—Bulletin de l'Académie Royale des Sciences, &c., de Belgique, Tome 30, No. xii. (Bruxelles).—Proceedings of the Royal Society, Edinburgh, Session 1894-95, Vol. xx. pp. 481-546 (Edinburgh).—Bulletin of the American Mathematical Society, January (New York, Macmillan).

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