THURSDAY, APRIL 9, 1891.

ANOTHER DARWINIAN CRITIC.

On the Modification of Organisms. By David Syme. (London: Kegan Paul, Trench, and Co.)

THIS little book is one of a class that was more common twenty years ago, when any acute literary critic thought he could demolish Darwin. Mr. Syme has, however, the advantage of having read some of the best works both for and against Darwinism, and is thus able to support his views by quoting writers of eminence. He begins boldly. In the table of contents of the first chapter we find such headings as, "A fatal admission—Darwin's definition misleading—Refutes his own theory." But when we look for the proof of these statements we find they rest on misconception, misrepresentation, or misquotation. A few examples will show that this is the case.

At p. 3, after quoting Darwin's definition of natural selection as "The preservation of favourable individual differences and variations, and the destruction of those that are injurious," Mr. Syme remarks: "Natural selection is therefore another name for the struggle for existence, and I cannot help thinking that the latter is much the better expression of the two, being less ambiguous." What are we to think of a critic who thus, at the very outset, misrepresents his author, by stating without qualification that of the three factors which lead to natural selection—rapid multiplication, heredity with variation, and the struggle for existence—the first two may be left out of consideration, and the last taken by itself as synonymous with the resultant of the whole? And this misrepresentation he makes use of again and again in his argument. At p. 8 he tells us that "Darwin never acquired the art of using precise language"; and, after quoting some of his statements, adds: "Had he substituted for natural selection the expression 'struggle for life,' there would, it is true, have been less novelty about it, but there would also have been less liability to error, both on his own part and on the part of his readers." And now, having repeated his own erroneous definition twice, he seems to have convinced himself that it is Darwin's also, for he says, in the same paragraph: "We have seen that he defines natural selection as 'the struggle for existence,' and again as 'the survival of the fittest.'" Mr. Syme actually gives both these terms between inverted commas as if they were Darwin's own words, and then goes on to show that elsewhere he speaks of the two as different things; and concludes by informing us that "Such inaccuracies of expression occur in almost every page of his writings"!

One more example of this system of criticism. At p. 10, after quoting a passage from Darwin about the origin of the eye, and of organs used only once in a lifetime, as within the power of natural selection, the critic goes on to say: "It is evident that we have here two kinds of natural selection. We have a natural selection which selects or preserves only, and we have another which adapts, modifies, or creates"; and then there is a quibble about the two being fundamentally different. But what we have to observe here is the word "creates," which Mr. Syme has brought in with an "or," and which he very soon imputes to Mr. Darwin himself. For example, at p. 15, he says: "in other places he insists that variations are created by natural selection"; and again at p. 17, he says:—"We have seen that Darwin has put forth two distinct and contradictory theories of the functions of natural selection. According to the one theory natural selection is selective or preservative and nothing more. According to the other theory natural selection creates the variations, and we are left to infer that it afterwards selects them." He adds that Darwin evidently favoured this latter view, and therefore he (the writer) "shall assume that it is a creative as well as a preservative and destructive process"!

Having thus, by means of various misconceptions and misquotations, shown his readers how inaccurate, illogical, and inconsistent Darwin often is, Mr. Syme surveys the position from his own superior stand-point, and points out the road over which he is about to lead them in a passage which, for its amazing statements and supreme self-confidence, deserves to be quoted.

"I venture to dissent altogether from Darwin on the question of the functions and tendency of natural selection. I maintain that natural selection does not create the favourable variations, at all events in the sense understood by him, and that it does not even preserve them. I go further than this, and assert that it does not even exterminate the unfavourable variations. I shall endeavour to show that it is neither creative, preservative, nor greatly destructive; that it neither produces nor preserves the fit, nor exterminates the unfit, and that, so far from being beneficent in its operation, as Darwin and his followers represent, the struggle for existence is, on the whole, pernicious, and tends to produce disease, premature decay, and general deterioration of all beings subjected to its influence."

How Mr. Syme establishes all this must be studied in the pages of his book. He appears to satisfy himself, and may perhaps satisfy such of his readers as know nothing from any other source of the subjects he discusses. Those who have such knowledge may estimate the value of Mr. Syme's teaching by his explanation of mimicry, which is, that natural selection has nothing to do with it, but that insects choose environments to match their own colours. He tells us that these extraordinary resemblances only occur among insects that are sluggish, and that, "to account for these likenesses to special objects, animate or inanimate, we have only to assume that these defenceless creatures have intelligence enough to perceive that their safety lies in escaping observation."

In a similar manner he deals with the supposed adaptations of flowers for cross-fertilization by insects. After quoting from Darwin the curious mode in which *Coryanthes macrantha* is fertilized by bees, he says that it is "utterly incredible" that this complex arrangement has been provided for the purpose of securing cross-fertilization, adding: "It is far more probable that the insects made use of the existing apparatus than that it had been expressly provided for them in order to get the alleged purpose effected." What use it can be to the insect to be imprisoned in a floral water-cistern he does not deign to explain: neither does he tell us how the flower comes to possess this complex structure. Topsy's explanation, that "it growed," is perhaps thought sufficient.

But though Mr. Syme believes that he has utterly smashed Darwinism, he still professes himself an evolutionist, and in his last chapter gives us an alternative theory in the intelligence of the vegetable and animal cells.

"They are," he says, "the sole agents employed in the construction, and afterwards in the maintenance, of the most complex organisms, and their economic and social organization is both comprehensive and complete. When an injury occurs to any part of the organism, they collect in force on the spot for the purpose of effecting repairs, which they execute with singular skill and judgment, varying the means employed according to the circumstances of each particular case."

This theory will be found much more thoroughly as well as more amusingly set forth in Mr. Samuel Butler's "Life and Habit"; but, whatever may be thought of its merits, few evolutionists will accept it as a complete and sufficient substitute for the Darwinian theory of natural selection.

Mr. Syme has a considerable reputation in other departments of literature as a powerful writer and acute critic; but he has entirely mistaken his vocation in this feeble and almost puerile attempt to overthrow the vast edifice of fact and theory raised by the genius and the life-long labours of Darwin.

Alfred R. Wallace.

METALLURGY.

An Introduction to the Study of Metallurgy. By Prof. W. C. Roberts-Austen, C.B., F.R.S. (London: Charles Griffin and Co., 1891.)

The well-known efforts of Prof. Roberts-Austen in leading students to appreciate the application of correct principles to the metallurgic art, led to high expectation when the publication of his "Introduction to the Study of Metallurgy" was announced, and this expectation has not been disappointed. Although, as regards minute and accurate description of detail and general thoroughness, the volumes of Percy stand alone, and although more condensed works, such as Phillips and Bauerman's "Elements of Metallurgy," are available for the student, yet there has been a distinct want of a systematic exposition of the general principles of metallurgy, and of clear statements as to the physical characters of metals and alloys. These are more especially needed by students on the threshold of metallurgy, who desire to enter profitably on the study of the more or less disconnected details of the art as applied to the several metals, such as are to be found in the monographs of Sir Lowthian Bell, and of the late Sir William Siemens. The evident purpose of the volume is to meet this want, the author having deliberately subordinated details of smelting operations, in order that he might deal at length with the physical properties of metals and the constitution and characters of alloys, modified as these properties often are by thermal treatment, and by the presence of small quantities of foreign elements. Such questions are treated with much wealth of research, and abundant reference to authority. The book will hardly be popular with the class of students who merely attempt to "cram."

The importance of the amount of impurity, which may either be valuable or prejudicial, in the application of metals and alloys in the arts, is strikingly shown by the aid of elaborate curves, among which may be noticed one indicating the influence of minute proportions of phosphorus on steel (p. 24), and others showing the action of nickel on iron, and of foreign elements on gold. A remarkable example of the effect of minute variations in the proportions of an added element is noted (p. 117) in the case of die-steel, which when containing 0·8 per cent. of carbon, may be made into dies that will strike 40,000 coins each, but which would be rendered practically useless by variations of under 0·2 per cent. of the carbon.

In noticing the special section of the work comprised in the first four chapters, the evidently strong points of the author should not be overlooked. He rightly regards it as of much importance that the student should be made conversant with the observations and works of the early metallurgists, with the reasoning which led to their practice, and with the advances which have, up to the present day, resulted from their labours. The treatment of metallurgy, as embodied in this section, is a novel feature, and must have involved much more labour and research than would at first sight be gathered from the fact that it has been possible to compress the conclusions into little more than a hundred pages. In the adoption of this treatment, the author has marked out for himself a course that cannot be too highly commended. The student will now be able to attack with advantage the difficulties he will have to grapple with later, and to discount erroneous statements and false reasonings, which, if presented under the guise of authority, prove to be veritable stumbling-blocks in practice, until the stern school of experience teaches better things.

Metallurgical processes are not treated in the detail usual in metallurgical text-books, and here the essential character of the author's method comes into prominence. Furnaces and apparatus, though classified and illustrated in the previous chapters, hardly appear again, but the general scope of metallurgical procedure is exemplified by means of typical processes, and these occupy thirty-five pages.

The classification of processes (pp. 238–241) well deserves praise. It presents to the student, in a few pages, and in a way not be found elsewhere, the essential and distinctive characters of the whole of the methods of metallurgy, whether by "dry" or furnace processes, by the solvent action of mercury, by solution and precipitation (the so-called "wet" processes), or by the latest and already important methods which involve electrolysis. The more typical of these have been selected for descriptions, which are illustrated by the aid of diagrams showing the essential steps and sequence of operations. By the aid of these diagrams, the student has clearly presented to his eye such excellent but apparently complicated processes as the smelting of copper ores by the Welsh method (p. 242), and the Freiberg method of smelting complex ores (p. 250), which have hitherto been found very confusing, even when the descriptions have been very carefully written. With such guidance, the details of furnaces, of successive roastings and fusions, as fully elaborated in other works, can be studied without confusion or difficulty.

Wet processes are made equally clear in the same way, and the only fault to be found with this division of the work is that it is too brief. Whilst the work merits ungrudging approval, it may be observed that the author, who has done well in giving special prominence to the influence of foreign matter on the metals and alloys, has perhaps exaggerated the relative value of the study of thermal and mechanical treatment of metals as compared with improvements in smelting operations. This has led him to subordinate and curtail descriptions of metallurgical processes, which is to be regretted, because his diagrammatic methods of description are most effective. This part of the work will well bear expansion in the next edition, which will doubtless soon be called for.

THOMAS GIBB.

THE RELATIVITY OF KNOWLEDGE.

The Prevailing Types of Philosophy : Can they Logically reach Reality? By James McCosh, LL.D., Litt.D., &c. (London : Macmillan and Co., 1891.)

WHAT is reality? The plain man when he opens his eyes and stretches forth his hands never questions the practical reality of the objects which surround him. The walnuts which he sees on his plate are resistant to the fingers, the wine in his glass has a *bouquet* the reality of which he can readily put to the satisfactory test of the palate. But the psychologist seeks to analyze these intuitions and perceptions, the validity of which no sane man doubts. He follows the intuitions and perceptions home, and finds that they are in some way associated with certain material transactions within the ivory casket of the human skull. And then he begins to speculate about an ultimate reality behind the reality of perceptions. The blush on the peach—is it really inherent in the fruit, or is it merely a mode of vibration of my own brain-molecules? And these brain-molecules—is their so-called matter aught but a figment of the mind? Or is this mind merely a subtle secretion of the grey matter of the cortex? Thus, many questions can be asked, and many answers given.

Dr. McCosh, in the little volume under notice, inquires, What do the leading philosophic systems of the day make of reality? "I am to put this question," he says, "to each of them. Do they acknowledge it, or do they deny it? Do they accept it in whole, or only in part? Do they attempt to prove it, or simply assume it?"

What, we naturally ask, is the "reality" concerning which Dr. McCosh asks these questions? He replies:—

"The only way of showing its nature is to point to examples of it. We look on the wall of the room in which we sit, and know it to be real. We see a bird flying, and know it to be an actuality. We are conscious of ourselves in pain, and we are sure of our own existence in a state of pain."

In other words, by "reality" Dr. McCosh means that practical reality which no man in his senses denies or thinks of denying.

The question Dr. McCosh sets himself to consider is therefore this:—How far do the leading philosophic systems of the day agree to regard *as absolute and ultimate* that practical reality, *relative to man as an organism*, in which every man out of Bedlam implicitly believes? Such being the true nature of his question, it is scarcely a matter of surprise that Dr. McCosh should find that neither the experiential and sensational schools nor the *a priori* or Kantian school, nor the Scottish school will agree to do anything of the sort. They all see too clearly the difference between relative and absolute reality to identify them after the fashion of the ex-President of Princeton College. Or, if individual members of any of these schools do so identify them, they are for the most part honest enough avowedly to confess that they do so on theological and not on philosophical grounds.

Into the theological aspects of the question we cannot enter here. Suffice it to say that we have never expressed aught but respect for those who believe that the world was created by Divine fiat, and that man was endowed with faculties which enable him directly to apprehend its existence and nature. But we do not think that philosophy apart from revelation would have been led to this conclusion; and we doubt the wisdom of those who appeal to philosophy in its support.

We would have it clearly understood that it is not to the appeal which Dr. McCosh makes, but the tribunal to which he makes it, that we take exception. "Agnosticism," he says, "is upheld and propagated in the present day by several influential men, such as Mr. Herbert Spencer and Prof. Huxley. It is in the air, and our young men have to breathe it and suffer the consequences. It is evidently exercising a relaxing influence on the faith and doctrinal convictions of the rising generation. It is in my view the grand office, at present, of the higher philosophy, to meet and expose this doubting spirit." We would counsel Dr. McCosh to substitute in future editions for "higher philosophy" other words, and to boldly assume at the outset the theological position on the grounds of fundamental and unalterable conviction. Any conclusions of philosophy, whether experiential, Kantian, or Scottish, which run counter to this fundamental assumption will then, for him and his followers, stand *ipso facto* condemned. If the relativity of human knowledge be one of these unorthodox conclusions, Dr. McCosh is assuredly right for his own part in rejecting it on the grounds of unalterable conviction, though mere unaided and uninspired philosophy has, as his little book plainly shows, conspired with one voice to sanction it.

Dr. McCosh writes clearly, tersely, and forcibly, and gives in a short space an excellent account of the conclusions reached by the prevailing types of philosophy so far as they concern the question of reality.

C. LL. M.

OUR BOOK SHELF.

An Explanation of the Phonopore, and more especially of the Simplex Phonopore Telegraph. By C. Langdon-Davies. (London : Kegan Paul, Trench, Trübner, and Co., Limited, 1891.)

THE present work contains a plain explanation of the phonopore, and is written in a manner that will be intelligible to all who are acquainted with the elements of electric technics and practical telegraphy. The phenomena which led to the invention of this instrument were the sounds generally known to telephonists as induction noises: many attempts seem to have been made to overcome them, and success has been achieved only at a great cost. The author, in investigating the causes of these disturbances, was led to make use of the force from which they were derived, and in the phonopore we have an instrument which is based on the results of the investigation.

This system of telegraphy is used in conjunction with the ordinary system, with the addition of certain instruments and apparatus. Short and rapid electrical impulses (which have no effect on the telegraphic instruments as usually employed) being sent successively along the ordinary wire by means of other apparatus forming part of the system, this pulsatory current is enabled to work relays and other telegraphic instruments; no interruption is caused in the ordinary telegraphic signals, which may be worked as usual and transmitted through the same conductor at the same time.

Of the instruments in the above-mentioned circuit, the principal one is that called by the author the phonopore; its action consists in allowing the vibratory impulses to pass freely through it, while ordinary currents, such as those used in telegraphic working, are stopped by it.

Previous to the description of the simplex phonopore telegraph, the author relates some of the experiments that led him to devise these ingenious instruments. The technicalities pertaining to the principles of their construction and to the methods of arranging them in circuit are then entered upon, and finally a comparison is drawn between the ordinary telegraph and the phonopore telegraph.

The five reports written by well-known men and inserted at the end speak of this new system as highly valuable. The following brief extract shows in a few words Prof. Silvanus P. Thompson's view of the utility of the system:—"In my opinion the experiments and demonstrations have been successful in establishing the entire practicability of satisfactorily working the simplex phonopore telegraph simultaneously with the ordinary telegraphs (both needles and Bright's bells) upon the same ordinary telegraph line."

In concluding our remarks, we must add that the subject-matter is presented in English and French, printed side by side; numerous illustrations are given showing the actual instruments, while others, in diagrammatic form, explain the methods of arranging them in circuit. Altogether the work is one that will be read with interest by electricians and by all those connected with practical telegraphy.

The Naturalist of Cumbrae: being the Life of David Robertson. By the Rev. T. R. R. Stebbing. (London: Kegan Paul, Trench, Trübner, and Co., 1891.)

Mr. Robertson, who is now in his eighty-fifth year, has done much good work as a marine zoologist; and the present record of his career will interest not only his personal friends, but all readers who admire energy, enthusiasm, and intellectual resource. It is no easy task to write a biography of one who is still alive, but Mr. Stebbing has done his work skilfully and with good taste, erring only occasionally by reference to small details which are scarcely worthy of a place in a serious narrative. For many years, as a boy and young man, Mr. Robertson worked as a farm labourer; but, having much intelligence, he missed no opportunity of cultivating his mind, and he contrived to fit himself for the study of medicine in Glasgow. Although he passed through the regular medical course, he preferred business to the life of a doctor; and he was successful enough to be able, in 1860, to retire with a competency. He had long been interested in various branches of natural science, and now he had leisure to gratify his tastes to the utmost. He settled on the Island of Cumbrae, and there he has since worked at marine zoology so steadily, and with so ready a power of interpreting observed phenomena, that he has added considerably to our knowledge, and has had many opportunities of being of service to eminent naturalists, with whose requests for specimens or information he has always been delighted to comply. It is pleasant to read of the friendships thus formed, and of the fine qualities which are so cordially appreciated by all to whom Mr. Robertson is personally known. The book belongs in some ways to the class which Mr. Smiles has made popular, but Mr. Stebbing's hero is distinguished by geniality and modesty of character from the type which most people are apt to associate with the idea of "self-made men."

By Track and Trail: a Journey through Canada. By Edward Roper. (London: W. H. Allen and Co., 1891.)

This book is rather too long, but it contains many interesting passages, and will give much pleasure to any readers who may have special reasons for desiring to obtain information as to the prospects of settlers in the Dominion. Everything set down with regard to agricultural matters is a repetition of what the author heard from men whom he either knew or believed to be trustworthy. His general conclusion is that there is no country under the British flag where a prudent settler has better chances than in Canada. In the course of his narrative he gives some very bright descriptions both of places and people; and he has brought together some valuable notes on the aborigines. Numerous reproductions of original sketches by the author illustrate his story.

An Etymological Dictionary of the German Language. By Friedrich Kluge. Translated from the Fourth German Edition by J. F. Davis. (London: George Bell and Sons, 1891.)

Prof. Kluge's work, of which this is a translation, is well known to all students of the etymology of the German language. The author has not only extensive knowledge, but a sound and penetrating judgment; and he displays a remarkable power of presenting concisely and lucidly results at which he can have arrived only after careful and elaborate research. Mr. Davis's translation is in every way worthy of the original, and no one who may have occasion to use it will fail to appreciate the skill with which he has accomplished a difficult and useful piece of work.

Nature's Wonder-Workers. By Kate Lovell. (London: Cassell and Co., 1890.)

In this book, Miss Lovell tells the life-histories of various insects, her object being "not so much to teach as to give fresh interest to the living, often despised creatures which constantly cross our path." She writes clearly and gracefully, and the information she presents has been derived from the best and latest authorities on entomology. The volume is well illustrated, and will stimulate the interest of its readers in some of the more curious facts of natural science.

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.*]

Phosphograms.

If a glass plate (or a celluloid film) is evenly coated with phosphorescent calcium sulphide in a suitable menstruum, and then exposed for a few minutes in a photographic camera, a phosphorescent negative can be taken of a sunlit view. When this plate is carefully applied to an ordinary photographic film for a short period in the dark room, on developing the latter a reversed negative (or *phosphogram* as I prefer to call it) of the original picture is permanently produced. If the picture consists (as in my case) of a vista of smoke-stained chimney-pots, the slated roofs and stuccoed walls of adjacent houses, the negative, under favourable circumstances, will show considerable traces of natural colours. In the case of phosphorescent calcium sulphide, which glows with a beautiful purple violet light, it is not difficult to understand why it acts on the gelatine film so readily as it does. Both the phosphorescent rays and the most active photo-chemical rays belong (as is well known) to the most refrangible rays of the solar spectrum; that the one should thus react on the other is not surprising.

If, instead of using the colourless calcium sulphide, as above described, we stain the mixture with an alcoholic solution of rosanilin acetate before applying it to the glass plate, on exposure to daylight it glows with a deeper intensity, and it still acts, although with less vigour, on a photographic film. M. Verneuil (*Comptes rendus*, civ. 501), and M. Becquerel (*ibid.*, 551), have investigated the causes of phosphorescence in calcium sulphide from a chemical standpoint, and the latter has shown that the colour of the light may be changed to a bright green by the admixture of a trace of lithium carbonate or of potassium persulphide, and to an orange-yellow by the addition of manganese peroxide. The green phosphorescence acts feebly, the orange-yellow scarcely at all, upon the ordinary gelatine film. The bromo-iodide plates are clearly only imperfectly responsive to coloured rays. To attempt coloured photography with the present dry plates is eminently unsatisfactory. By staining the film some makers have succeeded in increasing the sensitiveness of the plate to the less refrangible red rays, but my own experience leads me to conjecture that the green and green-yellow rays are quite as difficult to intercept by our modern plates as the deep-red, if they are not more so.

Brighton, March 28. W. AINSLIE HOLLIS.

Neo-Lamarckism and Darwinism.

UNDER the above heading (NATURE, March 26, p. 490) Prof. Henslow gives some criticisms of my paper on the Alpine flora, which seem to show that, for the sake of brevity, I unintentionally rendered my views obscure to him and possibly to others who share his opinions. It is therefore fortunate that he should have stated his difficulties, in order that I may try to explain what it was I intended, taking the various points in succession.

(1) I wrote "dwarfing may be—doubtless often is—the direct result of environment, as lack of nourishment." This, I believe, is strictly true as regards individual plants, and I do not think it affects the argument that dwarfing may also be due to other causes. Certainly, insufficient nutriment may cause extreme dwarfing altogether apart from cold: for instance, I very well remember some minute but flowering specimens of *Matricaria*, only an inch or so high, found by Mr. J. W. Horsley in a dry place near Chiswick, where the normal fully-developed form of the plant is abundant.

And if dwarfing is the result of cold, how is it explained that plants on different soils at the same altitude vary so much in this respect? Thus, along Swift Creek, in Colorado, *Mertensia sibirica*, which grows in very damp ground and in the rich vegetable deposits on the immediate banks of the creek, is large, tall, and rank, with broad leaves; while a species of the same genus, growing close by on the more exposed, dry, and more barren prairie land, is very much smaller, more compact, and with narrow leaves. Again, at the same place, *Oxytropis lamberti*, when growing in the damper ground by the creek, is tall, rank, and has white or very pale flowers; while on the adjoining prairie the same species is low, depressed, altogether smaller, and with crimson flowers, which turn purple in drying for the herbarium.

I did not say, and certainly would not say, that dwarfing was always the result of bad or insufficient nourishment; but neither is it always or normally the direct result of cold.

(2) When dwarfed varieties or forms have been produced by environmental conditions or otherwise, it is natural enough that they should tend to revert to the original type when removed again to the original conditions. Nevertheless, true Alpine dwarfed *species* do retain their characters when grown at low altitudes artificially. Florists' catalogues are full of allusions to dwarf varieties and species, most of which are very constant. As I write, I can see a clump of *Scilla sibirica* out of the window, and certainly it appears to me to have the true characters of the species, and the long cultivation which this species has undergone has not changed it into a large and more diffuse species like *Scilla nutans*. Minute species of *Narcissus*, *Gentiana*, and *Silene*, which come from mountain regions, may surely be grown in England without starting up and simulating the large temperate-region forms of those genera. I don't mean to say that dwarfing, like other specific characters, may not become changed or lost in time if the circumstances are favourable, or the utility of the peculiarity ceases, but I think it will be generally admitted that low stature, as a specific character, often proves as constant and hereditary as other specific characters.

(3) Surely it is not necessary to demonstrate that tall plants would be likely to be injured by the winds on Alpine summits? The trees at timber-line in Colorado would, I should think, convince anybody of this. Those which are exposed frequently bear branches only on the side towards the valley, the others having succumbed at an early stage to the violence of the winds. The trees at their highest limit are shorn off almost as if with a knife, so that, going down the slope, one does not meet with a full-sized tree until the topmost branches are able to obtain some shelter from the tree above it; so that, although the trees grow to timber-line, they do not raise their summits above it. Also, one may find some old stumps, perhaps fifty yards above the present timber-line, on the Sangre de Cristo Range, showing that, for some reason, trees which formerly grew there have since succumbed.

(4) I have not myself made any experiments to test the additional warmth that might be derived from close proximity to the ground, but I think the point is well established. The rocks and the surface of the ground would surely retain a certain amount of heat, and beds of vegetable tissue, such as peat, do not readily cool. Thus, Mr. E. J. Lowe relates (*Conchologist*, 1891, p. 4) that, in the great frost of 1860-61, the River Trent was frozen over; but a drain, cut through a bed of organic soil, though only a foot wide, and containing less than 12 inches in depth of water, remained unfrozen.

(5) As to "partial shelter": is it not clear that dwarfed plants, which grow close to the ground, and often between rocks or under projecting ledges (as in the manner of Alpine plants), will be partially sheltered, and thereby advantaged?

(6) Of course, I do not suppose that plants *know* anything about the coming season! Only this, those plants which naturally grew quickly and through rapid metabolism came to early maturity, would survive, while those which did not do so would perish. It is a simple example of natural selection. Various weeds and other wild plants, notably *Cleome integrifolia*, are in Colorado often accidentally sown as seeds dropped from hay, and thus appear round human habitations some hundreds of feet above their proper altitude. They grow and flourish, and duly flower, but they cannot mature their seeds in time, and hence never perpetuate themselves at these altitudes. The Indian corn, *Zea maïs*, varies considerably in its period for maturing. It can be grown in England, but does not sufficiently often mature to be a paying crop. In America, the quickly-maturing varieties can be grown at greater altitudes and latitudes than the others. Given such variation, is it not obvious that natural selection would preserve the more rapidly-maturing kinds where the summer was short? Let anyone take varieties of Indian corn differing in the period of maturing, and grow them at the extreme altitude at which this species can be cultivated, and it will not take a botanist to predict the success of the one and the failure of the other.

T. D. A. COCKERELL.

The Whirling Ring and Disk.

I REGRET that the interjection of a paradoxical rider to my proposition regarding a whirling ring should have had the effect of embedding in a storm of protest Prof. Ewing's interesting point concerning a disk. Let me do the best I can to set matters straight and clear.

First, it is plain that my rider concerning a cable was not well worded, and whereas it is quite true that a weightless steel girdle round the earth would be broken by its whirling tension, it would have been better to avoid any appeal to flotation as a practical means of securing weightlessness. The simplest plan is to abandon the cable illustration altogether as a not very pertinent or useful one. Nevertheless, it is plain that a cable fixed at two distant points and hanging free between them is liable to break, whether it be hanging downwards or floated upwards, *i.e.* whether it be supported too little or too much; and it further appears that if it be stretched to the curvature of the earth the slight difference between its true and apparent weights, *i.e.* its centrifugal force alone, is sufficient to break it.

But now returning to the main proposition, that the critical velocity at which a whirling ring breaks is the square root of the ratio of its tenacity to its density, no one calls it in question; some, indeed, say it is well known. That it was known, or at least so easily knowable as to be practically known, to applied mathematicians, is manifest; but that it is well known to practical men I have some reason for doubting. However that is of no consequence. What I write to point out is how singularly nearly the result for a ring agrees with the result for a disk of the same size.

For, as Prof. Ewing's letter shows, the critical peripheral speed of a uniform disk with a very small hole in the centre, when it is on the point of flying, is

$$\sqrt{\left(\frac{4T}{(3+\mu)\rho}\right)},$$

whereas for a ring it is the same expression without the $4/3\frac{1}{4}$.

Thus, then, a perforated disk of uniform thickness, although its greatest stress occurs round its central aperture, yet can stand only 11 per cent. greater angular spin than a ring of the same material fitting its circumference and devoid of all radial support.

A disk without a hole can, as Prof. Ewing says, stand 40 per cent. more rotations per minute than a perforated disk can stand (*i.e.* $\sqrt{2}$ times the speed), even though its perforation is a mere needle-prick or indeed is absolutely infinitesimal, so long as it destroys radial coherence across the centre.

I learn also from Prof. Ewing that Mr. Chree has obtained results for an oblate spheroid, which, reduced to an approximate disk, show that the stresses and strains at any point of the ellipsoid are $\frac{8}{11+\mu}$ths of what they are at the corresponding point of an unperforate disk of the same size but of uniform thickness (μ being Poisson's ratio).

I am obliged to Mr. Boys for calling attention to Maxwell's early memoir on the subject ("Scientific Papers," vol. i. p. 61), with which I was not acquainted. Since Maxwell uses an unusual notation, it may be convenient to note that his μ is volume-elasticity, his m is twice rigidity, and E = Young's modulus; so that his E/m is merely 1 + Poisson's ratio. There are, however, a few misprints with regard to signs.

Prof. Worthington's remarks refer to a straight *bar* with free ends, not to an endless band. It is true, however, that there was no need to drag in vibrations; the dangerous tension will be set up in a straight portion of any endless band, running in the direction of its length with the critical speed $\sqrt{\left(\frac{T}{\rho}\right)}$, by the agency of the curved portions, which necessarily exist somewhere.

It may save some confusion to mention that the suffixes of Prof. Ewing's radial and tangential tensions have become interchanged in the first column of his letter on p. 462 (March 19).

The observations of Prof. Greenhill are best dealt with under a separate heading.

Oliver J. Lodge.

March 21.

Formation of Language.

Permit me to reply to your correspondent Mr. W. J. Stillman, on the "Formation of Language" (Nature, March 26, p. 491). The interesting fact he records of the spontaneous invention and use of child-names for objects is not unknown to philologists. The phenomenon has been previously noticed, among others, by Miss Watson, of Boston, and Dr. E. R. Hun, of Albany, U.S.A.; by Archdeacon Farrar, in the case of Indian children left by themselves for days together in Canadian villages; and by M. Taine, in his work "De l'Intelligence." Numerous examples of children's language are given by Dr. Horatio Hale (philologist to the U.S. Wilkes Exploring Expedition), who has made a special study of the spontaneous development of roots among children the basis of his remarkable theory of the origin of linguistic stocks. Full details will be found in a paper on the "Development of Language," read before the Canadian Institute of Toronto, April 1888, and in an address on the "Origin of Languages and the Antiquity of Speaking Man," in the Proceedings of the American Association for the Advancement of Science, Buffalo, 1886, vol. xxxv. The occurrence has been often noticed in the families of philologists—the most noteworthy instance being that of the young nephew of the well-known Sinologist Dr. George von Gablentz. This boy, before he learned his mother-tongue, called things by names of his own invention. The constant elements were the consonants, the vowels being varied, and employed as they were deeper or higher to denote greatness or smallness. The root for round objects was *m-m*; a watch, plate, and the moon was *mem*, a large round dish or table, *mum*, and the stars *mim mim mim*; an ordinary chair was *lakail*, a great arm-chair *lukull*, and a little doll's chair *likill*. A distinguished Accadian, Chinese, and Semitic scholar, the Rev. C. J. Ball, makes no secret of the fact that, between the ages of six and eight, he and his young brother had names of their own devising—perfectly arbitrary monosyllables and dissyllables for several of the small tools and toys they valued most. Mary Howitt relates in her autobiography (edited by her daughters) that the silent sadness of the Quaker home circle extended to the nursemaids, and that in consequence of this the eldest child, her sister Anna, did not learn to talk until she was four years old. Long after they could talk, "being left chiefly to converse together, our ignorance of the true appellations for many ordinary sentiments and actions compelled us to coin and use words of our own invention. To sneeze was to us both *okis-kow*, the sound which one of our parents must have made in sneezing." Here we get a true *onomatopœia*, as in the monosyllable *mea*, employed by one American child for "cat"; in another child's vocabulary the extraordinary trisyllable *shindikik* designated that animal. The association of ideas and extension of meaning are often very suggestive—viz. *migno migno* = water, wash, bath; *waia waiar* = black, darkness, Negro. As in the case of the name for water, *bhumboo*, cited by Mr. Stillman, the vocables are often of two syllables, rarely of three. It is interesting to note the continued use of the little boy's own name for water as a means of identifying the acquired Italian *aqua* for the same object, as frequently happens with adults struggling to express themselves in a foreign tongue. Reduplication seems also to characterize these "child languages" like those of some savage tribes, and plurals are formed by repetition. The syntax, Dr. Hale remarks, resembles that of deaf-mutes and gesture language. If left to themselves there seems no reason why children with this aptitude should not develop a vocabulary at least as extensive as that of Dr. Farrar's three peasants, "who conversed for a long while without employing more than one hundred words." Many cases of "child language," no doubt, have passed away unrecorded. Soon after the children mix much with adults, the special vocabulary begins to die out. It is possible that the use of such spontaneously developed root-words might be prolonged among the children of the poorer classes, so often cared for by children but little older than themselves. The *crèches* of our large towns might afford further evidence of abnormal developments of this apparently inherent inventive linguistic faculty.

Brighton, April 2.

Agnes Crane.

Mr. W. J. Stillman winds up his interesting letter on the above subject by inquiring whether any of the readers of Nature have made observations analogous to his own on the development of language in children.

I have on several occasions requested parents to note down the sounds emitted by their children from their birth to the time when those sounds seemed to become developed into articulate words with a certain amount of intelligent meaning, but I have not obtained any satisfactory results. The subject has, however, been studied, and described with considerable detail, by that well-known member of the French Academy, M. H. Taine, in his work, "De l'Intelligence" (2 vols., 3rd edition, Paris, 1878). I would particularly refer to chap. ii., sec. 5, and also to that part of a long note at the end of vol. i., entitled "Acquisition du Langage par les Enfants" (pp. 356–383).

C. Tomlinson.

Highgate, N., March 31.

On Frozen Fish.

MR. F. H. P. COSTE (*ante*, p. 516) supplies the reference to the statement I had in memory as to "one of the Arctic voyages" (*ante*, p. 440), and my necessarily imperfect quotation does not materially differ from the actual statement.

With regard to Mr. Turle's letter (*ante*, p. 464), I would remark that my words, "comparatively shallow" waters, were not intended to apply to "two feet of water," and I would suggest that the innumerable sticklebacks embedded in the ice, and which did not revive, were probably dead (from suffocation) before they became so embedded.

It is well known that insects which habitually hibernate as larvæ or pupæ do not suffer from being frozen for a lengthened period. On the other hand, they suffer greatly in "open" winters with frequent alternations of wet, warmth, and cold. Therefore, from an entomological point of view, the season of 1891 promises to be an unusually productive one.

It is not my intention to return to this subject.

Lewisham, April 3. R. MCLACHLAN.

IN a letter appearing under the above heading in your last issue, the writer asks for well authenticated instances of the recovery of frozen insects. All climbers have, at one time or another, met with butterflies, lying frozen on the snow, on Alpine passes, and many persons have brought down some of these insects, which, on reaching the warmth of the lower regions, invariably recover animation, though when picked up they are so completely frozen, and consequently so brittle, that they break to pieces unless carefully handled. I have frequently, when climbing, placed these frozen butterflies on my hat, and, on descending, have noticed them always fly away. It must often have been a considerable time from the frozen stage till recovery. E. MAIN.

Grand Hotel, Montreux-Territet, April 4.

Quaternions and the Algebra of Vectors.

MY remark about Prof. Willard Gibbs was meant in all courtesy, and I am happy to find it so taken by him. The question between us, being thus a scientific one only, can afford to wait for a fortnight or so:—until my present examination season is past. P. G. TAIT.

THE MULTIPLE ORIGIN OF RACES.

IN NATURE of March 5 (p. 415), the Duke of Argyll has printed a very interesting letter of Mr. Darwin's, from which he drew the inference that the writer "assumed mankind to have arisen . . . in a single pair." I do not think myself that the letter bears this interpretation. But the point in its most general aspect is a very important one, and is often found to present some difficulty to students of Mr. Darwin's writings.

Quite recently I have found by accident, amongst the papers of the late Mr. Bentham at Kew, a letter of friendly criticism from Mr. Darwin upon the presidential address which Mr. Bentham delivered to the Linnean Society on May 24, 1869. This letter, I think, has been overlooked and not published previously. In it Mr. Darwin expresses himself with regard to the multiple origin of races and some other points in very explicit language. Prof. Meldola, to whom I mentioned in conversation the existence of the letter, urged me strongly to print it. This, therefore, I now do, with the addition of a few explanatory notes.

W. T. THISELTON DYER.

Royal Gardens, Kew, March 27.

Down, Beckenham, Kent, S.E., November 25, 1869.

MY DEAR MR. BENTHAM,—I was greatly interested by your address, which I have now read thrice, and which I believe will have much influence on all who read it. But you are mistaken in thinking that I ever said you were wrong on any point. All that I meant was that on certain points, and these very doubtful points, I was inclined to differ from you. And now, on further considering the point on which some two or three months ago I felt most inclined to differ, viz. on isolation, I find I differ very little. What I have to say is *really* not worth saying, but as I should be very sorry not to do whatever you asked, I will scribble down the slightly dissentient thoughts which have occurred to me. It would be an endless job to specify the points in which you have interested me; but I may just mention the relation of the extreme western flora of Europe (some such very vague thoughts have crossed my mind, relating to glacial period) with South Africa, and your remarks on the contrast of passive and active distribution.

P. lxx.—I think the contingency of a rising island, not as yet fully stocked with plants, ought always to be kept in mind when speaking of colonization.

P. lxxiv.—I have met with nothing which makes me in the least doubt that large genera present a greater number of varieties relatively to their size than do small genera.[1] Hooker was convinced by my data, never as yet published in full, only abstracted in the "Origin."

P. lxxviii.—I dispute whether a new race or species is necessarily, or even generally, descended from a single or pair of parents. The whole body of individuals, I believe, become altered together—like our race-horses, and like all domestic breeds which are changed through "unconscious selection" by man.[2]

P. do.—When such great lengths of time are considered as are necessary to change a specific form, I greatly doubt whether more or less rapid powers of multiplication have more than the most insignificant weight. These powers, I think, are related to greater or less destruction in early life.

P. lxxix.—I still think you rather under-rate the importance of isolation. I have come to think it very important from various grounds; the anomalous and quasi-extinct forms on islands, &c., &c., &c.

With respect to areas with numerous "individually durable" forms, can it be said that they generally present a "broken" surface with "impassable barriers"? This, no doubt, is true in certain cases, as Teneriffe. But does this hold with South-West Australia or the Cape? I much doubt. I have been accustomed to look at the cause of so many forms as being partly an arid or dry climate (as De Candolle insists) which indirectly leads to diversified [?] conditions; and secondly, to isolation from the rest of the world during a very long period, so that other more dominant forms have not entered, and there has been ample time for much specification and adaptation of character.

P. lxxx.—I suppose you think that the *Restiaceæ*, *Proteaceæ*,[3] &c., &c., once extended over the world, leaving fragments in the south.

You in several places speak of distribution of plants as if exclusively governed by soil and climate: I know that you do not mean this, but I regret whenever a chance is omitted of pointing out that the struggle with other plants (and hostile animals) is far more important.

I told you that I had nothing worth saying, but I have given you my THOUGHTS.

How detestable are the Roman numerals; why

[1] Bentham thought "degree of variability . . . like other constitutional characters, in the first place an individual one, which . . . may become more or less hereditary, and therefore specific; and thence, but in a very faint degree, generic." He seems to mean to argue against the conclusion which Sir Joseph Hooker had quoted from Mr. Darwin that "species of large genera are more variable than those of small."

[2] Bentham had said: "We must also admit that every race has probably been the offspring of one parent or pair of parents and consequently originated in one spot." The Duke of Argyll converts the proposition.

[3] It is doubtful whether Bentham did think so. In his 1870 address he says: "I cannot resist the opinion that all presumptive evidence is against European *Proteaceæ*, and that all direct evidence in their favour has broken down upon cross-examination."

should not the President's addresses, which are often, and I am sure in this case, worth more than all the rest of the number, be paged with Christian figures?

My dear Mr. Bentham,
Yours very sincerely,
CH. DARWIN.

HERTZ'S EXPERIMENTS.

I.

"OH! yes; I understand it all now. Electricity is the ether;" or, "Yes; it's just like everything else: electricity is a vibration." These are the remarks one hears made by those who think that a few scattered words picked up at a popular lecture make things quite clear. It is no doubt unfortunate that repeating a form of words is a different matter from understanding them, and still more different from understanding the subject they are intended to explain. In this case there is the added misfortune that the form of words is not accurately repeated, and in its inaccurate form does not mean what is true. It is often hardly worth while remarking this to those who make these statements, because the words convey to them little or no signification, and are to them as true as any other unmeaning sentence. The connection between electricity and the ether is certainly not, as far as is known, well described by saying that "electricity is the ether," and we cannot say with any certainty that electricity is or is not a vibration. Hertz's experiments have given an experimental proof of Maxwell's theory that electrical phenomena are due to the ether, and Hertz's experiments deal with vibrations. One cannot, however, say, because the pressure of 15 pounds per square inch exerted by the atmosphere is due to the air, that therefore "pressure is the air": nor even, because a person who studied the properties of the air had studied them by means of sounds propagated through it, can one assert that "pressure is a vibration." It is to be hoped no one will now assert that "electricity is pressure." The example is given to illustrate the absurdity of the statements made as deductions from recent experiments, and not to teach any new theory. And yet one comes across people who, after listening to an interesting lecture Lord Rayleigh might give, illustrated by Mr. Boys's sound-pressure-meter, would make the above statements, and really think they understood them. That blessed word "Mesopotamia" comforted the soul of an old lady with some reason, for religion is to some extent a question of feeling; but in science it is high treason to truth to be comforted by unmeaning sounds—they should produce despair.

It is to be hoped after this tirade that any reader of these articles who comes across statements he cannot understand will not tell himself the lie that he does understand them, nor pretend to others that he does. The subject is very difficult: one that has engaged the attention of thoughtful and clever men for many years, and is still in many parts, even to the most acute, shrouded with difficulties, uncertainties, and things unknown, so that nobody need be the least ashamed of not following even as far as others can go into this wonderful region. If the articles can give to most who read them glimpses which unfold intelligible ideas of even the outskirts of this region, it is all that any writer can reasonably expect who is not one of those masters of exposition who combine the highest scientific and literary abilities.

Consider for a minute the question at issue. That electric and magnetic phenomena are due to the same medium by which light is propagated—that all-pervading medium by whose assistance we receive all the energy on this earth that makes life here possible, by which we learn the existence of other worlds and suns, and analyze their structures and read their histories; that medium which certainly pervades all transparent bodies, and probably all matter, and extends as far as we know of anything existing: this wonderful all-pervading medium is the one we use to push and pull with when we act by means of electric and magnetic forces; and remember that we can pull molecules asunder by this means, as well as propel trains and light our houses. The forces between atoms are controlled by this all-pervading medium, which directs the compass of the mariner, signals round the globe in times that shame e'en Shakespeare's fancy, rends the oak and terrifies creation's lords in the lightning flash. It was a great discovery that proved all concord of sweet sounds was due to the medium that supplies the means of growth to animals and plants, and deals destruction in the whirlwind; and yet the 80 miles depth of our air is but a puny thing compared with the all-pervading illimitable ether.

That there is a medium by which light is transmitted in a manner somewhat analogous to that by which the air transmits sound has been long held proved. Even those who held that light was due to little particles shot out by luminous bodies were yet constrained to superpose a medium to account for the many strange actions of these particles. Now, no one thinks that light is due to such particles, and only a very few of those who have really considered the matter think that it can be due to air, or other matter such as we know. How does light exist for those eight minutes after it has left the sun and before it reaches the earth? Between the sun and earth there is some matter, no doubt, but it is in far-separated parts. There are Mercury and Venus, and some meteors and some dust no doubt, and wandering molecules of various gases, many yards apart, that meet one another every few days, perhaps, but no matter that could pass on an action from point to point at a rate of thousands of miles each second. Some other medium must be there than ordinary gross matter. Something so suble that the planets, meteors, and even comets—those wondrous fleecy fiery clouds rushing a hundred times more quickly than a cannon-ball around the sun—are imperceptibly impeded by its presence, and yet so constituted as to take up the vibrations of the atoms in these fiery clouds and send them on to us a thousand times more rapidly again than the comet moves to tell us there is a comet toward, and teach us what kinds of atoms vibrate in its tail. How can a medium have these contrary properties? How can it offer an imperceptible resistance to the comet, and yet take up the vibrations of the atoms? These are hard questions, and science has as yet but dim answers to them, hardly to be dignified by the name of answers —rather dim analogies to show that the properties supposed to coexist, though seeming contradictory, are not so in reality. One of the most beautiful experiments man knows—one fraught with more suggestions than almost any hundred others—is that by which a ring of air may be thrown through the air for many yards, and two such rings may hit, and, shivering, rebound. These rings move in curved paths past one another with almost no resistance to their motion, urged by an action not transmitted in time from one ring to another, but, like gravitation, acting wherever a ring may be, and yet the air through which they move *can* take up vibrations from the rings, showing thus that there is no real contradiction between the properties of things moving through a medium unresistedly in certain paths round one another, and yet transmitting other motions to the medium. This same air can push and pull, as when it sucks up water-spouts and deals destruction in tornadoes. Hence there seems no real contradiction between a medium that can push and pull and transmit vibrations, and yet offer no resistance to such fragile, light, and large-extended things as rings of air.

It is important to understand something about the properties that this medium must have in order to explain light, electricity, and magnetism, because there is no use expecting a medium to possess contradictory properties. It is also well to recollect that for about two hundred years the existence of a medium by which light is propagated has been considered as certain, and that it would be very remarkable if this medium, which can be set in vibration by material atoms, acted on matter in no other way. It seems almost impossible but that a medium which is moved by atoms, and which sets them into motion, should be able to move such armies of atoms as we deal with in material bodies. Even if we knew nothing of electricity and magnetism, it would be natural to look for some important phenomena due to the action of this medium on masses of matter. The medium is a *vera causa*, and if it can be shown that the same set of properties by which electric and magnetic forces are explained will also enable it to transmit vibrations that have all the properties of light, it will surely be beyond a doubt but that these electric and magnetic actions are those very ones we would naturally expect from the medium that propagates light. Clerk Maxwell some years ago showed that this was so, but as far as any facts known at that time could prove, there were other theories of electric and magnetic actions which explained their known phenomena without the intervention of a medium. The matter stood somewhat thus. The older theories of electric and magnetic force explained all phenomena then known. These older theories assumed that electric and magnetic forces were propagated instantaneously throughout space. That if the sun became electrified, it would instantaneously begin to induce electricity on the earth. That there would be no delay of eight minutes such as occurs between a light occurring on the sun and its acting on the earth. Similarly in the case of magnetic actions, they were supposed to be propagated instantaneously throughout space. It was, no doubt, known that it took time for an electric signal to be transmitted along a conducting cable. This is, however, a very much more complicated problem than the simple one of supposing a body surrounded by a non-conductor to be electrified. Will it or will it not instantaneously act on all conductors in space, and begin to induce electrification on them? As far as was known, such actions as this, actions through non-conducting space, were instantaneous. Such an instantaneous action could not be transmitted by the air. Air cannot send on from point to point any effect more rapidly than a molecule of air can moving carry it forward, and that is only a little faster than the velocity of sound; and there was every reason to know that electric induction through air was propagated much more rapidly than that. There was every reason to believe that electric and magnetic forces acted without any material intervention.

In fact, in these older theories there was no thought of any medium to transmit the actions; it was supposed that electricity acted across any intervening space instantaneously. There is no real difficulty in such a supposition: as far as we know, gravitation is just such an action, and as far as was then known, there was no experiment that disproved the supposition in the case of electric and magnetic actions. It was known that no experiment had ever been devised that could test whether this action was instantaneous or whether it was propagated at a rate such as that of light. It was known that this action was enormously more rapid than sound, but as light goes about 300,000 times as fast as sound, there was plenty of spare velocity. These older theories explained all that was known, and they supposed nothing as to the existence of an intervening medium. Any theory that assumed that induction was not instantaneous, but that energy having been spent on electrification at one place work would be done at another after some time, as in the case of light generated on the sun not reaching the earth for eight minutes, any theory that assumed such a disappearance of energy at one place and its reappearance at another after the lapse of some time must assume some medium in which the energy exists after leaving the one place and before it reaches the other. A theory that only supposes instantaneous action throughout space need not assume the existence of a medium to transmit the action, but any theory that supposes an action to take time in being transmitted from one place to another must assume the existence of a medium. Now, Maxwell's theory assumed the existence of a medium, and along with that led to the conclusion that electric and magnetic actions were not propagated instantaneously, but were propagated with the velocity of light. According to his theory an electric disturbance occurring on the sun would not produce any effect on the earth for about eight minutes after its occurrence on the sun. No experiments were known to test the truth of this deduction until the genius of Hertz brought some of the most beautifully conceived, ingeniously devised, and laboriously executed of experiments to a brilliantly successful conclusion, and demonstrated the propagation of electric and magnetic actions with the velocity of light, and thereby proved experimentally that they are due to that same wonderful, all-pervading medium, by means of which we get all the energy that makes life here possible.

The problem to be solved was, Are electric and magnetic actions propagated from place to place in a finite time, or are they simultaneous everywhere? How can experiments be made to decide this? Consider the corresponding problem in sound. What methods are there for determining the rate at which sound is propagated? An experiment that measures the rate can tell whether that rate is finite or whether it is infinitely great. There are two important methods employed for measuring the velocity of sound. The second is really only a modification of the first direct method, as will be seen. The direct method is to make a sudden sound at a place, and to find how long afterwards it reaches a distant place. In this method there is required some practically instantaneous way of communicating between the two places, so that the distant observer may know when the sound started on its journey. A modification of the method does not require this. It depends on the use of reflection. If a sound be made at a distance from a reflecting surface, the interval of time between when the sudden sound is made and when the reflected sound, the echo, returns, is the time the sound took to travel to the reflector and back again. A well-known modification of this method can be applied if we can secure a succession of sudden sounds, such as taps, at accurately equal intervals of time. We originate such a regular succession of taps, and alter the distance from the reflector until each reflected tap occurs simultaneously with the succeeding incident tap. Or if the distance at which we can put the reflector be sufficiently great, we may arrange it to be such that a reflected tap is heard simultaneously with the second, third, fourth, or any desired succeeding tap. The coincidence of the taps with their reflections can be fairly accurately observed, and a fairly accurate estimate formed of the velocity of sound, *i.e.* the velocity at which a compressing or rarefying of the air is propagated by the air. Instead of altering the distance of the source of sound from the reflector, we may ourselves move about between the source and the reflector, and we can find some places where the reflected taps occur simultaneously with the incident taps, and some places where they occur between the incident ones. This is pretty evident, for if we start from the source towards the reflector, as we approach it we get the reflected taps earlier and the incident ones later than when we were at the source. How far must we go towards the reflector in order that the original and reflected taps may again appear simultaneous? We must go half the distance that a tap is propagated during the interval between two taps: *half* the distance, because in going away from the source we are approaching the reflector and so make a double change—we not only get the original ones later, but we also get the reflected ones earlier, and so coincidence will have again been reached when we have gone half the distance between any pair of compressions travelling in the air. Now, if the taps succeed one another slowly, the distance in the air between any two of them travelling through it will be considerable; any one of them will go a considerable distance from the source before its successor is started after it. If, on the contrary, they succeed one another rapidly, the distance between the travelling taps will be small. In general, if V be the velocity with which a tap travels, and T be the interval of time between successive taps, the distance apart of the taps travelling in the air will be $\lambda = VT$. By arranging, then, that the taps shall succeed one another very rapidly, *i.e.* by making T small, we can arrange that λ may be small, and that consequently the distance between our source of sound and the reflecting wall may be small too, and yet large enough to contain several places at distances of $\frac{1}{2}\lambda$ apart between the source and the reflector where the incident and reflected taps occur simultaneously. Now, a very rapid succession of taps is to us a continuous sound, and where the incident and reflected taps coincide we hear simply an increased sound, while at the intermediate places where the incident taps occur in the intervals between the reflected taps we do not hear this effect at all. In the case of a succession of sharp taps we would hear in this latter place the octave of the original note, but if the original series be, instead of taps, a simple vibration of the air into and out from the reflector, the in and out motions of the incident waves will in some places coincide with the in and out motions of the reflected wave, and then there will be an increased motion, while at intermediate places the *in* and *out* motions of the incident wave will coincide with the *out* and *in* motions of the reflected wave, and no motion, or silence, will result, so that at some places the sound will be great and at intermediate places small. This whole effect of having an incident and reflected wave travelling simultaneously along a medium can be simply and beautifully illustrated to the eye, by sending a succession of waves along a chain or heavy limp rope or an india-rubber tube fixed at the far end so as to reflect the waves back again. It will then be found that the chain divides up into a series of places where the motion is very great, called loops, separated by points where the motion is very small, called nodes. The former are the places where the incident and reflected motions reinforce, while the latter are where these motions are opposed. If we measure the distance between two nodes, we know that it is half the distance a wave travels during a single vibration of the string, and so can calculate the velocity of the wave if we know the rate of vibration of the string. This is the second method mentioned above for finding the velocity of sound. There are so many things illustrated by this vibrating chain that it may be well to dwell on it for a few moments. We can make a wave travel up it, either rapidly or slowly, by stressing it much or little. If a wave travels rapidly, we must give it a very rapid vibration if we wish to have many loops and nodes between our source and the reflector; for the distance from node to node is half the distance a wave travels during a vibration, and if the wave goes fast the vibration must be rapid, or the distance from node to node will be too great for there to be many of them within the length of the chain. Another point to be observed is the way in which the chain moves when transmitting a single wave and when in this condition of loops and nodes, *i.e.* transmitting two sets of waves in opposite directions. There are two different motions of the parts of the chain it is worth considering separately. There is in the first place the displacement of any link up or down, and in the second place there is the rotation of a link on an axis which is at right angles to this up and down motion. Now, when waves are going up the chain those links are rotating most rapidly which are at any time most displaced: it is the links on the tops and bottoms of waves that are rotating most rapidly. On the other hand, in the case of loops and nodes the links in the middle of loops never rotate at all; they are much displaced up and down, but they keep parallel to their original direction all the time, while it is the links at the nodes where there is no displacement up and down that rotate first in one direction and then back again: there is, in the loops and nodes condition, a separation of the most rotating and the most displaced links which does not occur in the simple wave. There is a corresponding relation between the most rotated and the most rapidly moving links. These are the same links half-way up the simple waves, but in the loops and nodes the most rapidly moving links never rotate at all, while those at the nodes that get most rotated are not displaced at all. These remarks will be seen hereafter to throw light on some of the phenomena observed in connection with Hertz's experiments: hence their importance.

It will be observed that the method of measuring the velocity at which a disturbance is propagated along a string and which depends on measuring the distance between two nodes is really only a modification of the direct method of finding out how long a disturbance takes to go from one place to another; it is one in which we make the waves register upon themselves how long they took, and so does not require us to have at our disposal any method of sending a message from one place to another more quickly than the waves travel, and that is very important when we want to measure the rate at which disturbances travel that go as fast as light. If the wave travels very fast, we must have a very rapid vibration, unless we have a great deal of space at our disposal; for the distance between two nodes is half the distance the wave travels during one vibration, and so will be very long if the wave travels fast, unless the time of a vibration be very short. Hence, if we wish to make experiments in this way, in a moderate-sized room, on a wave that travels very fast, we must have a very rapid vibration to start the waves.

(*To be continued.*)

METEOROLOGY OF BEN NEVIS.[1]

THIS work, just issued by the Royal Society of Edinburgh as vol. xxxiv. of its Transactions, is a quarto volume of 467 pages, giving in detail the hourly observations at the Ben Nevis Observatory from December 1883 to the close of 1887, together with a log containing much that is interesting and novel in meteorological observing; the five daily observations made in connection therewith at Fort William, and a report by Dr. Buchan on the meteorology of Ben Nevis.

This is, it is believed, the only existing combined high and low level Observatories sufficiently near each other in horizontal distance as to be virtually one Observatory: completely placed in one of the greatest highways of storms in the world; and at such a height as to be occasionally above the storms that sweep over that part of Europe, and frequently, as the winds show, within a geographical distribution of pressure at this height widely different from what obtains at the same time at sea-

[1] "Meteorology of Ben Nevis." By Alexander Buchan, LL.D., Secretary of the Scottish Meteorological Society. Transactions of the Royal Society of Edinburgh, vol. xxxiv.

level. Further, the Observatory affords unique facilities for supplying, through its observations, those physical data which are absolutely indispensable in any discussion of the problems involving the relations of height to temperature, humidity, and pressure in the free atmosphere.

Last summer the Low Level Observatory was equipped by the Meteorological Council with a complete set of self-recording instruments, and the regular observing work began on July 14; and it is intimated that the discussion of the hourly observations made at the top and bottom of the mountain in their bearing on many of the more important meteorological inquiries has been commenced. As intimated in NATURE of February 26 (p. 397), a first instalment of this work is completed, showing the influence of high winds on the barometer at the Observatory on the top of the Ben.

The report summarizes the results in diurnal, monthly, and annual means of temperature, humidity, pressure, wind-force, rainfall, cloud, and sunshine. The latter part of the report deals with miscellaneous observations and discussions, which have been carried on, mostly of a novel character, for which Ben Nevis offers exceptional facilities.

The mean annual temperature of Ben Nevis for these four years is 30°·5, the lowest monthly mean being 23°·2 in March, and the highest 40°·9 in July. The mean annual difference between Ben Nevis and Fort William is 15°·9, the least monthly difference 14°·2 in November, and the greatest 17°·8 in May. These results show the rate of decrease of temperature with height to be 1° for every 275 feet of ascent for the year; the rates are most rapid in April and May, and least rapid in November and December, these being 1° for each 247 feet and 307 feet respectively. From a discussion of the daily maxima and minima it is seen that in winter the decrease of temperature with height is nearly as great during the night as during the day, but from April to September the rate of fall during the day is about a half more than that of the night.

But the rates of decrease from day to day differ widely from these mean values and from each other. The greatest difference between the two Observatories was 28°·1 at 2 p.m. of June 8, 1885. The temperature of the top, however, has frequently stood higher than that of Fort William. The greatest deviation occurred at 8 a.m. of November 18, 1885, when, while the temperature of Fort William was 22°·2, that of the top was 35°·1, or 12°·9 higher. These inversions of temperature are significant phenomena, from their obvious and intimate relations with anticyclones, and their connection with neighbouring cyclones; from the extraordinarily dry states of the air which frequently accompany them; and the unusually high barometer at the top when reduced to sea-level, as compared with the sea-level pressure at Fort William.

The repeated occurrence of excessive droughts is one of the most striking features of the climate of Ben Nevis. On July 30, 1885, there occurred a good example of these droughts, when, from 1 a.m. to 4 a.m., the dew-point fell from 44°·8 to 21°·8. But by far the greatest drought was in March 1886, commencing at 1 a.m. of the 11th, and ending at midnight of the 12th; the mean relative humidity of the first 24 hours was 19, and of the second 15, the lowest being 6 at 8 p.m. of the 12th. Such low dew-points and humidities, frequently marked off sharply from high dew-points and humidities, often occur; and, it may be remarked, their occurrence suggests an explanation of the irregular geographical distribution of those disastrous frosts which occur the following night in some districts, although not at all in districts closely adjoining.

The mean annual pressure at Ben Nevis is 25·300 inches, the lowest monthly mean being 25·106 inches in January, and the highest 25·516 inches in June, the difference thus being 0·410 inch. As compared with the sea-level pressure at Fort William, the mean annual difference is 4·562 inches, the greatest monthly difference 4·626 inches in March, and the least 4·483 inches in July, these being, it need scarcely be added, the months of lowest and highest mean temperature respectively. The difference between the mean monthly maximum and minimum is thus 0·143 inch. For these two months the mean temperatures of the stratum of air between the top and bottom of the mountain are 31°·6 and 49°·0, on the assumption that the mean temperature of the intervening air stratum is the mean of the temperature at the top and bottom. Hence the vertical displacement of the mass of the atmosphere for a temperature difference of 17°·4 is represented by a barometric difference of 0·143 inch. Considering the successful arrangements which have been made to minimize the effects of solar and terrestrial radiation at both the upper and lower Observatories, and their close proximity to each other, this result may be regarded as the most important datum hitherto contributed by meteorology for the discussion of inquiries into the relations of height to temperature and pressure in the free atmosphere.

A table of corrections for height for the barometric observations of the Observatory has been prepared, chiefly empirically, for each degree of air temperature and each tenth of an inch of sea-level pressure. With the two sea-level pressures of the upper and lower Observatories thus obtained, various questions may be investigated, such as the effect of high winds on the barometer; the varying relations of pressure and temperature to anticyclones, cyclones, and particularly the regions in front or rear of cyclones; and the relation of pressure to heavy falls of rain.

For the period dealt with, the mean annual amounts of rainfall at the top and bottom are 129·47 inches and 77·33 inches, or about a half more at the Observatory than at Fort William. The observations show that on Ben Nevis, one day in from three to four days has been without rain; and that in one day out of nine, 1 inch of rain, or upwards, has been collected. The time of the year when the great annual fall of temperature takes place is coincident with the time of heaviest rainfall; and the time of least rainfall with the time of the greatest increase of temperature.

The mean minimum temperature of the day occurs from 5 to 6 a.m., and the maximum from 1 to 3 p.m., according to season, the daily range in winter being only 0°·8, and in summer 3°·5. The great daily variations of temperature observed on Ben Nevis are not due to the sun, but they are the temperatures which accompany the cyclones and anticyclones as they successively appear and disappear. The great importance of the hygrometric observations lies in the part they play in the occurrence of daily changes of weather attendant on these cyclones and anticyclones; and in investigating these relations, it is not mean humidities, but the individual observations which are so valuable.

In all seasons, the barometric curves show the double maximum and minimum pressure. Their times of occurrence are, first minimum from 5 to 6 a.m.; first maximum from 11 a.m. to 2 p.m., second minimum 3 to 7 p.m.; and second maximum 8 to 9 p.m., according to season. Like all high level Observatories situated on peaks, the largest of the variations from the daily mean is the morning minimum, and the least, the afternoon minimum, the former being 0·016 inch below the mean, and the latter 0·001 inch above it, on the mean of the year. The relatively large minima in the early morning is due to the cooling of the atmosphere during the nignt, by which the air contracting sinks to a lower level, thus lowering pressure at high levels, and, consequently, this effect is greatest in the summer months when the diurnal range of temperature is at the maximum. The expansion of the air from the increase of temperature during the day raises its mass to a higher level, thus maintaining the barometer at a higher point during the time of the afternoon minimum.

The maximum velocity of the wind occurs during the night, and the minimum during the day, in all months of the year. This peculiarity in the diurnal variation, being just the reverse of what obtains at low levels, is explained by the circumstance that during the night cold currents of air descend the slopes of the mountain on all hands, and the air currents from higher levels which take their place bring with them their higher velocities. On the other hand, during the day, air currents ascend the slopes of the mountain, carrying with them to the summit their velocities diminished from various causes during the ascent. One effect of these ascending currents carrying with them the higher humidities of lower levels, is illustrated by the partition during the day of the sunshine. Thus, in the nine months of spring, summer, and autumn, there are 306 hours of sunshine before noon, but only 285 hours after it.

The concluding portion of the report deals with the miscellaneous discussions and investigations on the formation of snow crystals, winds and rainfall, diurnal variation of wind direction, the thermal wind-rose, rain-band observations, St. Elmo's fire, thunderstorms, mean temperature of each day in year, hygrometry, optical phenomena, and earth currents in the telegraph cable; but as the results of these have from time to time appeared in NATURE, further reference to them is unnecessary.

We would draw special attention to the Observatory log-book, pp. 316-56, which is a remarkably readable, as well as an instructive, document. The following extract presents an interesting side of the Observatory work in this isolated and exposed situation:—

"*January* 26, 1884.—In forenoon wind backed from south to south-east and blew hard. As the drift made it impossible to read thermometer at 1 p.m., Mr. Omond and Mr. Rankin went out tied together, but found it impossible to go farther than the end of the snow porch with safety; at 4 p.m. they got as far as the box, but could not see instruments as the drift blew up in their faces. No temperature observations were taken till 10 p.m., when wind moderated and observations were resumed. There was thus a gap in the temperature observations from noon to 9 p.m. inclusive. Quarter and half hourly barometer observations were taken in afternoon. Lowest recorder reading (corrected to 32°) 23·173 inches."

Optical phenomena, including after-glows, halos, mock suns, mock moons, glories, &c., have been fully observed, and the results are in many respects of the greatest interest. The following graphic account of St. Elmo's fire will be read with interest:—

"*October* 29, 1887.—At 1h. 5m. a.m. St. Elmo's fire was seen in jets 3 to 4 inches long on every point on the top of the tower and on the top of the kitchen chimney. Owing to the number of jets on each cup of anemometer, the instrument was quite ablaze. On the kitchen chimney the jets on the top of the cowl were vertical, and those on the lower edge of same horizontal. The fizzing noise from the different places was very distinct. While standing on office roof watching the display, the observer felt an electric sensation at his temples, and the second assistant observed that his (the observer's) hair was glowing. While standing erect the sensation and glow lasted, but on bending down they always passed off. On raising the snow-axe a little above his head, a jet 2 to 3 inches long shot out at the top. At 1h. 15m. a.m. the fire vanished from every place at the same instant. A shower of snow and snow-hail (conical) was falling at the time, and the wind was variable south to south-west breeze—very flawy."

THE PHOTOGRAPHIC CHART OF THE HEAVENS.

THE Permanent International Committee for the prosecution of the photographic chart of the heavens, has met, deliberated, and adjourned. It will be generally admitted, when the result of their discussions has been digested, that the promise of much good work has been given, and the accomplishment of a photographic chart been brought nearer within the scope of practical astronomy. At a future opportunity we hope to give in some little detail an outline of the arguments that have succeeded in causing deviations from the plan originally contemplated. Now, we can only give a brief account of the report of Admiral Mouchez, and a summary of the more important results which have received the support of the Committee.

Admiral Mouchez's report may be considered as divided into three parts: first, that relating to the installation of the photographic instruments; second, to the methods and apparatus necessary for the measurement of the negatives; and third, the manufacture and employment of *réseaux*. From the first of these sections we learn that all the participating Observatories are provided with satisfactory telescopes; but, unfortunately, the grave political events which have disturbed the Chilian Government are likely to operate adversely on the scientific advance of that country, and the station at Santiago may possibly have to be abandoned, M. Maturana, the Chilian astronomer, having received orders to report himself to his Government for military service. Some delay has been occasioned at Rio de Janeiro by the removal of the Observatory to a distance of six or eight kilometres, where, under more favourable atmospheric conditions, the photographic equatorial is being erected. With these exceptions, the Observatories declare themselves all ready for work.

With regard to the second section, we learn that the construction of the measuring instrument, generously offered by M. Bischoffsheim, has been delayed, owing to difficulties of correspondence between MM. Gill and Kapteyn: but now that these gentlemen have had an opportunity of deciding the particular form the instrument should take, it will be rapidly proceeded with, and it is believed that the employment of such an instrument will greatly abridge the difficulties of measurement and calculation.

The manufacture of the *réseaux* has been definitely abandoned by the authorities at Berlin, but M. Gautier has constructed an instrument for the ruling of these necessary adjuncts, and he is now in a position to supply those Observatories which have failed to obtain *réseaux* from M. Vogel.

Three important points have been before the Committee, and it has been found necessary on these points to modify the decisions of previous Committees. The first of these referred to the desirability of obtaining on the plates of shorter exposure, destined to form a catalogue, impressions of star images with one-fourth of the exposure found necessary to secure the impression of an eleventh magnitude star. The result of this decision would have been to give a picture of the heavens obtained with an exposure of a minute and a half, or a minute, or even less. This project has now been abandoned in favour of one arranged to give impressions, very feeble in character, of stars of the eleventh magnitude, while on the same plate will be exhibited a picture of the heavens obtained with an exposure of twice the length necessary for producing star images in the former case. Thus we may expect on these catalogue plates, exposures of three to four minutes, and also of six to eight minutes.

Another interesting point, over which excitement waxed keen, arose on a proposal of M. Henry to substitute three exposures of equal length, arranged at the angles of an equilateral triangle whose sides are about 5″, instead of one exposure of three times the length. It was asserted that a photograph obtained in this manner, in which each of the exposures was continued for twenty minutes, would exhibit more stars than a photograph with a single exposure of one hour. It was eventually decided that the chart plates taken at the even degrees should have a single exposure, while the exposures of the plates taken at the odd degrees should be left to the discretion of the observers.

The third and last point to which reference may be made here is the length of time to be generally devoted to securing a plate for the chart. It was generally felt that, if the exposures were made long enough to secure the impressions of a fourteenth magnitude star, as generally understood in photometric work, the length of time required would entail too great a strain upon the capacity of the observer. It has therefore been recommended by a sub-committee to confine the exposures to about forty minutes, which, it is believed, will be about eight or ten times the exposure required to impress the image of an eleventh magnitude star on plates as at present employed, and under the ordinary meteorological conditions of the Paris Observatory.

CRYSTALS OF PLATINUM AND PALLADIUM.

I HAVE found that crystals of the metals platinum and palladium are easily prepared as follows :—

A ribbon of the pure metal is stretched horizontally between two binding screws. Upon the ribbon, some topaz, reduced to a fine powder, is dusted. A current is now passed through the ribbon, of a strength sufficient to raise it to a bright red heat. In about half an hour's time, if the current be stopped and the ribbon examined with a microscope, it will be found that very small brilliant crystals cling here and there to projecting points on the partially decomposed topaz. If the heat be maintained, these crystals steadily grow ; in about two hours some will have attained to a size of about 0·1 mm.

Platinum was the first substance which I crystallized in this manner. When the topaz was scraped off the ribbon, the under surface of the caked fragments was found covered with a grey deposit of the small crystals. Under the microscope, using a 1-inch objective, these present a very brilliant appearance. They are opaque, and show a high metallic lustre, like that of clean mercury, but are somewhat whiter in colour. The faces are clean, and sharply defined. Their crystallographic system is very evidently cubic. The prevailing form is the octahedron, or some modification of it. When the octahedron is perfect, the triangular faces are equilateral. Strings of octahedra occur, after the manner of magnetite. A common modification of the octahedron is that well known in the case of alum, in which two opposite faces of the solid are dominant, giving a tabular form. Very thin hexagonal tables also occur, suggesting, at first sight, dimorphism. These, in the opinion of Prof. Sollas, might be referred to an extreme development of the alum habit. Many observations support this view, which seems preferable to supposing dimorphism.

Hemihedral forms occur, principally the tetrahedron and modifications of it. The combination of tetrahedron and cube, the tetrahedron being dominant, is observed. Acicular forms are also present, but are scarce. The following observations bear upon the nature of these crystals.

Treated in hot hydrochloric, sulphuric, or nitric acid, they are unattacked. They also appear unaffected in cold hydrofluoric acid. In hot hydrofluoric acid they seem slightly attacked. They are slowly but completely dissolved in boiling aqua regia, from which a precipitate of ammonium platinic-chloride is obtained on the addition of ammonium chloride. They are not attracted by the electro-magnet, and conduct electricity (shown by touching the opposite extremities of a crystal with fine needles in circuit with a battery and galvanometer). When crushed between two glass slips they spread out, giving the glass the appearance of a mirror. Although in this way much extended, they show no appearance of cracking upon the edge. They are therefore very malleable. Their melting-point approximates to that of platinum, for a ribbon with some of these crystals clinging to it may be raised to its melting-point, when some of the crystals will be found partially fused. The ribbon upon which crystals have been formed presents a roughened appearance.

The bodies present at their formation are platinum, fluorine, silica, alumina, and a trace of iron. From the physical characters observed, however, the crystals are certainly metallic, and in fact can only be platinum, probably in a very pure state. It would appear that fluorine, liberated at a high temperature from the topaz, attacks the platinum, forming a fluoride, which again breaks up, depositing the crystals.

M. Moissan has already observed that the tetrafluoride of platinum when reduced leaves distinct crystals of the metal (NATURE, vol. xli. p. 118). M. Moissan obtained the fluoride by the direct action of dry fluorine on platinum at a low red heat, and mentions that this body splits up again at a higher temperature. The position of the greater part of the crystals in the present experiment, close to the surface of the platinum, is in agreement with the instability of the fluoride at a high temperature. It seems necessary to suppose the formation of the fluoride to occur further away from the ribbon where the temperature is suitable. So that it appears that a considerable volatilization of the platinum takes place. It is remarkable that, if the conditions are such that the temperature throughout the mass of the mineral is uniform, the crystals of platinum are not formed. Thus, if powdered topaz is rolled up tightly in a tube of platinum, and this tube heated for a considerable time by the passage of a strong current, the mineral is decomposed, there is considerable loss of weight, but no platinum crystals are formed. On the other hand, acicular crystals, which are colourless and transparent, are found under these conditions plentifully intermixed through the partially fused *débris*.

It appeared probable that some related metals might afford crystals in the same manner. Palladium and iridium suggested themselves. The latter I have not yet been successful with, owing to the difficulty of obtaining a piece of suitable dimensions. Palladium ribbon was easily obtained, Messrs. Johnson, Matthey, and Co. supplying this and the other pure metals.

The ribbon of palladium was treated in the same manner as the platinum. Crystals were easily formed.

These are very similar in colour and lustre to the crystals of platinum. In form they appear isomorphous ; I have not been able to detect any difference, so that the foregoing remarks on the form of the platinum crystals apply to these also. It seems reasonable to suppose that a tetrafluoride of palladium is concerned in their formation.

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DR. NANSEN ON GLACIATION.

IT may be worth while to call attention to one or two points of scientific interest in connection with the results obtained by the heroic band of Norwegians, whom Dr. Nansen led across Greenland, as set forth by the chief of the expedition in the appendix to the "First Crossing of Greenland" (Longmans, 1890).

(1) The importance of the internal heat of the earth and its conductive transmission to the contact-plane, as a factor in the mechanics of glaciers, for which I contended eight years ago, may be said to be now established. The

views of Tyndall and Helmholtz as to the important part played by liquefaction of the ice under pressure and regelation, with all that follows from this law, have also received important corroboration.

(2) All that Dr. Nansen has recorded of the snow-covered "inland-ice," goes to show how important a factor a loose covering of snow is in dispersing the solar rays, and so preventing the transmission of a large part of the *luminous* energy into the ice, where, on the "greenhouse principle" (see Nature, vol. xxvii. p. 554) it would otherwise be converted into dark heat by absorption, and so facilitate the "flow" of the ice.

(3) The interpretation of the observed facts of the Waigatz Sound (about 71° N. lat.), quoted by Dr. Nansen from Prof. Helland, must be received with some caution. It is based largely on the assumption that the basalt capping of the district referred to was once a *continuous sheet*, whereas it is just as likely that the portions of the basalt cap now observed may be only originally separate tongues of the lava-flow, and that the soft Tertiary strata have been worn out by the action of frost and running water as an ordinary "fjord." Erosive action of glacier-ice seems here unnecessarily invoked. Even supposing these strata to be of an age so late as the Miocene, there is a wide berth in *time* for the action of running water, aided and supplemented by the disintegrating action of frost, which Dr. Nansen's own record shows, in the present condition of things, to be (in the interior) often some degrees below the freezing-point of mercury in such latitudes. Then, again, there is the important consideration, which Dr. Nansen touches upon but slightly, that the Miocene period of Greenland may not have synchronized with that period in more southern latitudes. The *relative* value of the different periods of the Tertiary age, as affected by latitude, is a matter, which, so far as I know, has not received from geologists anything like that consideration to which it is entitled, though the reason may not be far to seek.

(4) The extension of the lower parts of the ice-sheet into the valleys to form glaciers has, I think, been over-estimated as an agent of *erosion*, insufficient account having been taken of the expenditure of mechanical force in the shearing of the ice. This argument has been more fully worked out by myself in my paper on the "Mechanics of Glaciers" (*Q.J.G.S.*, vol. xxxix. pp. 64, 65).

(5) The summary manner in which the formation of "*åsar*" is dealt with (*op. cit.*, p. 494) seems to be based upon the assumption that they were formed beneath the ice-sheet; but this can scarcely be said to be established. Against the results arrived at by Dr. N. O. Holst, of Stockholm, from an extended investigation of these phenomena in Sweden, the non-observance of surface-rivers on the Greenland ice-sheet, rather late in the season of the year, by Nansen's party, can scarcely be said to militate very strongly, seeing that such rivers were observed by Nordenskiöld. The *åsar* are best known in formerly glaciated regions of more southern latitudes, successive zones of which would constitute the *marginal* areas of the ancient ice-sheet during its gradual diminution and the retreat of the ice from lower to higher altitudes; while a different configuration of the country would furnish the necessary moraine material, which would appear, from Dr. Nansen's observations, to be almost wanting in Greenland, though Dr. Holst saw a good deal in the summer of 1880 (see the *American Naturalist* for August 1888, p. 707).

A. Irving.

NOTES.

Last week men of science and the general public were sincerely sorry to receive bad reports as to the state of Prof. Tyndall's health. On Wednesday there was a severe crisis in his illness, but afterwards hopeful symptoms manifested themselves, and these, we are glad to say, have since been maintained.

The vacancy in the position of Director of the Royal Meteorological Institute of the Netherlands, caused by the death of Prof. Buys-Ballot last year, has been filled up by the appointment of Dr. Maurice Snellen, who has for many years been connected with the Institute. Dr. Snellen represented Holland at the Meteorological Congress of Rome in 1879. He was the chief of the Dutch Expedition to Dickson Harbour in 1882, to take part in Weyprecht's scheme. It will be remembered that the Expedition was caught in the ice, and eventually had to abandon the ship and return without ever reaching its destination.

The forthcoming International Ornithological Congress at Budapest is expected to be a great success. It will be held from May 17 to 20. There will be various interesting exhibitions, and the following lectures will be delivered:—On the life of West African birds, by Alexander Homeyer, of Greifswald; on the results of observations upon the passage of birds, by Otto Herman; on the life of birds in Arctic Norway, by Robert Collett, of Christiania; and on the life of Alpine birds, by Victor zu Tschudi Schmidthoffen. After the close of the proceedings expeditions will be made to various parts of Hungary.

The Medical Faculty of Queen's College, Birmingham, with the contents of the museums and other property belonging to it, is to be transferred to Mason's College, where for some time a part of its work has been carried on. This has been formally decided by the Council of Queen's College, whose action has been ratified by the governors of the institution. The decision is an important one, and may prove to be the first step towards the establishment of a University for the Midlands. During the last few years there has been a great increase of medical students, and this has rendered the present accommodation much too small, whilst the Queen's College authorities have no funds available which would permit them to carry out the necessary reconstruction. Moreover, it is felt that the systematic part of medical education should be carried out under the control of one Board, and not as at present under the direction of two independent Councils. New buildings, adjoining those of the Mason College, and connected with them, but having also an independent entrance, will be erected for the School of Medicine, and in the plans due provision will be made for the largely-increased number of students whom it is expected the more perfect equipment and enlarged facilities of the new school will attract.

The Council of the Society of Arts, acting under the provisions of the Benjamin Shaw Trust, have offered two gold medals, or two prizes of £20 each, to the executive committee of the Congress of Hygiene and Demography, for any inventions or discoveries of date subsequent to 1885, exhibited at or submitted to the Congress, and coming within the terms of the Trust. Under the conditions laid down by the donor, these prizes are to be offered for new methods of obviating or diminishing risks incidental to industrial occupations.

Steps are being taken in Paris to prepare the way for the holding of an International Colonial Exhibition next year on the Champ de Mars. According to the Paris correspondent of the *Times*, the sections would be geographical, not political, all the West Indies, for instance, forming one section, all India another, and so on. Specimens of all the native populations would be brought over and housed as at their homes, and two Congresses—a Colonial and an Ethnographical—would be held.

An International Agricultural Congress will be held at the Hague in September next, from the 7th to the 12th. A Commission will be appointed at the Hague to arrange for the reception of the members.

WE regret to have to record the death of Mr. Tuffen West, on March 19, at his residence, Furnell House, Frensham, in his sixty-eighth year, after a very painful illness. Having lived in great seclusion for many years, owing to infirmities of various kinds, Mr. West was but little known to the present generation of naturalists. Twenty or thirty years ago, a descriptive work on natural history was scarcely considered complete unless illustrated by him; and scientific magazines and the publications of the learned Societies bear testimony alike to the marvellous accuracy of his pencil and to the artistic feeling which characterized all his productions.

THE law transferring the U.S. National Weather Service from the War Department to the Agricultural Department will take effect on July 1. The *New York Tribune*, commenting on this change, says that Congress, in directing it, was probably "governed by the obvious fact that meteorological work is essentially civilian, and not military, in character." The *Tribune* urges that great care should be taken in the selection of a new chief. "Congress," it says, "has properly provided that the daring and skilful commander of the Fort Conger Arctic Expedition of 1881-84 shall, after the Signal Corps is relieved of its meteorological duties, retain his present military rank and emolument. No element of personal reproach or hostility has entered into the movement for the transfer, so far as we know. The change thus involves no indignity to that gallant and accomplished officer and popular hero. This is a fortunate phase of the matter. And since several of General Greely's most valued assistants either gain promotion in the reorganized branch of military service in which they intend to remain, or else go with the observers and clerks into the new civil bureau, the President will experience less embarrassment than would otherwise be the case in selecting the new Superintendent."

MR. C. S. MIDDLEMISS, of the Geological Survey of India, now in Hazara, has been appointed geologist with the Black Mountain Expedition.

THE *Pioneer* of Allahabad expresses a hope that the opportunity offered by the expedition into the Black Mountain will be utilized for exploring the unknown country lying to the north. Beyond Thakot the very course of the Indus is not certain, and exploration in this direction will be full of interest. It is quite certain that there is no easy passage into Cashmere from the west, or southwards from Banji towards Thakot, but British surveyors must feel it a reproach that the maps should be blank for even 150 miles. The valleys are said to be sparsely populated, and the hills rugged in the extreme; but a party with an energetic leader and a sufficient escort should find no difficulty in making their way up the Indus Valley. There will not on this occasion be any necessity for evacuating the Black Mountain district in a hurried manner, and it would have a good effect along the border if the country northwards to Gilgit were explored.

THE South London Entomological and Natural History Society will hold its annual exhibition at the Bridge House Hotel on April 15 and 16. According to the *Entomologist*, there is little doubt that this Exhibition will be "quite equal to, if it does not surpass, any of the Society's previous achievements."

WE have to record the publication of a new Italian monthly scientific journal, *Neptunia*, which takes the place of *Notarisia*. It is devoted to "the study of science pure and applied to the sea and to marine organisms." It is edited by Dr. D. Levi-Morrenos, and three numbers have already appeared. Fresh-water as well as marine Algæ are also taken under its cognizance; and, in the numbers already published, appear papers on Conjugation in the Zygnemaceæ, and on the Cystocarps and antherids of *Catenella Opuntia*, by our countrymen Mr. W West and Prof. Harvey-Gibson.

THE editor of *Neptunia* announces a project for the establishment of a marine station at Sebastopol. The plan followed in the erection will be that of the Zoological Station at Naples, but on a smaller scale. The building will consist of three stories, of which one will be devoted to the aquarium, another to the laboratory, and the third to the library, the museum, and the private rooms of the Director.

THE Shanghai papers report that an extensive collection of rare and beautiful butterflies, collected by the late Captain Yankowsky, the commander of a merchant steamer on the China coast, was recently sold by public auction. There was a good attendance and some spirited bidding. But the merchant princes and millionaires of the settlement were conspicuous by their absence, and the labour of a life-time was scattered and sold piecemeal. Strange to say, some of the cases containing butterflies which are not very rare went higher than those from the most distant parts of China, from Omiesan, Wusan, and Tachin-lu in the west of Szechuen. The chief purchasers were Mr. Carl Bock, who was present on behalf of the Shanghai Museum, and Inspector Howard, who bought on his own account. The entire collection fetched only Tls. 262 (about £50).

PERHAPS the most important addition that has been made to the interesting collection of animals in the Calcutta Zoological Gardens since its establishment fifteen years ago is the Greater Bird of Paradise (*Paradisea apoda*, Linn.). The committee of management had long been desirous of acquiring a specimen, but owing to the great rarity of the species, and the very great cost of those that are on rare occasions offered for sale, their efforts to procure one had hitherto proved unsuccessful. At last, however, they have succeeded, thanks to the liberality of the Maharajah Bahadur of Dumraon, who is a great patron of the institution.

MR. LITTLEDALE, of Baroda, has made a proposal that the Bombay Natural History Society should carry out certain interesting experiments with a view of ascertaining whether several of the game birds which are common in other parts of the country could be successfully introduced into the neighbourhood of Bombay, such as the chukor, black partridge, and Bengal florican. A special meeting of the Society was convened for the consideration of this proposal, which also includes the possible introduction of several of the African antelopes, and it was unanimously agreed to. Experiments of this character are of the greatest interest to naturalists as well as sportsmen.

A CLIMATOLOGICAL table issued by the German Meteorological Society shows that the mean temperature for the year 1890 ranged from 44°·1 F. at Königsberg to 50°·2 at Aix-la-Chapelle and Cologne, and 50°·4 at Strassburg, the mean for the Empire, deduced from the observations at twenty-five selected stations, being 47°·9. In January the mean ranged from 26°·2 at Königsberg to 35°·8 at Aix-la-Chapelle, the general mean being 31°·1. July had from 63°·1 at Hamburg to 67°·1 at Frankfort-on-the-Maine and Freiburg, the mean for the whole being 65°·2. The rainfall varies between 19·69 inches at Posen to 50 inches at Freiburg, but only four stations have more than 30 inches, and only eleven receive more than 25 inches, the mean amount for the Empire being 26·58 inches. For the sake of comparison we give the following results calculated from the publications of the Meteorological Office. The mean tempera- of the British Isles for the year 1890 was 48°·0, ranging from 45°·7 in the north of Scotland to 51° in the Channel Islands.

January had a mean of 42°·6 (38°·3 at Hawes Junction and 38°·4 at Braemar, to from 46° to 48°·3 at several stations in the south-west of England), and July 56°·9 (from 51°·5 at Sumburgh Head and Glencarron to 60°·9 at Southampton). The two months it will be remembered were exceptional in these islands, the former being unusually mild, and the latter exceptionally cold. Although, therefore, there was practically no difference between the mean temperature of the two countries for the whole year, Germany was 11°·5 colder in January and 8°·3 warmer in July. The rainfall of the United Kingdom was 33·4 inches, the eastern districts of England having from 22 inches to 25·4 inches, and elsewhere from 32·9 inches in the Channel Islands to 52·8 inches in the west of Scotland, and 55·1 inches in the north of Scotland. London was rather more than a degree warmer than Berlin for the year, and nearly 13° warmer in January, the means being, London, January 43°·7, July 60°·4, year 49°·5; Berlin, respectively 31°·1, 66°·2, and 48°·4. The former had 22·78 inches of rain, the latter 23·63 inches.

At the meeting of the Linnean Society of New South Wales on February 25, Mr. R. Etheridge, Jun., in continuation of former notes, read a paper on Australian aboriginal stone weapons and implements. Among the objects he exhibited and described were stone knives from Northern Australia; small and beautifully-formed spear-heads from Kimberley; larger lanceolate spear-heads from Nicholson River and Settlement Creek, North-West Carpentaria; and talismanic stones from New England and North Queensland, the latter a very interesting tael formed of two rock crystals joined by a gum-cement mixed with human hair.

A Royal Commission on Vegetable Products has just been at work in Victoria, and the result has been to bring together a valuable array of facts regarding what is called the "scent farming" industry in that colony. Mr. Shillinglaw, the secretary to the Commission, has just published a *précis* of the proceedings, which is extremely interesting, as conveying some idea of the extent to which the cultivation of perfume plants has been carried in Victoria. The Commissioners during the course of their inquiries, investigated the Dunolly Perfume Farm, and also drew largely from the experiences of Mr. Joseph Bosisto, the originator of the scent farming industry in the Wimmera district. The general result of the inquiry seems to be that the soil and climate of Victoria are particularly well suited for the cultivation of scent-producing plants.

Messrs. B. Westermann and Co., New York, have sent us the first part of what promises to be an important work on "The Fishes of North America that are caught on Hook and Line." The author is Mr. W. C. Harris. The coloured illustrations are executed most carefully.

A "Fauna of Normandy," by M. Henri Gadeau de Kerville, has been in course of publication in Paris since 1888. Two volumes have been issued—"Mammalia" and "Birds." There are no engravings, and the work consists essentially of an enumeration of the animals observed in Normandy, with bibliographical references, and some notes on modes of life, instincts, and general biology.

The fourth volume of the *Internationales Archiv für Ethnographie* opens with a double number of great interest. Dr. Masanao Koike gives an excellent account of the observations made by him during a residence of two years in Corea. This paper, written originally in Japanese, has been translated into German by Dr. Rintaro Mori. A description of the Corean collections in the ethnographical museum of Leyden is contributed by Dr. J. D. E. Schmeltz; and Dr. A. Baessler makes some valuable additions to our knowledge of the ethnography of the East Indian Archipelago. Prof. H. H. Giglioli has an interesting paper on two ancient Peruvian masks made with the facial portion of human skulls. The number, as usual, is admirably illustrated.

Mr. Philip B. Mason has an interesting little note in the new number of the *Zoologist* on a question which has lately been discussed in that periodical—whether squirrels remain active during the coldest weather. Mr. Mason is able to state that during the whole of the recent severe and prolonged frost several squirrels which had been accustomed to climb to the nursery window of Drakelowe Hall, Burton-on-Trent, where they were fed, continued their visits during the whole of the time, and seemed to be as lively as usual.

The first number of a new quarterly magazine for conchologists has been issued. It is called the *Conchologist*, and promises to be of much service to the class of students to whom it is specially addressed. If adequate support is received, the magazine will be issued as a bi-monthly in 1892.

We note the appearance of a new trade journal, the *Optician*. It is intended to act as the organ of the optical, mathematical, philosophical, electrical, and photographic instrument industries, and also to present a review of the jewellery and allied trades.

The Royal University of Ireland has issued its Calendar for the year 1891. We have already noted that the papers set at the examinations in 1890 are published in a separate volume, forming a supplement to this Calendar.

Messrs. Dulau and Co. have issued a catalogue of the botanical works which they now offer for sale.

At the last monthly meeting of the Manchester Geological Society Mr. Tonge read an interesting paper entitled "Coal-Mining in 1850 and 1890: a Few Contrasts." He said that during the forty years from 1850 to 1890 progress had not been more marked in any direction than in the industry of coal-mining. Not only had new fields been discovered which previously were not even suspected, not only had greater depths been reached, but far greater quantities of coal had been raised; and, what was more important, the collier's life had been made more secure and his work more pleasant, whilst his health, mental powers, and moral character had been more carefully studied and protected. In 1851 there were rather more than 50,000,000 tons of coal and other minerals raised; in 1889, 185,187,266 tons. In 1851 there were 216,217 persons employed above and below ground in coal-mining; in 1889, 563,735. In 1851 there were 984 deaths caused by accidents in and about mines, being at the rate of 4·56 persons per 1000 employed; in 1889 there were 1069 deaths, being at the rate of 1·88 per 1000 employed. In 1851 there was one death from accident for every 219 persons employed; in 1889, one for every 530, the degree of safety being two and a half times greater in 1889 than in 1851. He attributed the improved condition of things largely to beneficent legislation, to scientific and mechanical discoveries, bringing improvements in machinery, ventilation, and lighting, and to greater care in the use of explosives. The collier's labour had been considerably lightened. He had not such long distances to waggon his coal; he had not to go to the surface, as in former times, to look for props, and perhaps saw them for himself. The travelling roads and working places were in much better condition; and the system of working, being mostly long-wall, enabled him to get his coal with much greater ease, and to get a greater quantity than by the pillar and stall system which in 1851 was so much practised.

At the meeting of the Société Chimique of February 27, M. Hanriot communicated the fact that he had repeated the work of Messrs. Mond, Langer, and Quincke, an account of which was given in NATURE, vol. xlii. p. 370, upon the remarkable compound of nickel and carbon monoxide, $Ni(CO)_4$. He finds that it is a most highly poisonous substance, far more deadly than carbon monoxide itself. Blood poisoned by means of it exhibits the characteristic absorption-spectrum of blood containing carbon monoxide. The oxygen of the air diminishes somewhat the poisonous action of the compound, owing to the fact that it rapidly promotes dissociation into metallic nickel and carbon monoxide.

THE mineral hornblende has been artificially reproduced in well-formed crystals by M. Kroustchoff, and an account of his experiments is communicated to the current number of the *Comptes rendus*. The last few years have been most fruitful in mineral syntheses, so much so indeed that there remain very few of the more commonly occurring rock-forming minerals which have not been artificially prepared in the laboratory. M. Kroustchoff, who not long ago described a mode of preparing most perfect crystals of quartz, has made many attempts to reproduce hornblende, and has at length succeeded by the adoption of the following somewhat remarkable process. This process essentially consists in digesting together for a long period of time, *in vacuo*, and at a high temperature, the various oxides contained in natural hornblendic amphiboles, in presence of water. Small flasks of green glass were employed, each of which was exhausted by means of a Sprengel pump after the introduction of the substances to be digested together. The ingredients digested consisted of (1) a dialyzed three per cent. aqueous solution of silica; (2) an aqueous solution of alumina obtained by dissolving aluminium hydrate in an aqueous solution of aluminium chloride and subjecting the solution to dialysis; (3) an aqueous solution of ferric oxide obtained by the addition of ammonium carbonate to ferric chloride in such quantity as to redissolve the precipitate first formed, and dialyzing the solution; (4) carefully prepared pure ferrous hydrate; (5) lime water; (6) freshly precipitated hydrate of magnesia; and (7) a few drops of caustic soda and potash. The mixture presented the appearance of a gelatinous mud. The exhausted and sealed flasks were placed in a specially constructed iron many-chambered furnace, and heated for three months to a temperature of 550° C. At the expiration of this time the appearance of the contents had entirely changed, having become much darker in colour, and distributed throughout were numerous brilliant little crystals, almost black in colour, and reminding one forcibly of natural hornblende. On systematic examination they were found to consist of flattened prisms identical in character with hornblende. Under the microscope they exhibited the hornblendic yellowish-green colour and pleochroism. Their index of refraction was the same as that of natural hornblende, about 1·65. The angle between their optic axes was found to be 82°; that of natural crystals varies from 80° to 85°. Analyses gave the characteristic amphibolic percentages, that of SiO_2 being 42·3. In addition to these crystals of hornblende it is interesting to note that pyroxenic crystals resembling those of the augite family were also found in the flasks, together with crystals of a zeolite and of a variety of orthoclase felspar. And finally, some exquisite little quartz crystals were observed, showing cavities containing liquids and bubbles resembling those of natural rock crystals.

THE additions to the Zoological Society's Gardens during the past week include an Arctic Fox (*Canis lagopus*) from Iceland, presented by Mr. H. Sacheverell Bateman; a Squirrel-like Phalanger (*Belideus sciureus* ♂) from North Australia, presented by Mrs. FitzGerald; a Lacertine Snake (*Cœlopeltis lacertina*), South European, presented by Mr. J. C. Warburg; a Rhesus Monkey (*Macacus rhesus* ♂) from India; a Common Wolf (*Canis lupus* ♀), European, deposited; two Violaceous Plantain Cutters (*Musophaga violacea*) from West Africa, four Cape Colys (*Colius capensis*) from South Africa, a Carpet Snake (*Morelia variegata*) from Australia, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), a Vulpine Phalanger (*Phalangista vulpina* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

STARS HAVING PECULIAR SPECTRA.—*Astronomische Nachrichten*, No. 3025, contains a note entitled "A Fifth Type of Stellar Spectra," by Prof. E. C. Pickering, and one by Mrs. Fleming, in which she describes the following objects of interest, discovered during an examination of the photographs of stellar spectra taken at Harvard College Observatory with the 8-inch Draper telescope:—

Designation.	R.A. 1900.	Decl. 1900.	Mag.	Description of spectrum
	h. m.	° ′		
D.M. + 63°·83 ...	0 37·5	+64 14	9·5	Bright lines.
Cord. G.C. 23416	17 12·1	−45 32	7·2	" "
[Aquarius]	20 41·2	− 4 26	Var.	III. Type; G and *h* bright.
[Delphinus]........	20 43·1	+18 58	Var.	" " "

The similarity of the spectra of the two stars in Aquarius and in Delphinus to that commonly shown by variables of long period led Mrs. Fleming to suspect the variability, which was confirmed by an examination of photographs taken on various dates. With respect to this point, Prof. Pickering remarks, in the note to which reference has already been made:—"Probably most of the variable stars of long period give a spectrum resembling that of *o* Ceti, and having the hydrogen lines G, *h*, α, β, γ, and δ bright about the time of maximum. When the photographic spectrum is faint, only the brighter lines G and *h* are visible. Photographs have been obtained of forty-one of these objects, ten of which have been discovered by means of this peculiarity of their spectrum."

Following the classification suggested by Prof. Lockyer in the Bakerian lecture for 1888, spectra of Secchi's third type are divided into sub-groups, of which α Orionis, α Herculis, S.D. − 2°·3653, and *o* Ceti, are given as examples.

The fifth type of stellar spectra which Prof. Pickering proposes to establish will contain planetary nebulæ and bright-line stars, and is therefore equivalent to Lockyer's Group I. Photographs of the spectra of sixteen planetary nebulæ taken at Harvard resemble each other closely, and consist mainly of bright lines and bands. The bright-line stars are divided into three classes, in the first and second of which λ 469 is the most conspicuous line, whilst the third class is distinguished by the distinctness of a line at λ 464. A comparison shows that the position of the lines of these stars and planetary nebulæ is the same as in Orion stars, but the lines are dark instead of bright in the last-named bodies. Only a three-figure reference is now given to the positions of the lines, and more precise measures of wave-length will be required to show whether the slight differences which exist are real or not. The irregular distribution of these objects in the heavens is another point of resemblance. Four-fifths of the stars of the Orion type are found in the Milky Way. A similar distribution of the planetary nebulæ has long been recognized. By arranging thirty-three bright-line stars according to their distances from the pole of the Milky Way (R.A. 12h. 40m., Decl. + 28°), and its ascending node (R.A. 18h. 40m., Decl. 0°), Prof. Pickering shows that their distribution is also strikingly irregular. The average value of the galactic latitude of all the stars is 2° 6′. Groups occur in Argo, Scorpio, and Cygnus. The approximate longitudes are 257°, 313°, and 42°. The number of stars in these groups is 7, 5, and 8, or 20 in all. Each group is included in a circle 8° in diameter, and of all the members only three are isolated, being distant 10° or more from any of the others. It therefore follows that one half of the known bright-line stars are included in an area of about one three-hundredth of the entire sky. It is gratifying to find the suggestion, that stars with bright lines are physically similar to planetary nebulæ, supported by the numerous observations made at Harvard College Observatory. The anomalous character of the spectra of the Orion stars has long been a matter of discussion. Perhaps the agreement as to position of the dark lines in these stars with the bright lines in others will enable their place in stellar evolution to be determined.

The Nebula near Merope.—In *Astronomische Nachrichten*, No. 3024, Prof. Pritchard states that the nebula close south and following Merope, which Prof. Barnard observed on November 14, and announced as a discovery in *Astr. Nach.*, No. 3018, is plainly impressed on photographic plates taken at Oxford Observatory, with exposures varying from 20 to 120 minutes. Prof. Barnard believed that the exposure which would be necessary to produce an image of the nebula would so extend the diameter of the image of Merope that the two objects would coalesce. Prof. Pritchard, however, finds that stars of the 14th magnitude are perfectly distinct on plates taken with the above-mentioned exposures, and that the stellar disks of Merope in such cases vary from 20″ to 30′ in diameter. Since the supposed new nebula is some 40″ distant from Merope, the separation of the two objects is always plainly marked.

Comet *a* 1891.—The comet of which Mr. Denning announced the discovery in the last issue of Nature (p. 516) is probably identical with one independently discovered by Prof. Barnard, of Lick Observatory, on Tuesday, March 31.

BIOLOGICAL NOTES.

The Eyes in Blind Crayfishes.—The minute structure of the eyes of two species of blind crayfish have been very thoroughly investigated by G. H. Parker, under the direction of Prof. Mark. Mr. Parker is the Instructor in Zoology at the Harvard College Museum. The species were *Cambarus setosus*, a new species recently described by Dr. Walter Faxon, from caves in South-western Missouri, and *C. pellucidus*, Tellk., the well-known species from Mammoth Cave. After a short *résumé* of the investigations of Newport and Leydig, the author states that the principal questions concerning the eyes of blind crayfishes turn on their amount of degeneration; not only has the finer structure of the retina been affected, but the shape of the optic stalks has been altered. The optic stalks are not only proportionally smaller than those of crayfishes possessing functional eyes, but they have in these two cases characteristically different shapes. In crayfishes with fully developed eyes the stalk is terminated distally by a hemispherical enlargement; in the blind crayfishes it ends as a blunt cone. In both forms of crayfishes the optic ganglion and nerve were present, the latter terminating in some way undiscoverable in the hypodermis of the retinal region. In *C. setosus* this region is represented only by undifferentiated hypodermis, composed of somewhat crowded cells, while in *C. pellucidus* it has the form of a lenticular thickening of the hypodermis, in which there exists multinuclear granulated bodies; these are shown to be degenerated clusters of cone-cells. (*Bulletin of the Museum of Comparative Anatomy at Harvard College*, vol. xx. No. 5, November 1890.)

A New Stalked Crinoid.—The United States Acting Fish Commissioner was engaged during 1887 in dredging operations between Panama and the Galapagos Islands. One haul, taken from a depth of 392 fathoms, off Indefatigable Island, one of the Galapagos group, brought up three imperfect specimens of a most interesting stalked Crinoid. At a first glance, it might readily pass for a living representative of the fossil Apiocrinus; but on a closer scrutiny it showed some features which ally it with Millericrinus, and others with Hyocrinus and Rhizocrinus. Prof. Alexander Agassiz, who hopes very soon to publish a detailed account of this form, with figures, thinks that it shows structural features of all the above mentioned genera. It has, like Hyocrinus and Rhizocrinus, only five arms; they are, however, not simple, but send off, from the main stem of each arm, three branches to one side and two to the other. The system of interradial plates is highly developed, as in Apiocrinus and Millericrinus, six rows of solid polygonal imperforate plates being closely joined together, and uniting the arms into a stiff calyx as far as the sixth or seventh radial and to the third or fourth joints of the first and second pinnules. The interradial calycinal plates extend along the arms for a considerable distance beyond the first branch. The stem tapers very gradually, and in its general appearance recalled that of Apiocrinus; it expanded towards the base, but the actual attachment was not found. The stem must have been about from 26 to 27 inches in height; the height of the calyx to the interradials is 7/16 of an inch; its diameter at the inner base of the second radials is 11/16 of an inch, and at the height of the third joint of the second pinnule 1 inch. The arms were about 8 inches in length. It is named *Calamocrinus diomedæ*, after the steamer *Albatross*.

British Marine Algæ.—Almost forty years have passed since the issue of the last part of Harvey's "Phycologia Britannica." The publication of this work extended over five years, and it enumerates 388 species. In the preface to the first volume the author not obscurely hints at the existence of other forms, "some few but distinct looking, preserved in my own and other herbaria," remaining over for further research; but the time for this never came. During the years that have elapsed since 1851, though the labourers in this field of botany do not appear to have greatly increased, the methods of modern research have added to our exact knowledge of the subject, and the investigations of Agardh, Bornet, Flahault, Gomont, Grunow, Schmitz, Reinke, and others, have thrown a flood of light on the minute structure, fructification, and classification of the species of this group. Although the time has not yet, in our opinion, come for a new edition of Harvey's work, yet it is with extreme satisfaction that we notice the publication of "a revised list" of our British marine Algæ, which is the result of the joint labours of Messrs. E. M. Holmes and E. A. L. Batters. None but those who have engaged in such work can appreciate the immense amount of labour and care which is needed to bring such a list even fairly up to date, and when, as in the present instance, most of the additions to the known native species have been made by the individual efforts in collecting of the authors, it must add to the appreciation with which this revised list will be received by botanists. As a matter of necessity, for the present, any classification must be regarded as only provisional, but the authors have been enabled to give the very best one possible, and one quite in accordance with recent investigations. Exclusive of all varieties, the number of species recorded in this revised list is 536, after excluding some six forms, like *Sargassum bacciferum* and *S. vulgare*, which have only been met with as waifs. In the classification of the Cyanophyceæ, the authors have as to the Nostochinæ followed, with few exceptions, the guiding of Bornet, Flahault, and Gomont. In the Chlorophyceæ, while the researches of Agardh and Wille have not been overlooked, yet it can scarcely be doubted but that much new work is needed ere the numerous species of Chætomorpha and Cladophora can be satisfactorily described or arranged. Reinke has been in part followed in the arrangement of the Phæophyceæ, but in some orders of this group a good deal of additional information is needed ere a generally acceptable determination of even the limits of its genera is obtained. In the Rhodophyceæ the classification of Schmitz has been generally adopted, but we still wait expectingly for the final views of Schmitz on the group. In the working out of the details a great deal of trouble has been taken in the determination of the often very numerous varieties, and this will be an important help to the worker. An attempt has been made to give a general idea of the geographical distribution by dividing the coast-line of Great Britain and Ireland into fourteen sections, and then indicating in which of these the species or its varieties are or have been found. In an appendix a list of species known to occur either on the Atlantic shores of France or of Norway, or in the Baltic, and which one might expect to meet with on our own shores, is given, and will prove useful. Possibly Mediterranean forms may yet be met with, other than waifs, on the south-west shore of Ireland, a part of the coast-line hardly as yet investigated. Miss Hutchins collected in portions of Bantry Bay, and Miss Ball in the Youghal district, but the intervening coast has only as yet been casually visited. It is to be hoped and expected that one result of the publication of this most useful list will be to stir up enthusiasm in the collecting and study of our native species. This list appears in the *Annals of Botany* for December 1890.

Comparative Zoology Museum, Harvard.—The Annual Report of the Curator for the session 1889–90 has just reached us, and like all the annual reports from the pen of Alexander Agassiz it is full of interest. The geological section of the Museum, containing the exhibition-rooms and additional laboratories of the department, is now ready for use. On the first floor it contains a large lecture-room, capable of seating 320 students. On the second floor are placed the petrographical laboratories, one for general use, the other for advanced students. In the basement are the chemical laboratories, photographic rooms, &c.; while on a fourth floor are laboratories for the physical geography department. A view of the University Museum, as seen facing the north-west corner, including the newly erected mineralogical sections, is given. On the very evident ground of safety to the collections it has been arranged that no specimens be loaned even to specialists, and that for the future specialists must visit the collections, which will be freely placed in the Museum at their disposal. The reports of the various officers of departments are given, and all will congratulate the Curator on the great progress that this splendid Museum has made.

MECHANICAL TRISECTION OF ANY ANGLE.

THE following discussion gives simple methods of directly trisecting angles which do not exceed 180°. The relations on which the methods are based, hold, however, up to 270°, but it would be difficult to apply them far beyond 180°.

For angles greater than 180°, the excess over 180° can be trisected, and 60° added by laying off on the arc a distance equal to the radius.

Fundamental Relations.

With any angle, PCK, less than 180° (Fig. 1), if one-third of the angle is represented by KCX, and if, from any point I on the prolongation of one side PC, an arc, IKC′, is described with the vertex C as a centre, the chord IC′ makes with CC′ an angle equal to half PCC′, and therefore equal to KCC′, one-third the original angle.

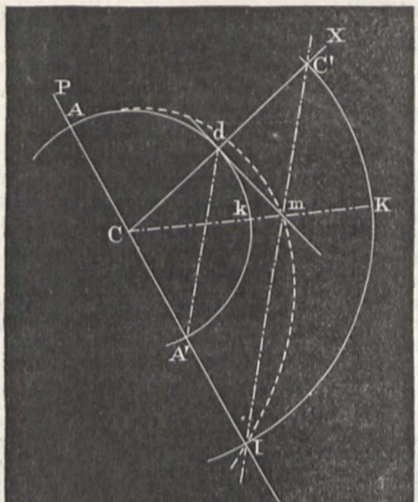

FIG. 1.

The intersection *m* of the radius CK and the chord IC′ is hence :—

(i.) Equidistant from the points C and C′.

(ii.) On a perpendicular to CC′ at its middle point.

The first relation (i.) gives a means of readily plotting the point of trisection, whether on paper or in the field ; the second relation suggests a simple instrument for trisecting an angle without describing an arc.

(1) To lay off one-third a given angle.

Assume any angle, PCK, less than 180° (Fig. 2), and describe an arc with any radius, CI, the vertex C being taken as a centre.

From the point I, where the arc intersects the prolongation of one side, PC, of the angle, draw right lines intersecting the other side, CK. From the points of intersection, *m*′, *m*″, *m*‴, &c., lay off, in a direction away from I, distances *m*′E′, *m*″E″, &c., equal to the distances of *m*′, *m*″, *m*‴, &c., respectively, from the centre C.

Only such lines need be drawn that the extremities will lie near the arc, both within and without, and the curve plotted by these points intersects the arc at the point C′, trisecting the arc PK.

The trisecting point of the supplementary arc KI lies 60° from C′.

(2) From the second relation (ii.) it is evident that the following simple instrument would accomplish the trisection.

Two equal arms, CI and CC′, are jointed at C (Fig. 3) ; one arm has a projection forming a right angle at *d*, and the other arm is prolonged at CA beyond the joint. Points I and C′ on the two arms, at twice the distance C*d* from the centre, are connected by an elastic cord.

Placing the point C over the vertex of any angle, PCK, less than 180°, with the edge CA extended along one side, CP, the other arm is moved until the point of intersection, *m*, of the cord with the perpendicular, *dm*, is over some point on the other side, CK.

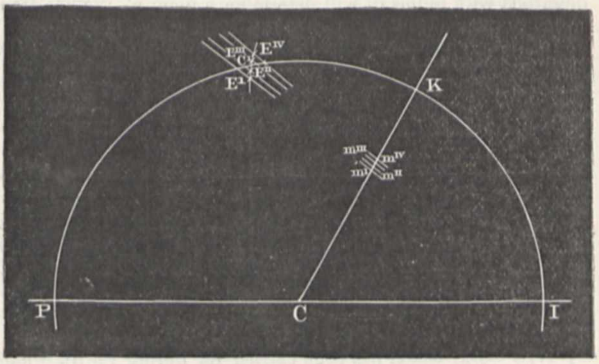

FIG. 2.

A line ruled along the edge CC′ will form an angle with CK equal to one-third PCK.

Instead of a cord connecting I and C′, a ruler may be used. The edges AI and CC′ of the two arms are radial from the centre of motion C.

The angles made by the lines C*m* and I*m* with the right-angled projection *dm*, correspond to angles of incidence and reflection if a plane mirror facing towards C is substituted for the projecting piece.

An observer sighting on an object K, the arm CA being

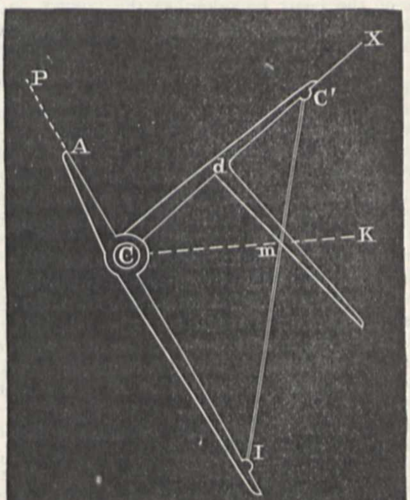

FIG. 3.

directed upon another object P, swings the arm C*d* until he brings the reflection of the point I in the direction of the point K.

Then, sighting in a direction perpendicular to the mirror, he can lay off the angle KCX, one-third the angle PCK.

An instrument of this character suggested the above constructions.

The locus of the point *m* is partially shown in dotted lines in Fig. 1. Its polar equation is evidently $r = a \sec \frac{1}{3}\phi$, a representing the distance C*d* (Fig. 1) ; ϕ, the angle PCK.

For $\phi = 0$, $r = a$, corresponding to the point A.

For $\phi = 180°$, $r = a \sec 60° = 2a$, corresponding to the point I.

For $\phi = 270°$, $r = \infty$, and the curve becomes asymptotic with the line *dm*, now tangent at A′ to the arc A*d*A′.

The curve, extended beyond A, will have two symmetrical branches with reference to the line AI.

The portion shown in Fig. 1 need alone be considered.

This curve possesses the following property:—

If a line is drawn making any angle, PCK, with CP, and from its intersection with the curve, at *m*, as a centre, an arc be struck with a radius equal to C*m*, its intersection with the arc described with radius CI will determine a point C′, the line from which to the centre C will trisect the angle ACK.

To Prof. John Peirce, of Providence, the suggested use of the curve is due.

A. H. Russell,
Captain U.S. Ordnance Corps.

Providence, R.I., April 13, 1889.

THE ACTION OF THE NERVES OF THE BATRACHIAN HEART IN RELATION TO TEMPERATURE AND ENDOCARDIAC PRESSURE.

(Preliminary Note.)

This work, which has been carried on during the past four or five months, has been chiefly concerned with the influence of temperature on the action of the sympathetic and the vagus, and only incidentally with the effect of endocardiac pressure. A re-investigation of the heat standstill of the heart has also been connected with the work. In most of the experiments tracings of the auricular and ventricular movements were taken simultaneously, and these were supplemented by a series of electrical observations.

Omitting details, the chief results are as follows. The term "vagus" is here used in its anatomical sense to denote the mixed vago-sympathetic trunk.

(1) Both the vagus and the sympathetic have their activity diminished as the temperature is lowered and increased as the temperature rises, whether changes in the rhythm or in the amplitude of the beats be taken as the test of activity. The sympathetic curve, however, falls more steeply with falling temperature than does the vagus curve, so that the vagus is generally still active with a temperature at which the sympathetic has ceased to react.

The secondary augmentor effects of vagus stimulation are extremely well marked at the higher temperatures, and become less conspicuous as the temperature is lowered. Not only are both nerves active on the very threshold of the heat standstill, but during the actual standstill, when it has not been obtained at too high a temperature, stimulation of the sympathetic may rouse the heart to vigorous contraction.

(2) An increase of endocardiac pressure sufficient to abolish the inhibitory action of the vagus leaves the sympathetic still active, and the primary augmentation which, under these conditions, has been seen as a result of vagus stimulation, may be attributed to the sympathetic fibres in the mixed nerve.

When a high pressure is suddenly reduced, standstill of the heart may occur, and this can be removed by stimulation of the sympathetic.

(3) Heat standstill of the heart, when there is no constant stimulus acting, such as a high endocardiac pressure, is always diastolic, and can never be described as "heat tetanus."

(4) Electrical effects, analogous to those described by Gaskell in the quiescent auricle of the tortoise on stimulation of the vagus, and in the toad's ventricle on stimulation of the sympathetic, have been looked for in vain in the heart of the frog and toad during heat standstill and the standstill caused by sudden relief of a high endocardiac pressure. The effects observed were always related to coincident or consequent mechanical changes.

So far as the sympathetic is concerned, I do not know of any previous work on the subject of this note; nor has the influence of temperature on the vagus been before studied by a suitable graphic method, but only to a small extent in any way and by methods yielding very imperfect information.

New Museums, Cambridge. G. N. Stewart.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—An examination for Minor Scholarships in certain branches of Law and Natural Science will be held at Downing College on Wednesday, July 8, and subsequent days. Candidates for Minor Scholarships in Natural Science must be under the age of nineteen at the time of commencement of the examination, and must send a certificate of birth with their other papers. There is no such limit of age for candidates in Law. Further information will be furnished by the Rev. J. C. Saunders, Tutor of the College.

SCIENTIFIC SERIALS.

The *American Meteorological Journal* for February contains:—An account of a lecture on the State Weather Service, delivered by Prof. F. E. Nipher at the request of the Board of Agriculture of Missouri. These services aim at weather predictions for the use of agriculturists and others, and the forecasts are of a more local character than can be attempted by the Central Office. The general use of the telephone is recommended for the purpose.—Wind-pressures and the measurement of wind velocities, by Prof. C. F. Marvin. This paper recapitulates the experiments which have been carried on in the United States, concurrently with others in this country and elsewhere, to determine more accurately than was at first done the constants of the Robinson cup-anemometer. The instrument used was the small pattern adopted by the Signal Service, the cups of which are 4 inches in diameter, and the arms 6·72 inches long, and for this form of anemometer the equation

$$\log V = 0{\cdot}509 + 0{\cdot}9012 v \log v$$

is recommended to be used instead of Dr. Robinson's original formula $V = 3v$, which is commonly applied to all cup anemometers, regardless of size of cup and length of arm. An account is also given of some experiments carried out at the summit of Mount Washington, with a view to determining the wind-pressure upon large plates, and comparing it with the wind velocity recorded by the cup-anemometer. The writer finds that, with ordinary barometric pressures, the wind-pressure can be satisfactorily obtained from the wind velocity by the following formula:—

$$P = 0{\cdot}0040 V^2 S,$$

where V is the wind velocity in miles per hour, P is the pressure in pounds per square foot, and S is the area of the plate.—Meteorological observations taken in four balloon voyages, by W. H. Hammon. The ascents were made in January, March, and April, 1885, by direction of the Chief Signal Officer. The Report contains full descriptions of the instruments and detailed observations during the ascents, which were made during conditions of the atmosphere that were considered likely to elucidate certain points. The ascents were only of slight elevation, and the observations are to be considered only as contributions to the special weather of the respective dates, and not as bearing particularly upon the questions involved in the general circulation of the air.

SOCIETIES AND ACADEMIES.

London.

Physical Society, March 20.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Prof. S. U. Pickering, F.R.S., read a paper on the theory of dissociation into ions, and its consequences. According to this theory electrolytes are entirely dissociated into their ions in weak solutions. This dissociation was held by Arrhenius to absorb heat, and although heat is evolved by the dissolution of hydrochloric acid, &c., it is not maintained that dissolution evolves heat, but that the heat absorbed by the decomposition of the molecule into its atoms is more than counterbalanced by the heat supposed to be evolved by the combination of the atoms with their electric charges. These actions the author considered improbable, and thought that before being accepted, the theory must give satisfactory answers to the following questions: How can matter combine with an affection of matter to produce heat? Whence do the electric charges originate? Why does not the opposite electrification of the different atoms make them cling more firmly together, instead of dissolving the union between them? And why should an atom which possesses a strong attraction for a negative charge (such as chlorine) go to the positive electrode during electrolysis? When a dilute solution, which is supposed to contain some gaseous molecules, is further diluted, then, according to the theory, some of the molecules are dissociated, and, if heat is absorbed, it follows that the dissociation, and therefore dissolution, of the gas must absorb heat; yet, he said, it can be shown that in some of these cases the dissolution of a gas evolves a large amount of heat. The antagonism between the present and the old electro-chemical theory, according to which the atomic charges are identical with the free energy of an atom, and are the cause of combination, not decomposition, was commented on, as well as the disagreement between the present theory and Clausius' view that there are a few ions or atoms present in a liquid, owing to accidental superheating of some of the molecules. Reasons, however, were adduced for believing the presence of even a few atoms in a solution to be improbable.—A communication on some points in electrolysis was made by Mr. J. Swinburne. Considering a reversible single fluid cell, the author, by a process of reasoning based on Carnot's principle and the conservation of energy, arrives at Helmholtz's equation—

$$E = E_c + \theta\frac{dE}{d\theta},$$

where E is the electromotive force, E_c the part due to chemical action, and θ the absolute temperature. Writing the equation in full, using suffixes n and p to denote the negative and positive plates, it becomes

$$E_n' + E_p = E_{nc} + E_{c} + \theta\frac{dE_n}{d\theta} + \theta\frac{dE_p}{d\theta}.$$

He then shows that, by having the two plates in different vessels, and heating them to different temperatures, the Peltier effects represented by $\theta\frac{dE_n}{d\theta}$ and $\theta\frac{dE_p}{d\theta}$, can be determined separately. Similarly, those of a two-fluid battery might be found by arranging the junction of the fluids in a third vessel. After pointing out the desirability that the conditions under which all thermo-chemical data have been obtained, should be clearly stated, he proceeded to show that any cell in which secondary actions occur (as, for example, if the zinc oxide primarily formed by electrolysis were to dissolve in sulphuric acid) must necessarily be non-reversible. He also contended that, in a secondary battery, the formation of lead sulphate on both plates is the essence of the cell's action, and that there is no intermediate formation of PbO. On the subject of so-called "nascent" hydrogen or oxygen, he said that reasoning from the conservation of energy showed that neither could exist. Taking the case of persulphate of iron in dilute sulphuric acid, which is said to be reduced to protosulphate by the "nascent hydrogen" liberated on putting a piece of metal (say magnesium) into the liquid, he said a better explanation of the phenomena would be, that the metal dissolves, if it either reduces the metal or evolves hydrogen; and as the former requires less energy, the reduction takes place, and, when no reducible salt is available, hydrogen is evolved. Evolution of hydrogen and reduction of the persalt are thus *alternative* and *not consecutive* actions. Examining Dr. Lodge's views on the contact E.M.F. between metals, he remarked that, if the tendency of a metal (such as zinc) to oxidize can produce an electric stress or difference of potential which prevents further combination, actual combination must charge the metal if it be insulated. A piece of sodium, however, oxidizes continuously, and therefore should become charged to an enormous potential. As this effect is not known to occur, the author suggested that the Volta effect may be due to films of water, and, in support of this view, adduced the fact that metals when perfectly dry do not combine with chlorine, and that even sodium is not attacked by dry chlorine.—In the discussion on the two papers, Prof. Pickering said the idea of nascent elements had, to a large extent, been given up by chemists, and pointed out that the fact of one reaction taking place rather than another was not merely a question of heat energy, but that a kind of chemical selection was involved. Prof. S. P. Thompson recalled attention to the fact that the products of electrolysis depend on the E.M.F. employed in producing it, and thought the E.M.F. required to produce the various products might be taken as a measure of their affinities. He did not agree with Mr. Swinburne's method of finding the E.M.F. of a secondary battery from thermo-chemical data, for he failed to see how two similar actions going on at the two plates of a cell could add anything whatever to the E.M.F. of the cell. The President said the question whether the potential difference between two dissimilar substances was due to oxidation or to mere contact could only be decided by direct experiments made in a vacuum, from which all traces of moisture and oxygen had been removed. Without agreeing with Dr. Lodge's view on the subject, he pointed out that the continuous oxidation of a piece of insulated sodium need not necessarily produce a great potential difference, for the combination might merely produce heat.—After Prof. Pickering and Mr. Swinburne had replied to the points raised, Mr. Walter Baily took the chair, and Prof. Perry read a note on the variation of surface-tension with temperature, by Prof. A. L. Selby. Considering unit mass of liquid at constant volume, but variable surface (S) and temperature (t), the author expresses the gain of intrinsic energy due to changes of the variables by

$$dH + dW = kdt + (l + T)\,dS,$$

where dH is the heat absorbed, dW the work done on the film, k the specific heat at constant volume, l the latent heat of extension, and T the surface-tension. This being a perfect differential, it is shown that $T = c - bt$, and $l = bt$, c and b being constants. Supposing c and b to be independent of the specific volume of the liquid it is shown that at the critical temperature $t = \frac{c}{b}$, hence this temperature may be determined by finding the surface-tension at two very different temperatures. Since also $l = bt$, the latent heat of extension is proportional to the absolute temperature. Reasons for supposing b to be independent of the specific volume are given in the paper. Mr. Blakesley described an effect of temperature on surface-tension which he had observed in sensitive spirit-levels. By warming one end of the tube, even by the hand, the bubble immediately moves towards that end. This effect, which might produce considerable error in engineering operations, was, so far as he was aware, not mentioned in the text-books. Prof. Perry remarked that although the volume, temperature, and surface of the liquid had been referred to in the paper, pressure was not mentioned, and on this point he inquired whether the results arrived at were true, quite independent of the pressure.—Prof. S. P. Thompson, read a paper on magnetic proof pieces and proof planes. The distribution of magnetism over magnets has been examined in various ways by different observers, but mostly by observing the force of detachment of either rods, ellipsoids, or spheres, &c., used as proof pieces. In all these cases it was, the author said, difficult to see exactly what was measured, for the presence of the proof pieces altered the thing to be tested. The pull exerted must also depend on the permeability of the piece used as well as on its shape and disposition with respect to the magnetic circuit. He had therefore investigated the subject, by finding the actual distributions, by means of a flat exploring coil and ballistic galvanometer, both with and without the presence of proof pieces of various shapes and sizes. The results show that the perturbations produced by the proof pieces are always large, in some cases the perturbed field about a point being four to six times the unperturbed field. In most cases, however, the ratio of the perturbed to the unperturbed field was constant, so long as the former did not exceed 6000 C.G.S. units. The amount of perturbation was also found to depend on the saturation of the magnet and on whether it was a permanent or an electro-magnet. The numbers obtained in various experiments, and curves plotted from such results, were shown. In conclusion, the author said that in using proof pieces, much depended on the accuracy of the contact, but in any case the results attained were not very trustworthy. The flat exploring coil, or magnetic proof plane, however, furnished a satisfactory method of examining magnetic distributions.

Chemical Society, March 19.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—Molecular refraction and dispersion of various substances, by Dr. J. H. Gladstone, F.R.S. This paper contains a number of observations of refractive indices, with the optical constants calculated from them, and remarks on their bearing on chemical or physical theory. Observations on carbon bisulphide and benzene confirm the old conclusion that the specific refractive energy of a liquid is a constant not affected by temperature. Data relating to a number of carbon compounds are given; the molecular refraction and dispersion calculated from experiment are compared with those calculated from the values already assigned to the constituent elements. In most cases the experimental and theoretical numbers agree fairly well; in some cases, however, there is a discrepancy. This, of course, points to some difference in constitution. The molecular dispersion of amylene shows the increase for double-linked carbons characteristic of the allyl compounds previously examined, and the allyl and diallyl acetic acids. The xylenes differ considerably in their actual indices, but have almost identically the same molecular refraction and dispersion. The optical properties of safrol agree with the normal value calculated for four double-linked carbons; but anethol, cinnamene, and butenyl benzene, which have the same number of double linkings, give a refraction and especially a dispersion far above that required by theory, thus indicating the presence of carbon in a still more refractive and dispersive condition, to which the author drew attention in 1881. Some of the results afford fresh confirmation of the analogy between molecular refraction and dispersion and molecular magnetic rotation. The nitrogen compounds give results confirmatory of the two values previously assigned to that element in combination. Stannic ethide gives a refraction equivalent for tin 18·1; zincic ethide gives a value for zinc 15·9. The halogen compounds of silicon and titanium gave results confirmatory of the former refractive equivalents; the atomic dispersion of silicon came out about 0·32, and that of titanium about 8·7.—Contributions to our knowledge of the aconite alkaloids: on the crystalline alkaloid of *Aconitum napellus*, by W. R. Dunstan and W. H. Ince. The alkaloid was extracted from the root of *Aconitum napellus* by amyl alcohol. The yellowish crystals melted at 188°·4, and were found to be associated with a small quantity of a gummy amorphous base. They gave, on combustion, numbers agreeing fairly well with the formula $C_{33}H_{43}NO_{12}$, which is that proposed for aconitine by Wright and Luff. The alkaloid is purified by recrystallization from a mixture of alcohol and ether, or by conversion into its hydrobromide and regeneration of the alkaloid from the salt, or by regeneration from its crystalline aurochloride. It crystallizes in tabular prisms belonging to the rhombic system, is very slightly soluble in water and light petroleum, more soluble in ether and alcohol, most soluble in benzene and chloroform, and melts at 188°·5 (corr.). Previous observers state aconitine to be lævo-rotatory; the authors find an alcoholic solution to be dextro-rotatory $[\alpha]_D = + 10°·78$; the aqueous solution of the hydrobromide is, however, lævo-rotatory $[\alpha]_D = - 30°·47$. The pure alkaloid gave, on analysis, numbers corresponding to the formula $C_{33}H_{45}NO_{12}$. Two crystalline *aurochlorides* are obtained, $C_{33}H_{45}NO_{12}HAuCl_4$ (m.p. 135°·5), and $C_{33}H_{45}NO_{12}AuCl_3$ (m.p. 129°). *Dehydra-conitine* or *apo-aconitine*, $C_{33}H_{43}NO_{11}$, is readily prepared by heating aconitine with saturated aqueous tartaric acid in closed tubes, melts at 186°·5, forms crystalline salts, and closely resembles the parent alkaloid. Three *aurochlorides* are obtained, $C_{33}H_{43}NO_{11}HAuCl_4$ (m.p. 141°). This salt on crystallization from aqueous alcohol forms a hydrate, $C_{33}A_{43}NO_{11}HAuCl_4H_2O$ (m.p. 129°), isomeric with aconitine aurochloride, into which it is easily converted. The third aurochloride, $C_{33}H_{43}NO_{11}AuCl_3$, melts at 147°·5. An *amorphous base* is obtained from aconitine, together with benzoic acid, by prolonged heating with water in a closed tube, and appears to be identical with the *aconine* of Wright and Luff. The amorphous base and its amorphous aurochloride gave, on analysis, numbers agreeing with the formulæ $C_{26}H_{41}NO_{11}$ and $C_{26}H_{41}NO_{11}HAuCl_4$.—The crystallographical characters of aconitine from *Aconitum napellus*, by A. E. Tutton. Aconitine crystallizes in the rhombic system. Habit prismatic; $a : b : c = 0·5456 : 1 : 0·3885$. It is a highly dispersive substance. The apparent acute angle of the optic axes in air 2E, for lithium light is 47° 0′, for sodium light 56° 10′, and for thallium light 65° 5′. Hence there is a dispersion of 18° 5′. The double refraction is positive.—The asymmetry of nitrogen in substituted ammonium compounds, by S. B. Schnyver. The primary object of the work was to produce stereo-isomers of pentavalent nitrogen compounds. The author has prepared quaternary ammonium compounds containing three different radicles: (1) by adding methyl iodide to diethylisoamylamine; (2) by adding isoamyl iodide to diethylmethylamine; (3) by adding ethyl iodide to ethylmethylisoamylamine. The platinochlorides of (1), (2), and (3) gave, on evaporation by heat, needle-shaped crystals belonging to the rhombic system. The platinochlorides of (1) and (2), on evaporation *in vacuo*, gave needle-shaped crystals belonging to the monoclinic system, which are easily converted into the rhombic variety; whilst (3), on evaporation *in vacuo*, gave needles which resembled those obtained on evaporation by heat. From these results the author concludes that this difference in crystalline form is due to the various arrangements of the radicles ethyl, methyl, isoamyl, and chlorine round the nitrogen atom, and therefore that the stereo-isomers are possibly capable of existence in the quaternary ammonium compounds.—Acetylcarbinol (acetol), $CH_3 . CO . CH_2OH$, by W. H. Perkin, Jun., F.R.S. Acetylcarbinol can be prepared in large quantities by adding freshly precipitated barium carbonate, in small quantities at a time, to a boiling aqueous solution of acetylcarbinyl acetate ($CH_3CO . CH_2 . OC_2H_3O$, prepared by digesting monochloracetone with potassium acetate in alcoholic solution). The product is purified by repeated fractional distillation under reduced pressure. Acetylcarbinol boils at 120°–122° (250 mm.), and at about 147° under ordinary pressures. When cooled in a mixture of ice and chlorhydric acid, it solidifies to a hard crystalline mass; its relative density at 15° = 1°·07915; magnetic rotation, 3·650. Sodium amalgam converts it quantitatively into methyl glycol.—The action of ethyl dichloracetate on the sodium derivative of ethyl malonate, by Dr. A. W. Bishop and W. H. Perkin, Jun., F.R.S. When a mixture of ethyl malonate (2 mols.), sodium ethylate (2 mols.), and ethyl dichloracetate (1 mol.) is digested in alcoholic solution in a reflux apparatus, and the product fractionally distilled, two new compounds are obtained: α-ethylic ethylenetricarboxylate $(CO_2Et)_2C : CH . CO_2Et$, and β-ethylic propanepentacarboxylate $(COOEt)_2 : CH . CH(COOEt) . CH(COOEt)_2$.—Benzoylacetic acid and some of its derivatives, Part 5, by W. H. Perkin, Jun., and J. Stenhouse. The preparation and properties of the following compounds are described in this paper:—Ethylic allylmethylbenzoylacetate, ethylic methyldibenzoylacetate, ethylic benzoylbenzoylacetate, benzylacetophenone, α-methyl-β-phenyllactic acid, α-ethyl-β-phenyllactic acid, and ethylic purpuralbenzoylacetate.—Syntheses with the aid of ethyl pentanetetracarboxylate, by W. H. Perkin, Jun., F.R.S., and B. Prentice. Ethylic pentanetetracarboxylate yields a disodium derivative, from which, by the action of alkyl iodides, higher homologues may be obtained, and these, on hydrolysis and subsequent splitting off of two molecules of carbon dioxide, yield higher homologues of pimelic acid,

$$(COOEt)_2 : CR . CH_2 . CH_2 . CH_2 . CR' : (COOEt)_2 + KOH = (COOK)_2 : CR . CH_2 . CH_2 . CH_2 . CR'(COOK)_2 + 4Et . OH,$$

and

$$(COOH)_2 : CR . CH_2 . CH_2 . CH_2 . CR' : (COOH)_2 = COOH . CHR . CH_2 . CH_2 . CH_2 . CHR' . COOH + 2CO_2.$$

The preparation and properties of ethyl dibenzylpentanetetracarboxylate, dibenzylpentanetetracarboxylic acid, and dibenzylpimelic acid are described.

Geological Society, March 25.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read:—Notes on Nautili and Ammonites, by S. S. Buckman.—On the drifts of Flamborough Head, by G. W. Lamplugh. The author describes in detail the characters and distribution of the glacial deposits on Flamborough Head, and classifies them as follows:—

Alluvial wash, fresh-water marls, &c. . . .	Recent.
Late glacial gravels	Glacial.
Upper boulder clay	Glacial.
Intermediate series. Stratified beds, with bands of boulder clay	Glacial.
Basement boulder clay	Glacial.
Chalky rubble	Glacial.
"Infra-glacial" beds of Sewerby and Speeton.	

He discusses their relationship with other drifts, and arrives at the following conclusions:—(1) The glacial deposits are divisible into upper and lower boulder clay, with an intermediate series. (2) The lower clay is a continuation of the basement clay of Holderness, and is the product of the first general glaciation of the area. The intermediate series passes laterally into the purple clays of Holderness, and has been deposited at the edge of the ice-sheet. The upper clay includes the Hessle clay of Holderness, and marks the latest glaciation of this region. (3) The fossiliferous beds of Sewerby ("buried cliff beds") and Speeton ("estuarine shell bed") are older than the basement clay, and therefore than the earliest glaciation. (4) The glaciation was effected by land-ice of extraneous origin, which moved coastwise down the North Sea, and did not overflow the greater part of the Yorkshire Wolds. (5) Neither the boulder clays nor the intermediate gravels are of marine origin, the shells which occur in them being derivative. (6) The ice-sheet seems to have filled the North Sea basin in this latitude from the commencement of the glaciation until its close. There is no clear evidence here for a mild interglacial period, but only for extensive fluctuations of the margin of the ice. After the reading of this paper there was a discussion, in which Mr. Clement Reid, Mr. Whitaker, Mr. E. T. Newton, Prof. Hughes, the President, and the author took part.—On a phosphatic chalk with *Belemnitella quadrata* at Taplow, by A. Strahan. (Communicated by permission of the Director-General of the Geological Survey.) Two beds of brown chalk in an old pit near Taplow Court owe their colour to a multitude of brown grains. These grains are almost entirely of organic origin, foraminifera and shell prisms forming the bulk of them. Mr. Player has analyzed specimens of the brown chalk, and finds that it contains from 16 to 35 per cent. of phosphate of lime. The tests as well as the contents of the foraminifera seem to have been phosphatized, the phosphate appearing as a translucent film in the former case, and as an opaque mass in the latter. In the case of the prisms of molluscan shells, the whole of the phosphate appears to be in the opaque form. Minute coprolites also occur, together with many small chips of fish-bone, in which Dr. Hinde has recognized lacunæ, while some have been identified by Mr. E. T. Newton as portions of fish teeth. Mr. Player observes that the phosphate occurs in such a condition that it would not improbably serve as a valuable fertilizer, without conversion into superphosphate. This condition is probably due to the partial replacement of carbonate of lime by phosphate in the organisms. The removal of the remaining carbonate leaves the phosphate in a honeycombed state, peculiarly favourable for attack by the acids in the soil. The author comments upon the resemblance of the deposit to the phosphatic chalk with *Belemnitella quadrata* which is largely worked in Northern France, and upon a less striking resemblance with that of Ciply, which is at a higher horizon. Some remarks on this paper having been made by Dr. G. J. Hinde, Mr. Whitaker, and Prof. Judd, the President, alluding to the geological and economic interest of the discovery described in the paper, said that, though the area occupied by the phosphatic layers seemed to be small, there was good reason to hope that somewhere else in the upper chalk districts the same or similar bands might yet be found. The search for such deposits would now be stimulated by the information so fully supplied by the author, who himself would no doubt follow up his observations at Taplow by a thorough examination of the higher members of the chalk in the east of England.

Entomological Society, April 1.—Prof. R. Meldola, F.R.S., Vice-President, in the chair.—Captain H. J. Elwes showed a small but very interesting collection of butterflies from Laggan Alberta, North-West Territory of Canada, taken by Mr. Bean at high elevations in the Rocky Mountains. Amongst them were *Colias elis*, Streck., which seemed to be very close to, if not identical with, *C. hecla* of Europe, *Argynnis alberta*, W. H. Edw., and *Chionobas subhyalina*, W. H. Edw. The resemblance between the butterflies of this locality and those found on the Fells of Lapland was very striking, some of the species being identical, and others very closely allied. Captain Elwes said that it was another proof, if one were wanted, of the uniformity of the butterflies found throughout the boreal region in the Old and New Worlds.—Mr. G. C. Champion exhibited several insects recently received from Mr. J. J. Walker, from Hobart, Tasmania. The collection included a curious species of *Forficulidæ*, from the summit of Mount Wellington; two mimetic species of *Œdemeridæ* belonging to the genus *Pseudolycus*, Guér., and the corresponding *Lycidæ*, which were found with them; also specimens of both sexes of *Lamprima rutilans*, Er.—Mr. N. M. Richardson exhibited a specimen of *Zygæna filipendulæ* with five wings; four specimens of *Gelechia ocellatella*, including a pink variety, bred from *Beta maritima*; four specimens of *Tinea subtilella*, a species new to Britain, taken last August in the Isle of Portland; also specimens of *Nepticula auromarginella*, a species new to Britain, bred from larvæ taken near Weymouth on bramble.—Mr. C. Fenn exhibited a series of *Tæniocampa instabilis*, which had been bred out of doors during the recent severe weather. They were all bred from ova laid by the same female, and many of them were of an abnormally pale colour. Mr. Fenn said that, according to Mr. Merrifield's theory, these pale specimens, in consequence of the temperature to which they had been subjected in the pupal state, ought to have been very dark. Mr. Jenner Weir said he had never before seen any specimens of so light a colour.—Mr. W. Dannatt exhibited a butterfly belonging to the genus *Crenis*, recently received from the Lower Congo. He said he believed the species was undescribed.—Mr. G. A. J. Rothney sent for exhibition several specimens of an ant (*Sima rufo-nigra*), from Bengal, together with specimens of a small sand wasp (*Rhinopsis ruficornis*) and a spider (*Salticus*), both of which closely mimicked the ant. It was stated that all the specimens exhibited had lately been received from Mr. R. C. Wroughton, of Poona. Mr. Rothney also communicated a short paper on the subject of these ants, and the mimicking sand wasps and spiders, entitled "Further Notes on Indian Ants."—Mr. G. C. Champion read a paper entitled "On the Coleoptera collected by Mr. J. J. Walker, R.N., in the neighbourhood of Gibraltar, with descriptions of new species." At the conclusion a discussion ensued, in which Mr. Kirby, Captain Elwes, Mr. McLachlan, Mr. Jenner Weir, Dr. Sharp, and Mr. Crowley took part.

DUBLIN.

Royal Society, March 18.—On the cause of double lines in the spectra of gases, by Dr. G. Johnstone Stoney, F.R.S. The alternations of electro-magnetic stress in the ether which constitute light form an undulation which is propagated through the ether under the same laws as the transverse vibrations of an incompressible elastic solid (see McCullagh's papers, *passim*). It is convenient first to regard certain points in the molecules of the gas as acting dynamically on such a medium, to inquire what oscillations within the molecules would impart to the medium the vibrations which correspond to the observed lines in the spectrum, and afterwards to correct the investigation so as to transfer it from the dynamical hypothesis of light to the electro-magnetic theory. Let us then suppose that a point P in the molecule acts on the ether, and is set oscillating within the molecule by the inter-molecular encounters. Its undisturbed path will be represented by the simple elliptic motion

$$\left.\begin{array}{l} x = a \,.\, \cos\theta \\ y = b \,.\, \sin\theta \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

θ being written for $2\pi mt/\tau$, in which m is the oscillation-frequency in the time τ. Perturbating forces operating within the molecule would cause an apsidal motion of the ellipse in its own plane, and perhaps a motion of the nodes of this plane upon the invariable plane. The apsidal motion is represented by

$$\left.\begin{array}{l} X = a \,.\, \cos\theta\cos\omega - b \,.\, \sin\theta\sin\omega \\ Y = a \,.\, \cos\theta\sin\omega + b \,.\, \sin\theta\cos\omega \end{array}\right\} \quad . \quad . \quad (2)$$

where $\omega = 2\pi nt/\tau$, n being the oscillation-frequency of the apsidal revolutions. Equations (2) are equivalent to

$$\left.\begin{array}{l} X = \dfrac{a+b}{2} \,.\, \cos\,(\theta + \omega) + \dfrac{a-b}{2} \,.\, \cos\,(\theta - \omega) \\ Y = \dfrac{a+b}{2} \,.\, \sin\,(\theta + \omega) - \dfrac{a-b}{2} \,.\, \sin\,(\theta - \omega) \end{array}\right\} \,.\, (3)$$

a motion which, when imparted to the surrounding medium, represents a pair of lines in the spectrum with oscillation-frequencies $(m + n)$, and $(m - n)$, and intensities which are to one another in the ratio of $(a + b)^2$ to $(a - b)^2$. These quantities are not altered when the various positions in which the molecules chance to be at any one instant are taken into account. Equations (2) and (3) represent an apsidal motion in the same direction as the primary elliptic motion, and in this case the more refrangible line is the brighter. If the apsidal motion is in the opposite direction, we shall have to change the sign of ω in equations (2) and (3). This does not alter the positions, but it is now the less refrangible line which is the brighter. A third case occurs when the primary motion is an oscillation in a straight line instead of in an ellipse: here $b = 0$, and the intensities of the two lines become equal. All three cases occur in the observed spectra of gases. A motion of the nodes on the invariable plane will give rise to additional pairs of lines, and if the inclination of the two planes is small, one of the pairs only is conspicuous. Such a motion may be what occasions the faint lines that have sometimes been called satellites. It would at the same time cause a shifting of the position of the bright lines—a matter of importance in grouping the lines of a spectrum into series. To pass from the dynamical investigation to the electro-magnetic, attention must be given to Faraday's law of electrolysis, which is equivalent to the statement that in electrolysis a definite quantity of electricity, the same in all cases, passes for each chemical bond that is ruptured. The author called attention to this form of the law in a communication made to the British Association in 1874 (see Scientific Proceedings of the Royal Dublin Society of February 1881, and *Philosophical Magazine* for May 1881, p. 385); and showed that the amount of this quantity of electricity is about the twentiethet (that is, $1/10^{20}$, or a unit in the twentieth place of decimals) of the usual electro-magnetic unit of electricity, *i.e.* the unit of the ohm series. This is the same as three eleventhets (or $3/10^{11}$) of the much smaller C.G.S. electrostatic unit of quantity. A charge of this amount is associated in the chemical atom with each bond. It appears to be irremovable from the atom, but becomes for the most part disguised when atoms chemically unite. If this charge be lodged at the point P of the molecule which undergoes the motion described above, the oscillation of the charge will cause an electro-magnetic undulation in the surrounding ether. The only change that has to be made in our investigation to adapt it to this state of things is to change θ into $\frac{\pi}{2} + \theta$, *i.e.* a mere change of phase. We in this way represent the fact that it is the direction and velocity of the motion of P, not the direction and length of its radius vector, which determine the direction and intensity of the electro-magnetic stresses in the surrounding ether. We have further to correct for the change of phase, about one-fourth of a vibration-period, consequent upon what takes place in the immediate vicinity of the moving charge. Within the molecule itself, the oscillation of the permanent charge probably causes electric displacements in other parts of the molecule, and to the reaction of these upon the oscillating charge we may perhaps attribute those perturbations of which the double lines in the spectrum give evidence. The periodic times of all the molecular motions dealt with in this paper, of the primary and apsidal motions, of the motion of the nodes, of change of ellipticity, &c., and also the form of the ellipse, can be deduced from what may be observed in the spectrum.

Paris.

Academy of Sciences, March 31.—M. Duchartre in the chair.—On the third meeting of the Permanent International Committee for the execution of the photographic map of the heavens, by Admiral Mouchez (see Nature, April 2, p. 516). —A new gyroscopic apparatus, by M. G. Sire.—New observations on the Marseilles sardine, by M. A. F. Marion.—The earthquakes at Algiers on January 15 and 16, by M. A. Pomel. It is remarked that no apparent relation appears to exist between the seismic movement and the geological structure of the region affected.—New nebulæ discovered at Paris Observatory, by M. G. Bigourdan. A list is given of new and interesting nebulæ discovered by M. Bigourdan between 0 hours and 9 hours of right ascension in the years 1887-90.—On the variations observed in the latitude of a single place, by M. A. Gaillot. Diurnal variations in latitude have been found from a discussion of meridian observations made at Paris during the years 1854-57. An annual variation does not appear very definitely marked. The results indicate that all hypotheses relative to the cause of the phenomenon are premature, for in all probability there is no real variability in the direction of the earth's axis, the question being merely one of temperature and refraction. —On the theory of conformable representations, by M. Paul Painlevé.—On the pressure in the interior of magnetic or dielectric media, by M. P. Duhem.—Propagation of Hertz's electrical undulation in air, by MM. Édouard Sarasin and Lucien de la Rive. The most important result obtained is, that the velocity of propagation of Hertz's electrical undulations across air is very sensibly the same as that with which they are transmitted along a wire conductor.—New method for the investigation of feeble bands in banded spectra; application to hydrocarbon spectra, by M. H. Deslandres. It is shown that the bands or flutings characteristic of hydrocarbon spectra occur periodically. M. Deslandres has discovered three new bands at λ 438·19, λ 437·13, and λ 436·5. These bands are not given by the combustion of hydrocarbons, but they may be seen with the ordinary bands of hydrocarbon and cyanogen in the electric arc and in the cyanogen flame. By the application of the above law, M. Deslandres has been able to determine that these bands really belong to hydrocarbon.—On the origin of the higher alcohols contained in industrial ferments, by M. L. Lindet.—On vegetable hæmatine, by M. T. L. Phipson.—On the employment of liquefied carbonic acid for the rapid filtration and sterilization of organic liquids, by M. A. d'Arsonval.—The males of the Ostracoda of sweet water, by M. R. Moniez. —On the influence of salinity in the formation of starch in chlorophyllian vegetable organs, by M. Pierre Lesage. According to the author, the presence of salt in waters is an obstacle to the production of starch in vegetable tissue.—Note on the simultaneous disengagement of oxygen and carbon dioxide in Cacti, by M. E. Aubert. Some observations show that Cacti, when submitted to a temperature of about 35°, and a light of moderate intensity, simultaneously emit oxygen and carbon dioxide.—The artificial reproduction of amphibole, by M. K. de Kroustchoff. The author has been able to produce crystals of this rock by synthesis.—Some magnetic anomalies observed in the centre of Russia in Europe, by General Alexis de Tillo. Anomalous magnetic disturbances are described, similar to those found by Profs. Thorpe and Rücker in England.—A definite depression in the centre of the Asiatic continent, by the same author. Some barometric readings indicate that a little south of the town of Tourfan, in the centre of the Asiatic continent, there exists a depression 50 metres below the level of the ocean.

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