THURSDAY, AUGUST 22, 1889.

AFRICAN FARM PESTS.

Notes and Descriptions of a Few Injurious Farm and Fruit Insects of South Africa. Compiled by Eleanor A. Ormerod, F.R.Met.Soc., &c. (London: Simpkin, Marshall, and Co., 1889.)

MISS ORMEROD is indeed energetic. Not content with waging bitter war against the destructive pests of British crops, she extends her campaigns to far distant lands. Although applications of farmers for information as to the attacks of many injurious insects arrive incessantly from all parts of the United Kingdom, Miss Ormerod finds time to deal with the complaints of cultivators in all other parts of the world, and surveys insects from China to Peru. In 1887 an elaborate treatise upon a scale-insect (*Icerya purchasi*) which seriously injures vines, fig, orange, peach, and other trees and shrubs in Australia, was published by Miss Ormerod, giving full scientific details as to the life-history of this coccid, and as to methods of prevention and remedies likely to be beneficial. Just recently another work has appeared, entitled "Observations on some Injurious Insects of South Africa," written in Miss Ormerod's usual clear and interesting style, and admirably illustrated. From this we propose to take some extracts to show the cosmopolitan entomological knowledge of the authoress, as well as to give some idea of the enemies of cultivated plants in "Afric's golden sands."

The first reflection that arises from the perusal of this book, and of others descriptive of the usual plagues which follow in the wake of civilized cultivation, is that no clime nor culture under the sun is exempt from them. No sooner had orange groves been formed in the suitable lands of Florida than various scale-insects swarmed upon the trees. The purple scale, the white scale, the red scale, and the "chaff" scale, among others, threatened to ruin the crops and to kill the trees, until remedies were discovered by the skilful economic entomologists of the United States.

In the newly made tea plantations of Assam a red spider (*Tetranychus bioculatus*) has suddenly shown itself, and threatens serious mischief to the plants, unless some wash can be invented which will kill the spiders, and leave no taste in the tea-leaves.

So in Africa, scale-insects, especially the *Icerya purchasi*, have within the last few years, as shown by Miss Ormerod, extensively spread in Cape Colony, upon oranges, lemons, vines, and other kinds of fruit-trees. This scale-insect has also now become one of the most dangerous pests infesting fruit-trees in California. It is the opinion of Prof. Riley, the United States Entomologist, that the *Icerya purchasi* originated in Australia. There was some doubt at first as to this. It was thought that it came from Mauritius; but it was discovered that it was the species known as *Icerya sacchari* which was imported from thence upon sugar-canes to California.

Frequent and rapid communication between countries, and the general interchange of commodities of all kinds, have undoubtedly disseminated insects throughout the world. Thus the Hessian fly was brought from the United States with straw in packing-cases, and the flour moth (*Ephestia kuhniella*), has been recently imported into England from some European country, as the authorities conclude.

At the same time it is considered that the conditions of cultivation adopted in these days, being somewhat of a forcing and unnatural nature, favour the increase of insects, and, in a degree, predispose plants to their attacks, which become more varied and intensified year by year not only in this country but in all others. It is of the greatest importance, therefore, that cultivators should be advised by competent persons as to the life-histories of these crop destroyers, and as to measures to be taken to prevent their spread, and remedies to be used against them.

Miss Ormerod has arisen, a very *Dea ex machinâ*, and for several years has given timely instruction and advice to agriculturists of many nations, just as Professors Riley, Lintner, and Comstock have helped the tillers of land in the United States to detect and combat the onslaughts of many crop-destroyers. Prof. Lindeman is doing the same good work in Russia, Dr. de Man in the Netherlands, Dr. Taschenberg in Germany.

And in this her latest work, "Notes and Descriptions of Injurious Farm and Fruit Insects of South Africa," Miss Ormerod conveys much practical information which can be readily understood and easily utilized. At the same time it is scientific enough for entomologists, and interesting to those who are not cultivators or men of science.

Miss Ormerod explains her reasons as follows for publishing this account of African farm pests, though no explanation appears necessary for such a valuable service:—

"About four years ago, Mr. J. D. Bristow, President of the East Province Natural History Society of Cape Colony, wrote to inquire whether, if I were furnished with notes regarding pests of the crops in East Province, Cape Colony, I would publish them. At that time there was not the opportunity which there now is of procuring sound and clear directions for treatment of insect as well as other attacks of crops and stock, by reference to the *Agricultural Journal*, published by the Department of Agriculture of Cape Colony, therefore I willingly agreed to do my best, in case trustworthy agricultural observations could be procured, and specimens of the insects referred to also sent for identification. Specimens of about fifty kinds of insects were sent over, of which a few proved to be of undescribed species; and I have given in the following pages, as far as I can, figures and observations of habits and means of prevention of some of the pests, and means by which they might be identified."

The first insect described is one of the well-known dangerous family *Melolonthidæ*, to which the familiar British cockchafer belongs. The scientific name is *Eriesthis stigmatica*, but it is commonly termed the "mealie," or maize, chafer, as it is the worst pest in the country for crops of maize, and destroys it from near Fort Beaufort right down to the Fish River mouth. This is very much smaller than the cockchafer, and smaller, too, than the rose chafer (*Cetonia aurata*), which is, however, of a different family.

Another chafer, *Hypopholis sommeri*, injures vines considerably, probably by devouring the leaves, in the perfect state, and feeding on the roots, in the larval state. It is much larger than the "mealie," of a red-brown colour, and, "judging from the number of specimens usually contained in collections received from South Africa, this would appear to be a very common species in that country."

It is desirable to destroy the insects belonging to this family both in their beetle and larval conditions. In the former case they may be shaken down from the trees and shrubs upon which they rest in a sluggish state during the day. The larvæ can be reduced in numbers by digging round the roots of the infested plants. "In Ceylon," Miss Ormerod remarks, "where the grubs of various kinds of chafers do much harm to coffee-plant roots, it is noted that on one estate a gang of coolies was employed to dig them out of the ground (for they are always near the surface at the end of the feeding rootlets), which they did at the rate of about a quarter of a bushel per man per day. Lime, salt, and carbolic acid were tried without effect, I believe."

Some species of Melolonthidæ, especially the common cockchafer (*Melolontha vulgaris*), are so destructive to trees, vines, and other plants in France, that the French Minister of Agriculture has recently issued orders to the various prefects to take steps to reduce their numbers.

A beetle of the family *Dynastidæ*, styled *Heteronychus arator*, and the "keever" by the farmers, is the most widespread and destructive of the African pests, according to a correspondent, being a subterraneous worker, attacking wheat by eating away the roots. In length of body it is from 12 to 15 millimetres, and resembles the British beetle *Aphodius fossor*, belonging, however, to a quite distinct family, of which no member occurs in Britain.

They are rarely seen above ground, biting off the wheat plants about an inch under ground. It will be plain to all practical persons that this insect must be a very difficult foe to deal with, as it comes out of the ground and flies about trees during the night, and retires to the wheat roots during the day. Miss Ormerod states that it is also found in St. Helena, and it is more particularly to be met with along the sides of roads near hay-fields and grass-lands, where it may often be seen dead in considerable numbers, "which points to this 'keever' beetle being a grass as well as a corn pest, and may give some clue as to where to look for their grubs, as in common course of things beetles die just after having laid their eggs." It is recommended to dress wheat plants thus attacked with paraffin and sand, or ashes, or dry earth, to prevent the attack of this beetle.

Two other beetles, the *Pentodon nireus* and *Pentodon contractus*, injure the wheat crops. They are larger than the *Heteronychus arator*, but are similar in their method of attack. Neither of these appears to have any common or local name.

Yet another beetle, belonging to the family of *Cetoniidæ*, in which the British rose chafer is grouped, and defined as *Rhabdotis semipunctata*, a pretty insect with a bright green upper surface marked with white lines and spots, causes much injury to the blossoms of apple and plum trees, and later on to ripening peaches and figs. In some parts it is so plentiful that the only peaches and figs saved were those tied up in muslin bags. Miss Ormerod suggests that the only measures against this insect are to catch the beetles in nets when flying in the sunshine, as is done in England, and destroying their grubs at the roots of trees and plants.

Among other Coleoptera figured are the *Mylabris oculata*, a large, handsome black beetle, with red or yellow bands, of the family *Cantharidæ*, possessing the same vesicating properties, and sadly troublesome to peas and beans and fruit blossoms. Also a *Bruchus*, an unknown species, but which Miss Ormerod conjectures may be *subarmatus*, Gyllenhal. "Of the British *Bruchi* this species is most nearly allied to *Bruchus loti*." From Miss Ormerod's careful account of its larva, it does not much differ from that of *Bruchus pisi*.

This species of *Bruchus* infests peas and beans just in the same manner as the British *Bruchus rufimanus*.

> "Its attack is seriously hurtful. In two of the beans figured above, I found four holes showing where the beetles had escaped; in another seven beetles had been present, and in another I found five beetles, or coloured chrysalids, still within. Mr. Bairstow reported that this pest when in larval condition reduces the interior of the seed to a fine powder, and passes into a pupa, sometimes in its powdery bed, which disappears almost immediately on the emergence of the perfect insect."

Miss Ormerod gives an elaborate account of the modes of attack of this *Bruchus*, which do not much differ from those of its British congener. Also, it is recommended that infested beans should be steeped in a solution of Calvert's carbolic acid, so diluted as not to damage the seed.

Among the destructive Lepidoptera, butterflies and moths, in South Africa, the large handsome *Papilio demoleus*, or orange butterfly, figures prominently. The larvæ of this insect eat the leaves of orange trees, appearing "generally at the end of November, or beginning of December, and in greater numbers on trees whose lower branches are allowed to trail on the ground. It is advisable to cut all branches one foot above the ground, to turn as often as possible the soil within a circle of one foot from the trunk, keeping it moist, and to watch for the grubs, which are large, and of a yellowish-green colour, during the summer season."

The caterpillars of a moth, *Trilocha ficicola*, belonging to the *Bombycidæ*, cause much damage to fig trees by eating their leaves and nascent buds. For this attack, hand-picking is advised, and turning over the soil beneath the trees where the cocoons may be.

The "diamond-back" moth (*Plutella cruciferarum*), so well known in Britain, is a source of much trouble to Cape cabbage-growers, for which Miss Ormerod prescribes nitrate of soda applications to push on the growth of the plants, and sweeping the infested plants with boughs fixed to a scuffler.

To fruit-growers, the orange fly "Trypeta" (*Ceratitis citriperda*) is a bitter enemy. "In some districts last year," Mr. Hellier writes, "Albany amongst the rest, four-fifths of our peaches, apricots, figs, and plums were uneatable." The fly deposits some half-a-dozen eggs in a fruit when it is getting ripe. From these maggots come, and soon render the fruit useless. Concerning its life-history, Miss Ormerod quotes Prof. Westwood's remarks, from which extracts will be received with much interest by all economic entomologists.

"The perfect insect is one of the most beautiful of the order to which it belongs; the male is singularly distinguished by having two slender filaments arising between the eyes, knobbed at the tips, a peculiarity which we believe is possessed by no other dipterous insect, and which is wanting in the female. The larva is a white, fleshy grub destitute of legs, very similar to the celery fly, and like it possesses two small black hooks at the front of its body, which it alternately protrudes and retracts, thereby tearing the delicate membrane in which the drops of juice are contained. There are generally several of these larvæ in each orange, and when removed and placed upon a flat surface, they have the power of springing to a considerable distance, in the same manner as the well-known cheese-mite. When full-grown they eat their way out of the orange, and undergo the change to the pupa state on the outside."

Prof. Westwood adds that the presence of infestation might be inferred sometimes by a puncture not larger than that made by a pin, but generally surrounded with a withered and discoloured spot varying in size from sixpence to half-a-crown.

Miss Ormerod characteristically observes:—

"One main point of information wanted to check attack is, Where does the maggot in natural circumstances undergo its changes to the chrysalis state? It might be found out very easily whether the maggots drop to the ground, by laying cloths smeared with any sticky material, which would prevent their straying away, beneath a few of the infested trees. Any fruit that is infested should not be allowed to remain on the ground to continue the attack, and if on investigation it should prove that the later maggots go into the chrysalis state in the ground, or even that a large proportion of them do so—or if it could be found where the flies generally hibernate—these points would help greatly in lessening attack."

Ostrich-farming, extensively adopted in South Africa, is much hindered by "a terrible pest," in the shape of the ostrich flies, of which sometimes there are "thousands on the ostriches, and that they irritated the birds so that half of their time was taken up in pecking at the flies, and that, judging from the increase in the last two years, if something was not done to destroy them, the feathers would not be worth sending to market, and the writer believed that in time they would destroy the birds."

The fly in question is akin to the "forest fly" (*Hippobosca equinæ*), which is found upon horses and ponies in the New Forest, in Hants. These get accustomed to the flies, and make no special demonstrations concerning them; but a horse which is strange to their attacks goes half mad when one of these insects gets upon it.

In the case of ostriches, Miss Ormerod considers tobacco decoctions, soapsuds, and other washes applied to the birds would be found to succeed. McDougall's dip has been used, and Miss Ormerod thinks that sulphur would be useful.

Cattle ticks (*Amblyomma hebræum*), of the family *Ixodidæ*, some of them three-quarters of an inch long by three-eighths wide, described as the "disastrous hide perforators of these regions," sadly vex the South African cattle, so that it would indeed be a boon to the country if any means could be discovered to destroy, or, what would be better, "prevent the presence of a little creature which moves a market downwards at a high percentage."

The most interesting part, perhaps, of this work is that which deals with some of the *Coccidæ*. First among these is the "Australian bug" (*Icerya purchasi*), which was noticed in the Botanic Garden at Cape Town in the latter half of 1873, and has now spread over 680 miles in the eastern provinces of Cape Colony. Where it came from, Miss Ormerod says, it does not appear, "but there is not room for doubt that the attack was set on foot by imported specimens, and that it rapidly became naturalized. It attacks" orange trees, vines, fig trees, deciduous fruit-trees, ornamental shrubs, and garden plants, even strawberry plants.

The life-history of this insect is remarkable. When the female, a salmon-coloured, tortoise shaped insect, with six legs and long antennæ, is one-sixth of an inch long, it secretes snow-white cotton-like matter, which is formed under the mother for the covering and protection of its eggs. For some time the female moves about, but after a while all movement is arrested, and the insect seems glued to one spot. In time the eggs are hatched, and larvæ, as many as 200 often from one female, go forth to find congenial food upon the plant, and in turn lay eggs in the same manner. The eggs are not hatched at once, but the process continues for several weeks. In the meantime the mother shrivels up, and becomes a mere dead husk, covering the eggs and hatching young. The whole process extends over many weeks, and gives ample opportunity for immense damage to be caused.

The male is rarely seen. It has wings more than a fourth of an inch in expanse; its body is about the eighth of an inch long, orange-red in colour, with long ten-jointed antennæ. Prof. Riley gives the best account of the male insect, stating that it is "fond of shelter, and will get under any projecting piece of bark or under bandage placed round the tree, often creeping under clods of earth. It is rather sluggish during the day, remaining motionless in crevices of the bark or wedged in between females on the tree. There seems, in fact, a well-marked attempt at concealment. At the approach of night they dart rapidly about on the wing, swarming round infected trees."

Miss Ormerod prescribes various washes as remedies against this and other coccids, and gives a list of insect enemies, which are happily numerous and formidable. The larva of the "golden eye" (*Chrysopa perla*) is most useful, destroying the young "Australian bug" just at hatching-time within the sac of the female. The Chrysopa also devours quantities of Aphides which infest hop plants in England. Species of ladybirds (*Coccinellæ*) are by far the best friends to African cultivators. They are invaluable in destroying the perfected young in the nidus of the female "bug," just as they, both as larvæ (niggers) and perfect insects, eat myriads of hop Aphides in the English plantations.

There are other insects which do good service in keeping down this *Icerya*, described by Miss Ormerod, of which, however, there is not space to give any account. Other destructive and useful insects have not been alluded to for the same reason, but those who take an interest in economic entomology will do well to peruse this instructive little book, and extract for themselves its valuable contents.

THE FOREST FLORA OF NEW ZEALAND.

The Forest Flora of New Zealand. By T. Kirk, F.G.S., late Chief Conservator of State Forests to the Government of New Zealand, &c. Folio, 345 pages and 160 plates. (Wellington: By authority; George Didsbury, Government Printer, 1889.)

MR. EDWARD BARTLEY states[1] that only four kinds of New Zealand timber are used in Auckland for building purposes, and that these are: kauri (*Dammara australis*), rimu (*Dacrydium cupressinum*), totara (*Podocarpus totara*), and kahikatea (*Podocarpus dacrydioides*); but Mr. Kirk's "Forest Flora" goes far beyond the timber-yielding element of the New Zealand forests. It illustrates and describes nearly the whole of the shrubby and arboreous plants of New Zealand, and gives very full particulars of their dimensions, qualities, uses, distribution, and propagation. It is by no means a mere compilation; and, although the author acknowledges various sources of information, a slight examination of the work is sufficient to convince us that it is very largely based upon personal observation, and that the details are elaborated with great care.

Altogether the flora of New Zealand contains rather less than a thousand species of flowering plants, and of these 115 are illustrated in the present work; some of the more important and specially interesting species by several plates. The tree ferns, which form so conspicuous a feature in the vegetation of the country, are not included, and some shrubby plants are omitted whose claims to notice are at least equally as strong as some of those admitted; but this is explained by the author's desire to do the work thoroughly as far as he went. For this reason, several of the small shrubby Coniferæ are figured, and a considerable number of plates are devoted to the illustration of the heterophyllous members of various natural orders.

Heterophylly is not a peculiar feature of insular floras, though it is perhaps nowhere more conspicuously developed than it is in New Zealand and Rodriguez.[2] Many of the New Zealand Coniferæ, the most valuable of the timber-trees, exhibit this peculiarity in a high degree, insomuch that different parts of the same tree have been referred to different species and even to different genera. Thus different parts of *Dacrydium Colensoi* might represent a juniper and a cypress, while in *D. Kirkii* there is a still more remarkable dimorphism. In explanation of Plate 97, Mr. Kirk writes:—

"As in the preceding species (*D. Colensoi*), the foliage is of two kinds, but the difference is of a still more striking character. The lower branches are spreading, the upper ascending or erect, the ultimate branchlets forming fan-shaped masses. The lower branches, sometimes to the height of 40 feet, are clothed with long narrow flat leaves; the upper branches are clothed with small scale-like, closely appressed leaves; so that the lower part of the tree resembles a silver-fir, while the upper part puts on the appearance of a cypress."

Figures are given of the dimorphic foliage of these conifers, and the confusion in nomenclature which has arisen in consequence of one kind only having often been taken by collectors is unravelled. It should be added that it is only the cypress-like branches that bear flowers and fruit.

In these New Zealand Coniferæ the foliage is dimorphic, but in several plants belonging to other natural orders it is polymorphic. Noteworthy among these are several members of the Araliaceæ, also *Rubus australis* and *Hoheria populnea*, of the order Malvaceæ. Mr. Kirk has very fully illustrated some of these remarkable plants, devoting five plates to the last-named, and as many to *Pseudopanax crassifolium* (Araliaceæ), a plant not unknown in cultivation in this country. The earliest leaves following the cotyledons of this *Panax* are small, narrow, and sharp-pointed, with entire margins; "but the leaves next produced are very different: they are distinctly stalked, 1 to 2 inches in length, rhomboid or elongate rhomboid in shape, and sharply toothed or deeply lobed, bearing some resemblance to those of the common hawthorn. Succeeding leaves are longer and of uniform width, until they sometimes attain the length of 3 feet 6 inches, while they scarcely exceed half an inch in width and are invariably deflexed. In this stage the leaves are thick and leathery in texture and acute at the apex, with distant sharp marginal teeth." This is the form in cultivation in this country, sometimes called the fish-bone tree, which Sir Joseph Hooker named *Panax longissimum*, as it had retained its peculiar character unchanged for fifteen years; though subsequent investigation proved that it was no other than *P. crassifolium*. In this stage of development the stem is invariably unbranched, and rises to a height of 20 feet; and, as it often retains its leaves almost to the base, it presents a very remarkable appearance. Following these long, narrow, undivided leaves, come others composed of three to five separate leaflets, and borne on petioles from 1 to 5 inches in length. The leaflets are less stiff than the long leaves, narrower, and having sharply-toothed margins. These are succeeded by similar leaves on longer petioles, having broader leaflets, thicker in texture, with coarser, more distant teeth. Flowers are sometimes produced at this stage, but not unless the stem has given off lateral branches. In the usual flowering stage the leaves have again become simple, and they gradually become thicker, the toothing almost wholly disappears, and they are from 4 to 6 inches in length, and borne on short stout petioles. To add to the perplexities of the plant, the male and female flowers are borne on different individuals. It is not surprising, therefore, that botanists working with herbarium specimens only have failed to limit or define the species correctly. As already stated, many other New Zealand plants are remarkable for the great variability of their leaves at different stages of development, and the kind of variation is as diverse as the extent of it.

There are so many other interesting subjects in Mr. Kirk's book that one is at a loss which to select for notice. Specially noteworthy is the presence in New Zealand of three species of *Fuchsia*, a genus otherwise restricted to America; and remarkable among these is *F. excorticata*, which sometimes attains a height of 45 feet, with a gnarled trunk up to 3 feet in diameter. The Central American *F. arborescens* is the only species that equals or approaches it in dimensions. *F. excorticata* has

[1] "The Building Timbers of New Zealand," Transactions and Proceedings of the New Zealand Institute, xviii. p. 37.

[2] For particulars of the latter, see Dr. Bayley Balfour's account of the botany of the island in the Philosophical Transactions of the Royal Society of London, vol. clxviii., 1879.

medium-size flowers, at first of a greenish colour streaked or blotched with purple, and finally of a dull red, with very small, almost black petals. The tube of the calyx is very much constricted immediately above the ovary, and there is a second constriction a little higher up. Moreover, the flowers are trimorphic in the relative lengths of the style and stamens. That this *Fuchsia* yields "one of the strongest and most durable timbers in the colony" will be news to most people. But, as the trunk is often crooked or gnarled, it is difficult to procure logs exceeding 8 or 9 feet in length, and its commercial value is therefore greatly diminished. Mr. Kirk says the wood is hard, dense, compact, and even, and deep brown in colour, relieved by streaks of a paler shade, and short narrow waved black markings. When much waved, it is of a highly ornamental character. Further, it is almost indestructible even by fire, except in a closed furnace.

Many more interesting facts might be extracted from this admirable book, the botany of which appears to be equally as good as the practical part. A few new species are described, and the female flower of *Podocarpus totara* and the male catkin of *Dacrydium cupressinum* are figured and described for the first time.

One more point deserves mentioning. Mr. Kirk is very much concerned about the many inappropriate popular colonial names, which he proposes to reform; but we think he has undertaken a task in which he must inevitably fail. From the time of the earliest settlements, the various species of native beech (*Fagus*) have been called red birch, white birch, &c., though not uniformly throughout the colony; and the Maori language has only one common name for all the species. Now, Mr. Kirk proposes calling them "entire-leaved beech," "tooth-leaved beech," and so on. Supposing it were possible to effect a reform in this direction, the substitution of such uncouth names as those proposed is less to be desired than the retention of their present botanically inaccurate appellations. W. BOTTING HEMSLEY.

AN ELEMENTARY TEXT-BOOK OF CHEMISTRY.

An Elementary Text-book of Chemistry. By William G. Mixter. Second and Revised Edition. (London: Macmillan and Co., 1889.)

THIS volume belongs to the well-known series of "Manuals for Students," and will pleasantly surprise those who imagine that the multitude of elementary text-books of chemistry has made originality impossible. Of those smaller matters that readers and students generally accept with no more thought than their daily mercies, the index deserves especial mention as being exceptionally inclusive, and bearing evident signs of having been compiled by someone who had a sufficient knowledge of the subject to go beyond the mere verbiage, and enough patience to carry the task through.

Looking at the book simply as a treatise on chemistry, the adoption of the periodic system as a method of classification is very desirable, but the difficulty as to which of its elements a compound shall be associated with is not easily settled. The tendency is doubtless to keep the compounds of any one metal together, and this is what the author has done as far as possible, but as, for example, nine metals are treated of before oxygen and sulphur, and thirty-eight metals before carbon, the compounds of those metals that are considered in the earlier pages are spread through the book in a manner that is doubly confusing, the interpolated sections seriously interfering with the general arrangement. The sulphates of sodium, for instance, cannot be considered with sodium itself before sulphur has been mentioned, nor can they be added to sulphuric acid, as that would disturb the natural sequence of sulphur, selenium, and tellurium compounds, and the best place remaining for them is in the middle of one of the groups of elements. As a natural result of this apparently unavoidable irregularity, such a common salt as manganese sulphate finds no place at all, and it cannot be supposed that it is intentionally omitted when manganese tetrafluoride, hydrogen auryl sulphate, gold sulphate, and other equally rare compounds have considerable space allotted to them.

There is another marked innovation that certainly deserves success—namely, the introduction of gravimetrical quantitative experiments. The very first experiment in the division of the book headed "Chemistry" is the preparation of hydrogen by the action of sodium upon water, and the student is directed, after weighing the sodium and measuring the gas, to calculate the relative weights of the two, making all due corrections. In the next experiment the proportion between the weights of equivalent amounts of zinc and hydrogen is determined.

If the volume before us were simply an experiment in text-books, we might leave off here by congratulating the author on the measure of success that he has realized; but his first words in the preface are, "This work is designed for use in schools and colleges." The tendency at the present time is not merely to introduce the study of chemistry, but to extend the general scope of education in many other ways, so that the time devoted to chemistry has been very much reduced in some cases to allow of the addition of other subjects to the student's carriculum. It would be difficult to find a student who would have time to work through 238 experiments in elementary chemistry, especially when many of them are in reality a combination of three or four; and besides the specific experiments enumerated there are suggestions in the text that the student should prepare certain compounds and verify their properties. We do not hesitate to say that the intelligent performance of a fifth of the number of experiments set down to be done, accompanied with suitable study, would give the student as serviceable a knowledge of the subject as if he went through the mass of practical work prescribed.

The author also says in his preface that "Graphic and constitutional formulas are much used," and the body of the book fully bears out the statement. Graphic formulæ have been perseveringly tried in this country as an aid to the elementary student of inorganic chemistry, and have been deliberately discarded as uselessly cumbrous, and as making what is difficult to the beginner more difficult still. Such formulæ, however, are occasionally useful; but we would ask what good end is served by setting down the graphic formula of $Na_4W_3O_{11}$, which is given at p. 190? The best formula is that which most simply expresses the properties of a compound, but it would be difficult to say what properties are indicated by such formulæ as that referred to.

There are a few statements on important subjects that at the present time concern no one but the historian. For example, the classification of salts as normal, acid, or basic, according to the proportion between "the bonds of the acid radical" and "the bonds of the basic radical," was never generally accepted, and is now universally allowed to be an altogether faulty method. The extraordinary prominence given to hydrogen dioxide or "free hydroxyl," which has four pages devoted to it, while sulphuric acid has little more than two, is scarcely justifiable, we think, at the present time. But on the whole, the book is a useful compendium of the principal properties of not only the more important substances but also of many of the compounds of the rarer elements.

OUR BOOK SHELF.

A Treatise on Geometrical Conics. By A. Cockshott and Rev. F. B. Walters. (London: Macmillan and Co., 1889.)

THIS work is not intended to supersede such works as Besant and Taylor, which, being drawn up for University students, naturally cover a good extent of ground, but to meet what is a pressing need in school teaching. The need of some recognized sequence of propositions, as our authors state, has long been admitted. It was with a view to meet this need, as we have previously stated in this journal, that the Association for the Improvement of Geometrical Teaching published its syllabus of the subject, which had been accepted by the Association at its annual meeting in January 1884. The work before us has been drawn up in accordance with the syllabus, the authors' aim being to invest the skeleton of the syllabus with suitable raiment. A main feature of the outline was the prominence given to Adams's property (which boys will call Adam's property), the S U K I (now changed rightly, as O is used for the external point, to the S U O I) proposition. In the parabola, we are told we may employ the property in proving tangential propositions; in the case of the ellipse and hyperbola, the authors use Adams's, and also give two other constructions. But this is a matter of detail. The proofs are neat and well suited to beginners. A capital feature is the appending quite elementary riders to the respective propositions, these not being too difficult; and in most cases, being true riders on the propositions they follow, they will encourage the young student to prosecute a study which becomes very fascinating when once the student gets a grasp of it. A short chapter on orthogonal projection follows that on the parabola, and is likely to be of use as showing the intimate connection which exists between the circle and the ellipse. A large collection of Cambridge problems, duly labelled, closes the work.

We have waited long for this quasi-authorized edition of the Association's syllabus, "thereby hangs a tale," and now it has reached us we are not disappointed. There are very numerous figures, many of which are excellent, but others are like Pharaoh's lean kine, "very ill-favoured."

Phormium tenax as a Fibrous Plant. Edited by Sir James Hector, K.C.M.G., M.D., F.R.S., &c. With Plates. Second Edition. (Wellington, New Zealand: By authority, George Didsbury, Government Printer, 1889.)

THE original edition of this little hand-book appeared in 1872, since which period a great deal of consideration has been given in this country to the further development of vegetable fibres generally, amongst which New Zealand flax or hemp has had its share. The book has such a varied amount of authentic information on the subject with which it treats that the appearance of a new edition is a distinct gain to those—and they are many—who are occupied at present in the investigation of vegetable fibres.

The description of the *Phormium tenax*, its habit and rate of growth, cultivation, transplanting, and propagation, with an account of the native and European methods of preparing the fibre, are all brought together here in a compact form. The reports, prepared in New Zealand by Messrs. Skey, Nottidge, and Hutton, together with those of Profs. W. R. MacNab and A. H. Church, prepared in this country, are also very valuable. These latter appear in full in the new edition, and the former are in some particulars more detailed. The book is, however, almost a reprint of that which appeared in 1872, in some cases even to the reproduction of errors; thus on p. 2 of both the old and new editions the Raupo, *Typha angustifolia*, is printed *Typhus*.

The most interesting part, at the present time, of Sir James Hector's new issue is, in consequence of its being the newest matter, the preface, from which we learn that during the last two years the demand for *Phormium* fibre has been steadily on the increase, and that one important application is for the production of twine for use in the harvesting machine, it having been found that as a substitute for wire in reapers and binders no fibre is equal to it.

Revision of the British Actiniæ. By Prof. A. C. Haddon. Part I. (London: Williams and Norgate, 1889.)

THIS revision of the British sea anemones by Prof. Haddon, will be welcomed by all students of this interesting group. We know a good deal already of our native species, thanks to the writings of Sir J. Dalzell, Dr. George Johnston, and P. H. Gosse; and the last-mentioned author, in his well-known "History of the British Sea Anemones and Corals" (1858), succeeded, by the aid of chromo-lithography, in giving very fair representations in colours of the living forms. But the "Report on the Actiniaria of the *Challenger*," by Richard Hertwig, in which he sought by anatomical investigations to establish a scientific classification of the group, opened up a new standpoint for the study of these forms, of which Prof. Haddon has most wisely and energetically availed himself; and in this first part of his revision we have a most excellent monograph of the *Chondractininæ*, and studies of several genera, which may be regarded as more or less representing the various stages in the evolution of the typical hexamerous Actiniæ. These latter belong to the families *Edwardsidæ* and *Halcampidæ*. There is also a description of the remarkable *Gonactinia prolifera*, Sars, some notes on *Zoantheæ* and on the development of Actiniæ.

Chitonactis marioni, *Paraphellia expansa*, *Edwardsia tecta*, and *Halcampa arenarea*, are described and figured as new species.

Seven plates accompany this memoir, of which the first two, representing *Chitonactis marioni*, n. sp., *Gephyra dohrnii* (von Koch), *Actinange richardi* (Marion), *Paraphellia expansa* (g. et sp. nn.), *Halcampa arenarea* (n. sp), *Chondractinia digitata* (Müller), and an undescribed species of *Sagartia*, are very beautifully printed in colours, being perhaps the most life-like illustrations of Actiniaria as yet published in the Transactions of any of our learned Societies.

This memoir forms Part V. of the fourth volume of the Royal Dublin Society's Transactions.

Practical Iron-Founding. By the Author of "Pattern-Making," &c. (London: Whittaker and Co., 1889.)

THIS little volume is an attempt to give, in a condensed form, an account of the principles and practice of iron-founding. To begin with, iron-founding is an art most difficult for the non-professional man to understand, even when going through a foundry, where the various branches of the work are going on before his eyes. How much more difficult it must be for a student to get much real knowledge of the art from a book it is easy to imagine.

As an elementary hand-book this volume will, no doubt, serve its purpose. At the same time, it ought to be clearly understood that the iron-foundry is the only place where iron-founding can be learned thoroughly. A little idea of the art may be obtained by other means, but moulding, of all the engineer's arts, is the one which requires the practical work in an engineer's foundry for its development. The machine tool is largely to blame for the deterioration of our skilled workmen generally, but this has been least felt in the foundry. The moulder must still have his trade between his fingers to be efficient, and no amount of machinery as at present designed will help him to mould, say, a pair of locomotive cylinders in one casting. The book is carefully written, and represents good all-round practice as far as it goes. The illustrations of tools, &c., are clear and accurate. N. J. L.

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.*]

Ice Blocks on a Moraine.

BLOCKS of ice, so far as I know and so far as I remember to have read, are not usual constituents of a moraine. So it may be well to call attention to an instance which I saw lately when walking over the Gorner Glacier with my friend Mr. J. Eccles, who is even more familiar than I am with glaciers, and to whom the sight was novel. At the base of Monte Rosa, where it begins to rise from the Gorner Glacier, are two buttresses of ice-worn rock; the northern called *Ob dem See*, the southern *Auf der Platte*. Between these a glacier, evidently of no great thickness, descends towards the west, and adjacent to each, rather on the in-side, as it may be called, is a little lake. In the northern of these (called the Gorner See, and the only one some five-and-twenty years ago, if I remember rightly) several blocks of ice are now floating; not far from it are the blocks on the moraine.

To explain how they attained their present position, in some cases more than a hundred yards from the water, and probably quite twenty above it, a little more topographical description is needed. The moraine of which they form a part is not a ridge composed of, or at least masked by stones, but a very gentle swell of ice, over which, especially on the eastern side, blocks are scattered in open order. It extends from one lakelet to the other, and is produced as follows. As said above, the glacier which passes between these rock-buttresses is by no means a thick one, but the southern flank of Auf der Platte is swept by a huge ice-stream which descends from the snow-fields of the Lys-joch, and is prevented from much lateral expansion by the pressure of a second large glacier which drains the northern face of the Zwillinge. This enormous mass of ice tends to pond back the smaller glacier; thus the moraine, mentioned above, is mainly formed by the left lateral moraine of the latter, by a few blocks which come down its mid stream, and possibly by the right lateral moraine of the Lys Glacier. The obstructed ice, however, is forced up so as to form a sort of flattened wave, so that if one were coming right down the face of Monte Rosa one would mount 50 or 60 feet from the margin of the Gorner See, or perhaps half as much from the middle part of the Monte Rosa glacier, and then, after a slight descent, would again ascend a gentle slope in order to arrive on the broad united ice-stream which bears the name of the Gorner Glacier.

The blocks of ice are numerous. A few of the largest must contain about 8000 cubic feet—many vary from 2000 to 5000 cubic feet—indeed, in the northern part of the moraine I think the ice exceeds the rock in actual volume. These ice-blocks, in some cases, are mounted on ice-pedestals, just as is rock in a glacier-table; the support rising perhaps a couple of feet above the level of the glacier. Of course they were "perspiring" freely under a July sun, and do not make a long journey; probably few succeeding in getting a furlong away from their source.

That these blocks of ice began as bergs in the Gorner See is indubitable. They have been elevated to their present position by the strugggle between the confluent ice-streams; the smaller of these impinging upon the larger almost at right angles, and being thus forced upwards by the obstacle. The number of the blocks suggests the possibility that the glacier itself may form part of the bed of the Gorner See; for they would be more readily removed from the water, if the actual bed of the lakelet, instead of being at rest, were slowly travelling forward and upward.

The above description illustrates the way in which (as I have seen suggested) blocks of rock in past geological ages may sometimes have been carried up-hill by glaciers. At the same time I may observe that I should myself be reluctant to found upon it any very sweeping generalization. T. G. BONNEY.

The Inheritance of Injuries.

IN the notice of Dr. Weismann's "Ueber die Hypothese einer Vererbung von Verletzungen" (NATURE, July 25, p. 303) there occurs the following commentary:—It is not so certain that all will admit Weismann's contention that the demolition of the inheritance of injuries furnishes strong presumptive evidence that acquired characters are not inherited. *It might well be urged that there is a great distinction between characters which are obviously not useful (such as injuries) and useful characters.*"

I have italicized the last sentence, desiring to call the attention of those interested in the subject to some points of difference between useful and not useful or disabling variations, as these may be supposed to lend themselves to transmission by inheritance. The appreciation of these points of difference is calculated, I believe, to greatly assist in settling the important question as to the inheritance of acquired characters.

In my work on "Dissolution and Evolution and the Science of Medicine" (Longmans and Co.) an attempt is made to show from various considerations that non-congenital diseases, including injuries, are not inheritable. The chief contention is that diseases and injuries are simply disorganizations of pre-existent functions and structures. They are not, as useful and normal characters are, integrated and organized arrangements of the organism's energies, but bodily disintegrations inseparable from the actions of the environment. Diseases as dissimilar as a common burn and general paralysis of the insane, are shown, in the work I speak of, to be alike in so far that they are disintegrations of the body and causally related to the environment. It is this intrinsic nature of disease and injuries and their dependence on external conditions which goes far, as I believe, to make them uninheritable. Since my work is probably accessible to few of the readers of NATURE, I may perhaps be permitted to quote the following extracts as further argument and illustration.

"True diseases, as we have just seen, cannot be separated from their causes; and causes, being of the environment, are not handed down. But there are additional reasons for the feeble hold which heredity has upon pathological states. When we discriminate between the variations of function and structure that are passed on by parent to offspring and those that are not, we are forced to see that natural selection, working always in confederation with heredity, seizes upon *favourable* variations. Natural selection appropriates organismal acquisitions. But analysis discloses the fact that diseases are losses, not gains; are unfavourable variations, and offer no 'purchase' for the co-operative influence of these two modes of action. . . . But more important than influences of this sort is that influence which springs from the differences of nature and conditions between normal and abnormal traits. Normal structures were evolved in long periods of time, and have been transmitted through generations unnumbered; therefore, the tendency to their perpetuation by inheritance must be immensely predominant over any tendency to the perpetuation by inheritance of the transitory changes of disease. I believe that the 'vestiges' of once useful structures owe their astonishing persistence to the fact that they have become deeply pressed into the organic arrangement by the selection and transmission of such structures for secular periods. This makes intelligible the rarity with which deprivation of a limb or other part leaves any impress upon offspring. Though circumcision has been practised among the Jews for ages, it has not produced congenital preputial imperfection in the race.

Nor do we ever find that amputation of a limb, or loss of the cortex of the kidney from Bright's disease, is followed by corresponding anatomical deficiencies in children. In the African transported to northern latitudes, the dark skin persists through indeterminable generations—provided there is no cross-breeding—but the endemic diseases of his race are not transported with him.

"Hitherto all reasoning upon the heritableness of diseases has proceeded on the tacit assumption that morbid changes are subject to the same laws of vital action as healthy changes. It has been discovered, however, that the two are dissimilar both in nature and in the circumstances of their genesis. The traits we every day recognize as inherited are the results of an infinity of co-ordinate actions. There may be instanced the bony framework of the face, the colour of the iris, the gait, special mental aptitudes. All these, and attributes of the same order, represent a vast integration of forces, groups of organized energies. It is this organization which gives them individuality and makes their hereditary transmission possible. They are, in other words, self-existent, having been independent of the original conditions out of which they grew."

The conclusion, deduced from evolutionary principles, that non-congenital diseases and injuries are not inheritable, might, I think, be supported inductively from the facts of medical observation, and it is most interesting that the results of Dr. Weismann's investigations are confirmatory. But from what has been said it clearly appears, in harmony with the annotation I commenced by quoting, that the non-inheritance of injuries is no evidence of the non-inheritance of acquired useful variations. C. PITFIELD MITCHELL.

24 De Vere Gardens, W.

Classified Cataloguing.

WHENEVER a collection has been catalogued anew, and all the numbers are in the museum order of the specimens, the placing of additions at the end, without any sequence but that of acquisition, always seems a melancholy collapse of the order just established. So strongly is this felt that some curators even enter additions with the same numbers as similar specimens, distinguished by letters, as 3247*a*; but, as formerly in the British Museum, this system breaks down when such additions far outnumber the original series, and we reach figures like 3247*fy*. At the same time this is an approach to an entirely different and logical system of cataloguing, which ought to be considered. Another stage of arrangement has been by appropriating so many thousand numbers to each branch, so that the articles of one class may have contiguous numbers.

The complete system of cataloguing which has been thus felt after, and sought for, is what may be called "fractional cataloguing," treating all numbers as decimal fractions and arranging them accordingly. Thus 21·765, 21·77, 21·8, and 22 might appear as successive numbers in a catalogue: the numbers being arranged solely by their successive order of the left-hand figures, regardless of the length of the number. By this system, therefore, any quantity of additions can be brought into their right order without disturbance; fifty new specimens like No. 371, for instance, being numbered 371·01 to 371·50.

The first two or three places of the number will therefore indicate the nature of the specimen in any given catalogue: and this leads at once to the desirability of all collections having a similar numerical basis for their catalogues, so that if all the parrots, for instance, begin with 56 in one collection they should do so in all other museums.

The first step therefore in classified cataloguing would be to agree on a set of 100 or 1000 numbers, to subdivide each branch of science, the distribution of the numbers being partly settled by the average number of specimens, partly by natural divisions. Thus in mineralogy, elements might be 001 to 099; binary compounds 100 to 299; silicates 300 to 799; non-metallic acid salts 800 to 899; metallic acid salts 900 to 999. In all museums, then, silicates, say of lime, magnesia, and alumina, would begin with 61, the different species being marked 610 to 619, and varieties and individual specimens numbered with additional decimals following these bases, *e.g.* 615·47. The set of numbers in each science would be best fixed by a committee at some International Congress, so as to insure general acceptance, like the scheme of geological colouring.

The disadvantages of this system would be—(1) that the catalogue would have to be kept like that of a library, subject to additions at any point, and therefore on slips which could be transferred; and (2) that the total number of specimens would not be known except by counting. These are not serious difficulties, and the following advantages seem to entirely out-weigh them.

(1) The numbers would indicate to all students the nature of the specimens quoted in any collection. (2) The catalogue would be classified in natural order throughout, so that all similar specimens would be described together. (3) The numbers in the museums would be in order from end to end. (4) Any specimens moved could be rearranged by unskilled assistance, solely by the numbers. (5) Any object in the catalogue or hand-books could be at once found in the museum by its number. (6) A great help would be given to the arrangement of minor museums by having a uniform scheme of cataloguing fixed. (7) The numbers being in constant use would soon form technical symbols for species, a short-hand briefer than chemical symbols even, and applied to all sciences; and also a valuable key to the memory.

Bromley, Kent. W. M. FLINDERS PETRIE.

Head Measurements of Students at the University of Cambridge.

I WAS rather too precipitate when I stated that the figures relied on by Mr. Galton were totally inadequate to support his conclusions; for, as regards the second of them, viz. that the "high honour" man has a head perceptibly larger than the "poll" man, the evidence is fairly strong; but with regard to the other three conclusions, referring to the growth of heads, I must repeat what I have said. In the light of the discussion given below of a large number of observations, I cannot even admit that the tables and curves given in Mr. Galton's paper (see NATURE, vol. xxxviii. p. 15) give even "an approximately true idea of what we should find, if we had the opportunity of discussing a much larger number of observations."

Having heard that all the measurements taken have been indexed for reference, I went to the laboratory, and, by the kind permission of the custodian, copied out the head measurements of fifteen individuals, each of whom had been measured at least five times. In one case, measurements had been taken at seventeen times. The average number was 7·1.

Since the first case quoted in my last letter forms one of these, I had better point out that Mr. Galton's objection to it is unsound. He notes a grouping of the observations, which makes him suspect that "some peculiarity in the shape of the head caused doubt as to the exact line of maximum height." But the observations of height are 5·2, 5·3, 5·4, 5·5, 5·5, and 5·6; and show no grouping. Mr. Galton must have meant that the calculated products were grouped. This is the case, but could not be due to the cause he suspects, for that would cause grouping in the simple heights.

The fifteen series of measurements fully bear out the conclusions which I drew before from two only. The measurements of width vary 0·1, 0·2, or 0·3 inch, those of length vary to the same extent, and in one instance up to 0·4 inch, while the height in most cases varies 0·4 or 0·5 inch. In only two cases does it vary so little as 0·2 inch, while in one case it varies 0·7 inch. The last case (where the figures are 5·4, 5·6, 5·1, 5·8, 5·5) is partly accounted for by the fact that the first three observations were taken by one observer, the other two by a second. (The statement in Mr. Galton's original paper that all the measurements were taken by one observer, must have been due to misinformation.) I have calculated the probable error of each observation of the height of head for each series of observations, using the approximate formula

$$r = \frac{0{\cdot}845\Sigma v}{\sqrt{n(n-1)}},$$

and I find it on the average 0·095 inch. Since the average height is less than 5·5 inches, this error amounts to 1·7 per cent. If the error in length and width were each half of this, the probable error of the product would be about 2 per cent.

To test whether any of the variation found is due to actual growth, and not to accidental error, I have used the following method. Arrange all the measurements of any one individual in the order of the dates on which they were taken, and separate them into two equal groups. Take the mean measurements of the first set, and put opposite them the mean of the dates; then do the same with the second set. On subtracting these we can see at once whether the head has grown in any direction.

As an example, G. F. R., who was measured sixteen times, gave the following mean values:—

Date.	Width.	Length.	Height.
September 1886	5·91 ...	7·82 ...	5·74
October 1888	5·94 ..	7·81 ...	5·60
	+0·03 ...	−0·01 ...	−0·14

The differences show the alterations occurring in twenty-five months.

I have calculated the other fourteen cases in the same way, and find that if the growth in width of the whole fifteen be added together, and the same be done with the length and height, the totals are only

+0·24 inch in width,
−0·56 inch in length,
−0·82 inch in height.

The average period during which these changes took place was thirteen months.

These 107 measurements which I have discussed therefore show a small *diminution* in the head capacity as the individual grows one year older, but this is so small compared with the probable error, that the observations are quite consistent with the hypothesis that the head remains quite stationary.

Ten measurements of the height of one individual, taken at successive years from twenty to thirty, would give us a better idea of the normal growth during that period, than many times that number if each were taken on separate individuals, for the range of individual variation at any age far surpasses the amount of growth in any one of them. For the same reason I think that the above 107 measurements made on fifteen individuals furnish a surer answer to the question, "Does the head of a University student grow after the age of nineteen?" than ten times the number made indiscriminately, and simply grouped by age.

F. M. T.

Trinity College, Cambridge, August 3.

The Supposed Connection between Distant Earthquake Shocks.

It is very commonly assumed, whenever seismic disturbances at different localities occur synchronously, that, however remotely they may be situated, there is necessarily a connection of some kind between the shocks, originating in a common cause. A forcible illustration of this fallacy, as I think, having recently appeared in your columns, I beg to take this opportunity of questioning the position so generally adopted, and of testing the validity of the involved theory of earthquake causation by the light of the evidence furnished in the concrete case which has been presented in support of it.

On April 18 a somewhat remarkable earthquake took place in Japan, at about the same time that some rather considerable shocks were registered at two seismic stations in Germany. These occurrences were ably treated from the point of view in question in Nature of July 25 (p. 294), by Dr. E. von Rebeur-Paschwitz, of Potsdam, and argued to a final issue.

It is taken for granted, primarily, under this hypothesis, that every minor earthquake-shock at any given point is the effect, direct or indirect, of some more violent disturbance elsewhere, near or distant; and it is further supposed that the latter is, in its turn, a more direct result of a volcanic outburst at some special centre of activity, known or unknown. From this premise it must be inferred that no micro-seismic shock can ever take place otherwise than as a throw-off from some violent disturbance more or less remotely located. When we examine this conclusion, however, and consider more broadly the concomitant relations of earthquake phenomena, we shall, I think, find that it possesses no foundation whatever in facts, and that the hypothesis does not bear the strain of such evidence as may be adduced from recent observation. We know, for instance, (1) that seismic activity is an experience of almost daily frequency in some localities, as in Japan, obviously without relation to the volcanic eruptions which occasionally take place in near proximity; (2) that, on the other hand, volcanic explosions sometimes occur unaccompanied by any widespread upheaval or distant undulations; and (3) that the earth's crust is, moreover, subject to an apparently constant state of slight vibrations or micro-seismic palpitation, in parts of the globe where perceptible shocks are less commonly experienced.

In the light of these facts the theory under discussion might be readily disposed of as a foregone conclusion, but it will be more satisfactory to prove its fallacy by means of the evidence afforded by the earthquakes recorded in these pages as having taken place at Tokio and in Germany on April 18, especially as the data have been so completely recorded by Dr. von Rebeur-Paschwitz. After considering the details registered by the seismometers stationed at Potsdam and at Wilhelmshaven (distant about 220 English miles), he has applied himself to the problem of making these facts fit in with the record of a shock which was experienced at Tokio at nearly the same time. It is to be remarked that the only source of information respecting the latter shock is the note which appeared in Nature of June 13 (p. 162), in which the information that a violent volcanic eruption was taking place at Vries Island, "possibly 60 miles off," is repeated from a note on the previous page where the island is spoken of under its local name, Oshima. Dr. von Rebeur-Paschwitz too hastily accepts the connection suggested, and upon this slender basis rests his whole argument.

Now it would certainly happen that any earthquake transmitting its effects to a locality on the opposite side of the earth would affect also, in varying degrees, all the countries along the line travelled by the seismic wave; and that a constant diminution in its force would be a characteristic feature of such a phenomenon. But what is the actual evidence in the case in point? We are told that at Tokio the maximum oscillation registered was no more than about 17 mm., while at Potsdam the greatest amplitude of oscillation is stated to have been 154 mm. The seismometers in use may possibly be different in type, but there is surely not such great difference in construction as to cause the complete reversal of the readings, as would be required also by the accepted theory; it may rather be accepted, no doubts being entertained, that the resultant figures may be fitly compared. But the conclusion arrived at is at once seen to be absurd, for the lesser shock at Tokio could not possibly produce the ninefold greater one in Germany; while no mention whatever is made of any intermediate effects being produced along the path of the wave. It is, moreover, especially stated (p. 163) that this particular shock at Tokio was by no means a strong one; its "peculiarity lies, not in its violence, but in the extreme slowness of its oscillations." Yet it is calculated by Dr. von Rebeur-Paschwitz that the mean velocity of transmission of the wave (occupying 1h. 4·3m.) was "2142 metres of propagation," which is really 167 metres above the mean of the different rates computed by the two authorities cited.

Again, the volcanic eruption of Oshima or Vries Island, at about 60 miles distance from the main island, which is said to have destroyed upwards of 300 houses, and killed 170 persons, took place, *not* on the 18th, but on the 13th and 14th of April (cf. pp. 162 and 179, where fuller details are given). Besides this difference in the dates, no exceptional earthquake shocks are reported to have occurred at Tokio during the eruption, and I fail to see the possibility of any connection between the two particular phenomena considered.

If there were any direct seismic connection between Japan and Germany, the latter country would surely be subject to regular transmissions of the shocks which prevail at Tokio. The ample data furnished suffice to prove that it is not the case. We are informed, in a postscript to the letter under consideration (p. 295), that several other shocks were recorded at both Potsdam and Wilhelmshaven about this period, viz. on April 5, 8, 15, 28, and May 31; at Potsdam *alone* on April 25, May 21, 25, and 26; and at Wilhelmshaven two shocks were felt on May 30, "probably connected with the English earthquake of this day." These various shocks were not experienced equally by the instruments at the two stations, and they do not even seem to have been necessarily connected throughout, for we are told that on May 30 there was "perfect steadiness at Potsdam," while four of the shocks mentioned were not felt at Wilhelmshaven.

It is curious to note that no connection is supposed in respect of other earthquakes, and of volcanic activity in nearer proximity in Europe, although several such occurrences have happened during the same period. Thus, on April 26, several shocks were experienced at Schaffhausen, &c. (Nature, vol. xl. p. 84); on April 27, a "severe" one, lasting 4 seconds, was felt at Agram (*ibid.* p. 45), a district much subject to seismic disturbance; on April 30, there was one along the coast of Norway (p. 133); and on May 8, a "severe" one, lasting 3 seconds, took place at Plevje, in Bosnia (p. 84). From Dr. Johnston-Lavis we further learn that Vesuvius was in a state of active eruption during the end of April and the beginning of May (p. 34), but no relation is suggested even in this case, and it is quite evident that the facts do not serve to prove the connection assumed.

I do not wish to assert that in no case are synchronous earthquakes related, for we have undoubted evidence that certain shocks have been very *widespread* from a single centre (as the great Lisbon earthquake in 1755); but it is my belief that nearly every earthquake, whether large or small, is due to strictly local stratigraphical causes, quite irrespective of volcanic agency. Having made many fruitless attempts myself to co-ordinate different well-authenticated shocks, I have been compelled to disbelieve the theory of their general connection, and have now adduced sufficient evidence to show that the verdict pronounced by Dr. von Rebeur-Paschwitz, that "we may therefore safely conclude that the disturbances noticed in Germany were really due to the volcanic action which caused the earthquakes of Tokio," is not proven, and fallacious.

4 Mecklenburgh Square, W.C. WILLIAM WHITE.

The Sources of Nitrogen in Vegetation.

THE discrepancy between the results obtained in the experiments on plants grown in free air instead of closed vessels appears to be general, as stated in NATURE (p. 333). Some years ago I ventured to suggest an explanation of this, based on an observation made in the course of some investigations of atmospheric dust particles.

Sheets of paper, smeared with adhesive coatings, and shallow vessels of water, were laid on the ground in a garden near Willesden. The resulting catch included far more organic than inorganic matter, the organic matter consisting chiefly of small insects. The amount of these was surprisingly great towards the end of summer or beginning of autumn, quite sufficient, I think, to account for the varying results obtained by Sir J. B. Lawes and Prof. J. H. Gilbert, especially for the "eccentric" behaviour of the Leguminosæ—"sometimes the plants died of nitrogen hunger; sometimes, after a time of such hunger, they recovered and produced abundant growth." The explanation may be confirmed or refuted by exposing an unplanted layer of sterilized sand, or other soil, of the same area as that on which the experimental plants are growing, and comparing the gain of combined nitrogen in both cases. W. MATTIEU WILLIAMS.

The Grange, Neasden, August 16.

Do Cats Count?

AFTER reading all the accounts of the interesting experiments lately performed on the famous "Sally," I am persuaded the following incident may not be without a certain interest to some readers of NATURE.

About two weeks ago, the cat of a dairyman in this neighbourhood gave birth to three kittens. Next day, one of them was removed, during the mother's absence, and drowned. On returning from a foraging expedition, and discovering her loss, puss immediately set out in search, presumably, of the missing one. All her efforts in this direction, of course, proved fruitless; but, evidently determined to at least make up the right number, she did so, curiously enough, by carrying off, from its nest close by, a young hare, not more than a week old. This she is at present suckling side by side with her own kittens. In view of these facts the above question very naturally suggests itself.

Winchburgh, N.B., August 15. J. T. WALKER.

Anapophyses.

MY attention has been called to a statement by Prof. Cope (NATURE, July 25, p. 298), that anapophyses are "wanting from the vertebræ of anthropoid apes and man." He probably means that they are very feebly developed, which is true. I have found them, however, to exist distinctly in *Troglodytes* and *Simia* from the eleventh dorsal to the second lumbar vertebra, and in *Hylobates* from the tenth to the fifteenth trunk vertebra, and sometimes beginning as high up as the third dorsal vertebra.

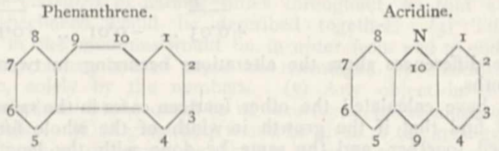
ST. GEORGE MIVART.

Hurstcote, Chilworth, August 12.

The International Chemical Congress—A Correction.

A MISPRINT has occurred in the notation adopted by the Chemical Congress for phenanthrene. The Secretary of the Nomenclature Section has also informed me that a slight modification has been made on the minutes, with regard to acridine, to make the notation adopted correspond more closely to that of the other "multi-ring" compounds. The two formulæ should be—

Phenanthrene. Acridine.



YOUR CORRESPONDENT.

THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE eighteenth meeting of the French Association for the Advancement of Science opened under the happiest auspices. The magnificent Exhibition on the Champ de Mars and the Esplanade des Invalides, which has attracted to Paris an exceptionally large number of foreign men of science, is in itself the most conclusive proof of French energy and the progress of French science during the last decade. It is, perhaps, as M. de Lacaze Duthiers remarked, a more effective declaration of a sincere desire for peace than the utterances of certain diplomatists. But there is no place for politics in the principles of the French Association. Foreigners of all nations have been welcomed to Paris not only with warm-hearted hospitality, but with an artistic splendour of which they may well be envious for their own countries, and to which we shall allude later.

We may mention the presence of Messrs. J. H. Gladstone, F.R.S, R. McLachlan, F.R.S., Catalan, Candèze, Istrati, Berlinerblau, Orloff, Stephanos, Van Beneden, General Wauwermans, MM. Szabo, Valdemar Schmidt, Alexeieff, Bierens de Haan, Coleman, Colley, Franchimont, William Watson, Egoroff, Joukovsky, R. de Luna, A. Macfarlane, Ragona, Thomas Wilson, Benedikt, Hugo Gyldèn, Packard, Retzius, de Selys-Longchamps, Angström, Brusina, Flavitzky, Graebe, Dufour, Timiriazeff, von Goldschmidt.

The first meeting took place on Thursday, August 8, in the large hall of the Palais des Sociétés Savantes, which has just been erected by private enterprise in the quaint little rue Serpente which now affords a home to so many of the learned Societies of Paris.

The President of the Association for the year is the well-known Professor of Zoology at the Sorbonne, M. de Lacaze Duthiers, who chose as the subject of his inaugural address, "The Development of Zoological Method."

M. de Lacaze Duthiers began his address by thanking the Municipality of Paris for their invitation and for their generous subvention of £1200 recently voted towards the year's expenses. He then made a touching allusion to the foundation of the Association.

"It was one day in July 1871, that Wurtz, whose geniality and whose kindly vivacity you have not forgotten, took me, in his friendly way, by the arm, and said to me as we left the Academy of Sciences, 'Come to my house to-morrow night; I want to speak to some of our colleagues of a plan which I much wish to succeed.' On the Tuesday we met at M. Wurtz's house, just a few friends, MM. Delaunay, Claude Bernard, Decaisne, and myself. One may say that this was the first meeting of the French Association.

"As the only survivor of those who were our masters and our friends, I could not forbear from speaking to you of the intimate conversation in which our Association originated. I still seem to see and hear Wurtz with his kindling enthusiasm, with an activity at times feverish, but always good-hearted, striding across his drawing-room and giving us a picture of what our Society should, according to his idea, be—of what it has since become."[1]

M. de Lacaze Duthiers then proceeded to describe the state of zoology a hundred years ago.

"When 1789 came, Linnæus and Buffon had just died, and their fame was at its greatest splendour. They were the absolute masters of zoology, and the whole science was summed up in their work. Yet how different were the two in their nature and their work.

"Linnæus, precise, methodical, a classifier above all, introduced clearness and order into the most minute details of natural history, and as he proposed a nomenclature at once simple and precise his influence acquired such a preponderance that von Haller complained of his tyranny. If the reform of scientific language proposed by Linnæus took such immediate and powerful hold on the scientific world, it was because it exactly corresponded to the need of the moment.

"Before Linnæus, the objects of natural history were designated by the help of names or phrases formed by the addition of adjectives describing their characteristics, which overburdened the memory. All this he reduced to two words, just as in our families we distinguish the various members by a family name and a Christian name. The simplicity, the ease of application, and above all the opportune appearance of his nomenclature were the causes of its great success; but we must add that its intrinsic value is such that even to-day we hardly depart from the rules on which it is founded.

"The opposite of Linnæus, Buffon delighted in broad delineations, and his general reflections were animated by a potent inspiration. We are carried away and enthralled by the profundity of his thought, the elevated standpoint from which he views science. By reasoning and the consequences of observations which he interprets, he seeks as much to discover what will take place, and what must have taken place, as to determine the exact nature of what he observes. And thus he was often in advance of his epoch, and his lofty views could be understood only by the few.

"Linnæus, on the contrary, established *facts* with simplicity and clearness.

"With qualities such as these, these two men of genius, who died towards the end of the last century, must often have been at variance; and though the name had not yet been pronounced, we may already at this epoch distinguish between the school of facts and the school of theories (*l'école des faits et l'école du raisonnement*).

"But there was wanting to the works of Linnæus and Buffon a foundation whose necessity made itself imperatively and universally felt. For it was beginning to be perceived that the study of the habits, of the geographical origin, and of the external characters of animals was not sufficient.

"Then came Cuvier. It was the great merit of Cuvier to see clearly that if zoologists would arrive at a truer knowledge of animals, they must not content themselves with a description of their external forms, but must investigate their internal construction. It was thus that he introduced the notion of *anatomy* into natural history."

M. de Lacaze Duthiers proceeded to defend Cuvier's memory from unjust attacks which it had suffered owing to his opposition to Lamarck.

"Let us not judge great men by their weaknesses. By wishing to depreciate them, we often arrive at a contrary result, and lessen ourselves by a display of systematic and unjustifiable hostility. Let us see in them only what is good and great. And then we shall not fail to recognize the fact that Cuvier is entitled to our fullest meed of admiration."

The lecturer continued by pointing out that Geoffroy St. Hilaire and Lamarck occupied the same position with regard to Cuvier that Buffon had held with regard to Linnæus. Their speculations were too advanced, they came before their time.

"Zoology remained stationary for many years after Cuvier, and it was not before the beginning of the century that new ideas sprang up directing research along fresh paths, and profoundly modifying certain branches of biological science.

"It is strange to see how books and observations of considerable value often remain obscure for years, and are then finally brought to light by some unexpected discovery.

"I remember that about the year 1855 Prof. Huxley, the illustrious English *savant*, wrote to me (I was then Professor at Lille): 'In England we are all much excited about the discoveries of M. Boucher de Perthes.' You all remember the interest aroused by the discovery of the chipped silexes of St. Acheul, and the famous jaw of Moulin Quignon.

"English geologists came to Amiens. The most lively discussions took place, and an International Committee was formed to direct an official excavation.

"But perhaps the real discovery made, and this was due chiefly to the journey of the English *savants*, was that of the books, the researches, and the new ideas of Boucher de Perthes, which had hitherto passed almost unnoticed.

"We may say that from this moment dates the study of prehistoric times, which has undergone so immense a development. The excitement produced by the discoveries of M. Boucher de Perthes had begun to calm down, and researches were being pursued in every direction, when the first of Darwin's essays appeared in 1858 and 1859. These dates will remain for ever memorable in the history of natural science, for henceforward zoology (the only science of which I am at present speaking) takes an entirely new direction.

"We must acknowledge that whatever the measure of confidence we have in the theory of transformism, if we admit it to its full extent, with all its consequences, or if we reject it, it can be a matter of doubt to no one that this theory has led to a truly extraordinary scientific activity. Partisans and detractors both—in seeking for proofs of their opinions, either in the mysteries of embryology or in the investigation of the animal remains in geological strata—all, whatever their method, their ideas, their opinions, or their hostility, have caused zoology to take great strides.

"How far removed we are from the time of Linnæus, when external characteristics were everything—from the time of Cuvier, when anatomical notions and the study of exterior forms alone guided the classifier!

"To-day, what we seek for first of all is the enchainment of animals, either by going back to the forms of the past from those of the present, or the converse. We seek to explain the varied forms we see by the laws so happily formulated by Darwin—laws, indeed, which are as true as they are seductive.

"Who can deny the struggle for existence? Is it not to be found everywhere? And who will not admire the effects of selection? But why exaggerate its true bearings?

"Evolution is to be met with everywhere, in the civilized world as in the world of Nature, development is the great and eternal problem, resolved by the destruction of some, by the advancement of others; everywhere the struggle for life shows itself, inflexible and fatal.

"One may be a partisan or not of the theory of evolution—and I have not here to declare my own opinions, but rather to determine the exact influence of certain dis-

[1] It need hardly be said that Wurtz founded the French Association on the model of our British Association. From the war of 1870 dates a remarkable renaissance in French scientific activity, which had been in abeyance during the Empire, or at least till M. Duruy came into power in 1869 as Minister of Public Instruction, and founded that unique institution for the encouragement of research, the École des Hautes Études.

coveries and certain men on the progress of science—but one is forced to bow to facts, and to acknowledge the magnitude of the prodigious impulse given to science by the great English naturalist. But, as Claparède would say, there are *enfants terribles* of evolution—men more anxious to be spoken of than to discover the truth.

"We must carefully distinguish them from the conscientious men of science who investigate precise facts patiently, scrupulously, and laboriously, to deduce from them consequences in support of their theory. Men such as these make science advance surely, whereas the others sometimes compromise the cause.

"The only thing to oppose to exaggeration, the influence of excessive enthusiasm, is recourse to *experiment*. To-day, that is the great reform which we feel to be as necessary as those other reforms of whose history I have spoken."

Before developing this idea, the lecturer spoke of Lamarck, and of the reason why his ideas were not adopted during his lifetime.

"Of late years people have thought they had re-discovered Lamarck, and have severely blamed the generation of zoologists who neglected for half a century the author of the 'Philosophie Zoologique.'

"It is true that in the works of Lamarck there are whole pages which contain the theory of transformism completely developed, to which Darwin has added nothing, and which we may say he has but confirmed.

"But if Lamarck had not the satisfaction during his lifetime of seeing his ideas admitted, it was that the minds of zoologists were not sufficiently prepared for them; it was that he had not the rare good fortune of finding a precise and lucid formula, whose truth is evident, and which is necessarily accepted by all.

"Some of Lamarck's conceptions of Nature are even difficult to understand, and especially to make clear to others; and I do not see that even his most ardent admirers have insisted on this portion of his work, and yet it is by no means inconsiderable.

"We should have been glad to see it more clearly explained than it is in the original, and brought within the comprehension of all, and one wonders why it should never be referred to.

"For a reformer to succeed, his idea must be striking in brilliancy and precision; it must master us by its intrinsic seduction.

"Take Darwin, showing us the struggle for existence taking place everywhere and at every instant, and leading to the selection and survival of the victor.

"Take Cuvier, who, it was said, reconstructed an extinct animal from a fragment of bone. A statement like this appealed to the imagination of the masses, and he excited the admiration of a whole generation when he compared an organism to an equation of which we may determine the unknown by the known quantities it contains.

"If Lamarck did not have the success he deserved, it was because of the abstract form he gave to his opinions, and the often *naïve* proofs which he adduced in confirmation of his theories at a moment when enthusiasm and popular attention were diverted to another aspect of science.

"Lamarck was, moreover, regarded as a visionary because he believed in the possibility of forecasting the weather from observations of the atmosphere and of the forms of clouds, and yet who would now blame him for his hopes?

"Lamarck was a man of genius who foresaw the advances of science on many sides, but who did not possess the faculty of being able to present his ideas in a felicitous form intelligible to his contemporaries."

The lecturer then returned to his theme, that experimental research in zoology is the need of the hour, and proceeded to give some striking examples of the results to which it has led.

Firstly, he described the alternation of generations in the gall-producing insect, whose two forms were originally known as Biorhiza and Teras. The Biorhiza, a wingless and asexual form, is born from eggs laid in the roots of the oak; it crawls up to the branches, and there causes the gall excrescence as it lays its unfertilized eggs. From these eggs issue the sexual winged form Teras, which conjugate; and the female then lays her eggs in the roots of the oak, and from these spring the Biorhizæ.

He then spoke of the strange metamorphoses of a certain insect *Sitaris*, semi-parasitic on a species of bee, *Anthophora*, which have been investigated by M. J. H. Fabre, and of those of the lobster. In all these cases, forms supposed to have been different have proved to belong to the life-history of one and the same animal.

"It is because zoology is at this moment at a critical period, and because of the positive nature of the affirmations made by the partisans of transformism, that the methods of the science must be modified, and that besides simply registering the existence of species, we must have constant recourse to the test of experiment. Such is the conclusion at which we logically arrive, and which to-day, I repeat, has become imperative.

"I have sought to point out to you the considerable part which our country has played in the progress of the natural history of man and of animals during the century which is drawing to a close.

"I should have wished to speak also of the origin and development of other branches of biology, of comparative and general anatomy, of experimental physiology, anthropology, and palæontology. But I think I have said enough to show that I am justified in spurning the reproaches and inimical accusations so often made against us, that France is a country in which scientific work is on the decline, and whose decadence is at hand.

"We open our meeting full of joy in the present, of hope for the future, in presence of the imposing spectacle whose success has but increased since it began in May, and which demonstrates the inanity of these accusations. Let your labours, varied as they are important, prove once more, during a year so fertile in pacific manifestations, that we work only for the restoration of our country, and that the peace of which others speak much, and perhaps believe in but little, is the sole preoccupation of all men of sense, of all who are in earnest in this country of France—France whose desire is to remain free and independent, ever animated by the most generous and patriotic of sentiments."

At the conclusion of M. Lacaze Duthiers' speech, which was most enthusiastically cheered, the Treasurer announced that the receipts for the year had been £3760, the expenses £3480, and that the total capital of the Association amounted to £33,060. An important legacy of about £7000 had been bequeathed to the Association by M. Girard, for the promotion of researches on prehistoric man.

The proceedings concluded with a report on last year's meeting at Oran, and the members then adjourned to the École des Ponts et Chaussées, where the Sectional sittings took place.

The French Association is subdivided into seventeen Sections and sub-Sections: (1 and 2) Mathematics and Astronomy; (3 and 4) Civil and Military Engineering and Navigation; (5) Physics; (6) Chemistry; (7) Meteorology; (8) Geology and Mineralogy; (9) Botany; (10) Zoology, Anatomy, and Physiology; (11) Anthropology; (12) Medical Science; (13) Agriculture; (14) Geography; (15) Political Economy; (16) Pedagogy; (17) Hygiene.

An immense number of papers were contributed to the

Sections. We may mention:—(Sections 1 and 2), M. Joukowski, on an apparatus for determining moments of inertia. (3 and 4) M. Pichou, on a universal paddle-wheel; by altering the direction of the paddles, a steamer may be reversed without reversing the engines. (5) MM. Baille and Féret, on a method for reading the oscillations of the balance with great precision; the authors employ a very simple optical arrangement, which allows them to estimate the 1/2000th part of a milligram by the displacement of a series of Newton's rings. (6) MM. Alexeieff and Werner, on the heat of neutralization of certain aromatic compounds; the authors show that the general results already acquired enable one to determine in certain cases the position of disubstituted compounds. Messrs. Gladstone and Perkin, on the relation between constants of refraction, dispersion, and magnetic rotation in organic compounds. M. Cazeneuve, on camphor derivatives. M. Franchimont, on the action of nitric acid on organic bodies according to their function. M. C. Chabrié, on the synthesis of selenium organic compounds. M. Raoult, on a new form of the apparatus for cryoscopic observations; the author produces cold by the evaporation of carbon disulphide. M. Meunier, on insoluble crystalline compounds of mannite and sorbite, with the aldehydes, serving to isolate these bodies from organic mixtures (the juice of fruit, &c.). M. Berlinerblau, on a sweet body, $NH_2 . CO . NH . C_6H_4 . OC_2H_5$. M. Istrati, on new general methods for the synthesis of aromatic compounds. (7) M. Crova, on the standardization of actinometers. M. D'Abbadie, on the qobar. M. Angot, on the representation of the variation of temperature by a harmonic formula. (8) M. Szabo, on the opal mines of Hungary. M. Malaise, on the Belgian Olethamias. M. Bleicher, on the glacial formation of the Vosges. (9) M. Timiriazeff, on the *rôle* of light-intensity in the assimilation of carbon by plants. M. Mer, on variations of structure in the wood of pines. M. Clos, on certain cases of hybridity in plants, and on the vegetation of *Marchantia polymorpha*. (10) M. de Lacaze Duthiers, on the fusion of nerves and ganglia in Mollusca, and on the interpretation of the membrane which separates them. M. Beauregard, on the Cetacean ear. M. Packard, on the distribution of the organs of taste in insects. M. Sirodot, on the dentition of elephants. M. Jourdain, on the necessity of conjugation for the continued propagation of Protozoa. M. de Varigny, on the action of certain convulsive poisons on *Carcinus mænas*. (11) M. A. Bertillon, on the anthropometric characteristics of the French nation classed by the departments. M. V. Schmidt, on the Stone Age and the prehistoric Iron Age in Denmark. M. de Mortillet, on right- and left-handed individuals in prehistoric times. M. Manouvrier, on anthropometric researches on natives of Algeria. Dr. Maurel, on the relation of the section of the thorax to the height and the length of the foot. (12) M. Massé, an instrument for determining the position of the fissure of Orlando. M. Steinhaus, on the causes of suppuration. M. Jolly, on the use of iodine in tuberculosis. M. Luys, on the pathological anatomy of madness, and on the action of rotating mirrors on the nervous system. M. Michel, on the influence of drinking-water on public health. (13) M. Kunckel, on the destruction of locusts in Algeria. M. Ladureau, on a rapid method of colorimetrical analysis for nitrates. M. Dehérain, on the loss and gain of nitrogen in arable lands and on the graphic representation of crops. (14) M. Castonet des Fossés, on the future of the Negro race. M. Gauthiot, on a route for mining exploration from Bangkok to Korat. The Prince of Monaco, on dragging in deep water. M. de Guerne, on an exploration of the Forno de Graciosa (Azores). (15) M. Arthur Raffalovich, on economic legislation in England in 1888. M. Martineau, on the true motives for the introduction of free trade into England. M. Ch. Grad, on insurance against old age and infirmity in Germany. (16) M. Frederic Passy, on a system of writing which permits blind people to communicate with the non-blind; M. Paul Passy, on spelling reform; M. Herzen (of Lausanne), on the organization of certain secondary schools to enable them to continue the teaching of the primary schools. M. Morel, on the organization of the *enseignement spécial* in the secondary schools of Paris. (17) M. Delthil, on diphtheria and its treatment. M. Teissier, on diphtheria at Lyons. MM. Dubief and Brutel, new experiments on the value of sulphurous acid as a disinfectant.

The work of the Sections was most usefully supplemented by visits to different institutions and to the Exhibition. Sections 1 and 2 inspected the collection of calculating machines at the Conservatoire des Arts et Métiers, under the guidance of M. Ed. Lucas. Sections 3 and 4 examined the new steerable balloons with the inventor, Commandant Renard, while various other parts of the exhibition of the Minister of War were explained by officers of the special departments. Section 6 inspected the fine collection of chemical products at the Champ de Mars (Class 45), general explanations being given by MM. de Clermont, Riche, Suilliot, Billault, Istrati, and others. M. Tanret showed his interesting exhibit, including specimens of pelletierine, the alkaloid of the pomegranate bark now so extensively used as a tænifuge, and ergotine, the active principle of ergot of rye, by whose discovery M. Tanret has rendered such signal service to therapeutics; and last, but not least, M. Grimaux, Professor at the École Polytechnique, gave an account of the fine exhibition of Lavoisier's papers and instruments, which he had selected from the collection of M. de Chazelles, to whom they belong,[1] and pointed out the portrait of M. and Mme. Lavoisier, by David, in the Palais des Beaux Arts. Section 7 met at the Exhibition to see the instruments which record the direction and the vertical and horizontal components of the wind on the top of the Eiffel Tower, the results being transmitted electrically to the Palais des Arts Libéraux, and inscribed on MM. Richard's revolving drums. The Section also met at the Central Meteorological Office to see experiments on cyclones by M. Weyher. Sections 8, 9, and 10 inspected the fine galleries at the Museum, which have just been opened.

Other scientific visits were paid to the Institut Pasteur, the sewers of Paris, and to various glass-works, gas-works, &c. On Thursday, August 8, the Municipality of Paris threw open their magnificent suite of rooms at the Hôtel de Ville to the members of the French Association; while extraordinary animation was lent to the scene by the presence of the students of Paris and their foreign comrades, 8000 guests in all having been invited. On the following evening, the Association was received by M. Yves Guyot, the Minister of Public Works, and Mme. Guyot at the Ministry in the Boulevard St. Germain. A concert was given during the evening by members of the Opéra Comique; while the gardens lighted up by Chinese lanterns afforded a pleasant change from the crowded *salons*.

On Sunday, August 11, an excursion was made to St. Germain-en-Laye, and to Meudon, dinner being served in the orangery which belongs to the beautiful grounds of the Astronomical Observatory, directed by M. Janssen. M. Janssen offered a warm welcome to the members, and said that he should be very willing to let his Observatory be used for the scientific purposes of the Association. On Monday, August 12, a lecture was delivered by General Tcheng-Ki-Tong, on the social economy of China. On Wednesday, August 14, the Association offered a banquet to its foreign members at the Restaurant d'Alsace-Lorraine on the first story of the Tour Eiffel.

M. de Lacaze Duthiers made a short speech, in which he declared that he would not say good-bye to the foreign guests, but *au revoir*, as he hoped to see them all again

[1] M. Grimaux has published this year a remarkable monograph on Lavoisier (Félix Alcan).

at the meeting of the Association next year at Limoges. That they did not forget past events was proved by the name of the restaurant in which they met, but he firmly believed that "*Le temps prime la force*," and he drank "to the peace of the whole world."

MM. van Beneden, Dekterev, Istrati, Hartog, Ramon de Luna, Llaurado, Stephanos, Timiriazeff, and Watson, spoke in the name of their respective countries, and drank to the cause of peace and to the prosperity of France.

On Friday, August 16, a general excursion took place to the paper works at Essonne, and to the works of M. Decauville (the constructors of the narrow-gauge railway in the Exhibition), who offered a lunch to their guests.

Thus concluded this brilliant and successful session of the French Association.

All foreigners must have carried away with them a pleasant memory of their welcome. Special thanks are due to Prof. Gariel, the Secretary of the Association, and to the Vice-Secretary, Dr. Cartaz, for the remarkable kindness and courtesy they showed to the foreign guests.

EXPERIMENTS ON ELECTRO-MAGNETIC RADIATION, INCLUDING SOME ON THE PHASE OF SECONDARY WAVES.

In continuation of some experiments which were described in NATURE, vol. xxxix. p. 391 ("Repetition of Hertz's Experiments and Determination of the Direction of the Vibration of Light") attempts were made to obtain periodic reflection of electric radiation from plates of different thicknesses, analogous to Newton's rings, with the view of further identifying these radiations with "light."

It was there described how a sheet of window-glass refused to reflect the Hertzian waves, but how a masonry wall reflected them readily. The non-reflection from the thin sheet is due to the interference of the reflected waves from each side which takes place owing to a change of phase of half a period on reflection at the second surface, as in the black spot of Newton's rings.

By making the reflecting plate such a thickness that the reflection from the back has to travel half a wave-length further than that from the front, the two reflections ought to be in accordance, for they differ by a whole period, half arising from difference in path, and half from change of phase on reflection; but if the difference in paths were made a whole wave-length by doubling the thickness of the plate, there ought again to be interference, and so on.

The first plan tried with this end in view, was to fill a large wooden tank to different depths with water or other liquids. On gradually filling the tank reflection should be obtained, and at a certain depth equal to $1/4\,(\lambda \sec r)/\mu$, reach a maximum; further addition of the liquid then should diminish the reflection, and at double the above depth the reflection should reach a minimum, the two waves interfering.

The mirrors for concentrating the radiation had for this purpose to be suspended over the tank as shown in the figure. The tank was first tried empty, but unfortunately the wooden bottom was found to reflect, thus it was useless for the purpose intended. I then tried what ought to have been tried before constructing the tank—namely, whether ordinary boards, such as flooring, reflected. The floor was found to reflect readily. This was attributed to moisture in the wood causing it to conduct, specially as wood was found not to polarize by reflection. Experiments were then undertaken to determine if water reflected, even though in thin sheets. A large glass window was placed beneath the mirrors and flooded with water; this was found to reflect well, both when the mirrors were in the position shown and when rotated to the position "at right angles." Thus water also acts like a metal, reflecting the radiation however polarized. The glass had to be hardly more than damp in order to get some reflection.

The wooden tank being unsuitable, a glass tank was thought of, but was given up for solid paraffin, which, being in slabs, could be easily built up into a vertical wall of any desired thickness. Through the kindness of Mr. Rathborne a large quantity of this was lent for the purpose.

A thin sheet of paraffin about 2 centimetres thick was found not to reflect, as was expected. Next a wall 13 centimetres thick (180 centimetres long, 120 centimetres high) was tried, and found to reflect, this being the thickness required in order to add another half period to the retardation of the wave reflected from the back at an incident angle of 55°, the wave-length being taken as 66 centimetres, and the index of refraction being taken as 1·51, the square root of 2·29, the value taken as the specific inductive capacity of paraffin.

Then a wall twice the thickness was tried, but it also reflected, contrary to expectation. While in doubt as to the cause of this, it was decided to make a determination by direct experiment of the index of refraction of paraffin for these waves, by a method suggested in NATURE (vol. xxxix. p. 393), which consists in interposing a sheet or wall of paraffin between the resonator and the metallic reflection in the Hertzian experiment of loops and nodes which are formed by the interference of the reflected wave with the direct radiation; the ratio of the velocity in the wall to that in the air being easily found from the observed shifting of the loops and nodes towards the screen.

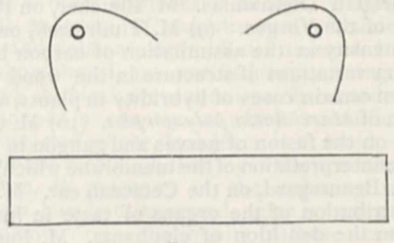


FIG. 1.

In this way the index of refraction for the radiation of the period employed was found to be about 1·8, so that the paraffin walls which had been used were too thick; The proper thickness being about 10 and 20 centimetres—exactly so for an incident angle of 51°. On making this alteration I fancied I could detect a slight difference between the reflections from the thick and thinner walls; still the difference was not sufficient to be at all satisfactory. The nature of the observing apparatus makes it almost impossible to say if the reflection on one occasion is more intense or less so than on another so long as sparks can be obtained. This is due to the sparking-point in the receiving apparatus continually requiring readjustment when working with small sparks, as the distance between them changes either from shaking or from the points getting burnt up. Dust and moisture from the observer's breath are also troublesome.[1] Thus it might be quite possible that the points had always to be much closer with the 20 centimetre wall than with the 10 centimetre wall in order to get sparks, and yet the difference escape detection; the thing observed being whether sparks can be obtained or not, the eye being incapable of comparing with any degree of accuracy the intensity of light on one occasion with that on another.

However, if it had been possible to suddenly change the wall, while viewing the sparking, from being 10 to 20 centimetres, it would have been easy to detect any

[1] With *very small* sparks the thermal expansion must be counteracted by unscrewing.

difference which might have existed, but unfortunately it took some little time to alter the wall.

In order to obviate this difficulty the following device was resorted to with the object of showing that there was a difference in the behaviour of the wall when 10 centimetres thick to its behaviour when 20 centimetres thick. (For at the time I did not see that the experiment was inconclusive, the effects observed being the same whether the back reflected at all or not). A *small* sheet of zinc was placed at the back of the wall, and the effect on the sparking observed while an attendant suddenly removed or again replaced the zinc. It was supposed that when the wall was 20 centimetres thick and there was sparking, that on suddenly placing the zinc on the back the sparking would increase, owing to the phase of the reflection from the back being half a period different from that of the reflection from the zinc; but when the wall was 10 centimetres thick that the presence of the zinc would diminish the sparking.

It was with no little surprise that the reverse was observed. That is to say, placing a sheet of zinc about 30 centimetres square on the back of the wall actually aided the reflection from the back so as to diminish the sparking with the 20 centimetre wall, but increasing it with the 10 centimetre wall. This observation made it look as if it must be on the first reflection from the paraffin—that is to say, on passing from a rare to a dense medium—that the "change of phase" occurs, and not at the back,—at a reflection from a dense to a rare medium, as is ordinarily supposed. For Hertz's experiment of loops and nodes showed that there was no change of phase on metallic reflection—that is, of the *magnetic* displacement. There is a change of phase of the *electric* displacement. It is important to bear in mind that the electric loop and the magnetic node occurred at the same place, and of course so too the electric node and the magnetic loop.

In order to investigate this, attempts were made to obtain Hertz's loops and nodes off a paraffin wall as reflector, but no reflection could be discovered, the intensity of the vertically reflected rays being insufficient. However, by inclining the incident radiation to an angle of 57°, the intensity of the reflection was found to be amply sufficient. With a circular resonator, which is for these

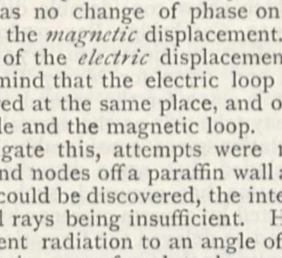


FIG. 2.

waves about 10 centimetres in diameter, sparks were obtained close to the reflector, the circle being held at right angles to the wall so as to be equally inclined to both direct and reflected radiation, and this was confirmed by a straight resonator giving none there. At 30 centimetres from the wall[1] there was interference with the circle, and vigorous sparking with the straight resonator. This being about the right distance for the loop to be from the reflector at an incident angle of 57°,

$$\frac{\lambda}{2} = l + l' = p \sec i(1 + \cos 2i) = 2p \cos i.$$

Thus there is no doubt that it is on the second reflection that the change of phase occurs.

Here, then, was a difficulty—the small sheet of zinc at the back of the paraffin undoubtedly reflected with a change of phase, while, according to the Hertzian experiment, metallic reflection is unaccompanied by change of phase. On mentioning this to Prof. Fitzgerald, he pointed out to me its complete agreement with wave theory. For by considering the secondary waves produced by dividing up a primary wave with reference to any point into half-period zones, it can be seen that the effect of the primary is equivalent to half of that arising from the central circle, and in consequence is half a period behind the phase which would be at the point if an infinitesimal portion of the centre alone acted. For the effect of each ring can be considered as destroyed by half the effect of its two

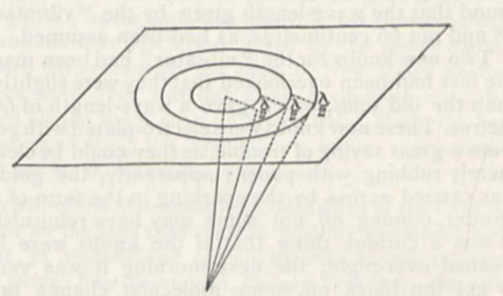


FIG. 3.

neighbours, and thus half the effect of the central circle is left uncompensated. But the distance of the edge of this circle is half a wave-length further from the point than its centre is, so that the resultant phase at the point will be behind that due to the centre, but in front of that due to the edge, which effect would be half a period behind that arising from the centre. Taking the mean between them, the resultant phase then at the point is a *quarter*[1] of a period behind what it would be if the centre alone acted. Thus it was that the reflection from the *small* sheet of zinc differed from what I had expected it to be.

Experiment showing Phase of Secondary Waves.—To experimentally test this, the small sheet of zinc was used as reflector in the Hertzian experiment of loops and nodes. Employing the circular resonator, the position of interference was found to have shifted out from 17 to over 24 centimetres, which nearly corresponds to an acceleration of phase of a quarter of a period, the wave going in all nearly a quarter of a wave-length further, and nevertheless being still only half a period behind the phase on starting. The furthest out the loop could be is 25·5 centimetres: to obtain this would require an indefinitely small reflector. Of course, when the resonator was close in to the sheet, no change of phase was found to occur, the sheet being then practically infinite.

Another interesting observation was made. A *long* sheet of zinc 30 centimetres wide was found to act similarly to the sheet 30 centimetres square, provided it was placed with its breadth parallel to the electric displacement. When thus placed at 24 centimetres from the circular "resonator," there was interference, but on rotating the reflector so as its length was parallel to the electric displacement, sparking occurred, and now the "resonator" had to be brought back to 17 centimetres in order to again obtain interference. This experiment is interesting in connection with the electro-magnetic way of looking at the acceleration of phase as being due to the accumulations of electricity on the edges of the reflector, which is the same as the reason why it is necessary to use *long* cylindrical mirrors, as was pointed out by Prof. Hertz in a letter last February to Prof. Fitzgerald. This experiment is really the same as Stokes's *experimentum crucis*, as Prof. Fitzgerald points out.

If, instead of using the whole primary wave in the

[1] It would occur at about 17 centimetres on vertical reflection. This experiment was also tried with a metallic reflector.

[1] That it aided the back rather than the front was probably due to their phase not being an exact period or half period different from each other.

former experiment, it be passed through a screen with a hole in it (either square or a long slit at right angles to the electric displacement), the position of interference, as might be anticipated, was not shifted out as much as before. In the rough experiment made, it was found to occur at about 19 centimetres from the screen.

It was now thought well to repeat the determination of the index of refraction with a larger wall and metallic reflector than had been used before, as this change of phase might have affected the former results. But it was found that it had not done so to a sensible extent. However, the result of these new experiments was finally to give for paraffin, $\mu = 1{\cdot}75$, and at the same time it was found that the wave-length given by the "vibrator" was 68 and not 66 centimetres, as had been assumed.

Two new knobs for the "vibrator" had been made, and the fact had been overlooked that they were slightly larger than the old ones, which gave a wave-length of 66 centimetres. These new knobs were electro-plated with gold, and were a great saving of trouble, as they could be cleaned by merely rubbing with paper; apparently, the gold which was carried across by the sparking in the form of a black powder coming off, but some may have reburnished on. It was a curious thing that, if the knobs were left uncleaned over-night, the next morning it was very hard to get the black off, some molecular change probably occurring.

If the value of μ thus found be not in some way due to the paraffin being in separate blocks, it would show a remarkable anomalous dispersion for paraffin near these curiously slow vibrations, and, as suggested by Prof. Fitzgerald, may be connected with the vibration periods of atoms in the molecule, as it can hardly be connected with the vibrations in the atoms themselves. It might be interesting to investigate whether these slow vibrations could cause dissociation, and thus lead to a photographic method of observing them. It may also be allied with ordinary electrolysis by very long period currents, as is also suggested by Prof. Fitzgerald.

Assuming $\mu = 1{\cdot}75$,[1] and $\lambda = 68$ centimetres, the thicknesses of the walls in the "Newton's ring" experiment, as above described, were wrong. However, it was found more convenient to alter the angle of incidence to suit the walls than to change the thickness of the walls. Thus, the mirrors were put at 25°, which is the proper angle with the above data for 10 centimetre and 20 centimetre walls. On now repeating the experiment, better results were obtained than I should have anticipated. When the wall was 10 centimetres thick, continuous sparking was easily obtained, but, when 20 centimetres thick, it was only after much adjustment and patience that perhaps one slight spark could be obtained. This was quite sufficient, considering the nature of the wall, for it was only built up of plates, which afforded internal reflections, weakening the transmitted rays, and also since it requires the sum of the effects arising from the multiple reflections back and forward inside the wall to completely interfere with the front, and some of these are lost at the edge of the beam.

FRED. T. TROUTON.

ICE GROWTH.[2]

SEVERAL of the Arctic Expeditions have instituted observations on the rate of growth of the ice during the winter, and in the "Contributions to Arctic Meteorology," vol. i., 1885, records of this nature are given for the following stations: Gulf of Boothia, Assistance Bay, Port Bowen, Walker Bay, Cambridge Bay, Camden Bay, Princess Royal Islands, and Mercy Bay. The second German Arctic Expedition has also undertaken such investigations and published results in its report (Leipzig, 1874).

In the present paper these observations are compared with the results of the theory of ice-formation, the basis of which has been explained by the author in a paper on some points in the theory of the conduction of heat.

The theory gives for the thickness (λ) of ice formed in the time (t) the following formula, which is approximately correct—

$$\lambda^2\left(1 + \frac{cb}{3\lambda}\right) = \frac{2kT}{\lambda\sigma}.$$

In this formula (c) is the specific and (λ) the latent heat of ice; (k) is the coefficient of conduction, (σ) is specific gravity, (f) is the temperature at the surface of the ice at the time (t), (k) the sum of cold for the same time; the last is the sum of the temperatures counted downwards from 0° C., from the commencement of ice formation up to the time (t).

The squares of the measured ice-thicknesses were taken and multiplied by the factor contained in the above formula, and the differences between the successive values were divided by the sums of cold for the corresponding intervals of time. The quotients for the different observations of each station differ in some degree from each other. Those which are most regular are the Gulf of Boothia, Assistance Bay, Port Bowen, and the German Expedition. The mean values for these four stations are, respectively, 0·877, 0·851, 0·869, and 0·878. Of the stations which show considerable differences, Walker Bay gives 0·919, Cambridge Bay 0·780, Camden Bay 0·791. The smallest quotients are, 0·755 for Princess Royal Islands, and 0·700 for Mercy Bay in the first winter, whilst it is 0·810 for the second winter.

We may take 0·87 as the normal value of the quotient. If any station gives considerably less than this, it may be concluded that some disturbance of the process of ice formation has occurred, such as, *e.g.*, if the station was affected by a warm current which brought up warmer water, and so delayed the formation of the ice.

The units for the figure 0·87 are the Fahrenheit degree, the English inch, and the day. For Celsius, centimetres, and the day, the figure would be 10·092. From it the coefficient of conduction of Polar ice, k, is 362·4. If the second be taken as the unit of time, the value of k would be 0·0042.

[1] This value agrees with polarization experiments. No reflection was obtained at its corresponding angle, while at $\tan^{-1} 1{\cdot}51$ some sparks were occasionally seen.

[2] Abstract of Dr. Stefan's paper, "Ueber die Theorie der Eisbildung, insbesondere über die Eisbildung im Pola-Meere," from the *Sitzungsberichte* of the Vienna Academy.

DR. C. FORSYTH MAJOR'S DISCOVERIES IN THE ISLE OF SAMOS.

WE are glad to learn that Dr. H. Woodward, who has just returned from Florence, has secured for the Palæontological Department of the British Museum (Natural History) the valuable collection of Vertebrate remains of Lower Pliocene age obtained by Dr. C. Forsyth Major during the year 1887 in the island of Samos, in the Turkish Archipelago. Dr. Forsyth Major lately contributed to the *Comptes rendus* (vol. cvii. pp. 1178–81) a preliminary notice of this collection. Among the remains are a large number of forms specifically identical with the mammals from the equivalent deposits of Pikermi in Attica, Baltavar in Hungary, and Maragha in Persia; but there are also some new types, which are of interest either from a distributional or a purely zoological point of view. Among these new forms is a species of ant-bear (*Orycteropus*), which is the only representative of that genus yet known beyond the Ethiopian region. A large pangolin, which is estimated to have been nearly three times the size of the West African *Manis gigantea*, is made the type of the new genus *Palæomanis*; and is of interest as showing how the African pangolins may have been connected with those of India. Perhaps the most striking new type is a large ruminant, referred by the author to the *Giraffidæ*, and stated to connect *Helladotherium* and the giraffe with some of the aberrant antelopes of Pikermi. Finally, a large ostrich is especially noteworthy from a distributional point of view, since we now have remains of this genus from Samos, the Thracian Chersonese, and Northern India.

NOTES.

We regret to have to record the death of the eminent American physicist, Prof. Elias Loomis. He was born at Wellington, Connecticut, in 1811, and was educated at Yale College, where he acted for some time as tutor. He was successively Professor of Mathematics and Natural Philosophy in Western Reserve College, Ohio; Professor of Natural Philosophy in the University of the City of New York; and Professor of Natural Philosophy and Astronomy in Yale College. The latter appointment he received in 1860, and he held it until his death. Prof. Loomis was the author of more than 100 scientific treatises. Among his works was a series of text-books on mathematics, natural philosophy, astronomy, and meteorology. Of this series more than 500,000 copies were sold. During the last twenty-five years of his life he devoted himself chiefly to original research, and gave much time and energy to the preparation of a full account of the principles of meteorology. He was a member of various American and European scientific Societies.

On August 19 the fiftieth anniversary of the foundation of the Observatory at Pulkowa was celebrated. Among those present on the occasion were the President of the St. Petersburg Academy of Science, the Grand Duke Constantine Constantinovitch, the Russian Ministers and Court dignitaries, the German and French Ambassadors at the Russian Court, and deputations from the Universities and Academies of Russia, and from foreign observatories, including Greenwich and the chief German observatories. Numerous congratulatory telegrams were received. The Czar expressed in a telegram high appreciation of the scientific merits of the Observatory.

This week the meeting of the Photographic Convention of the United Kingdom is being held in London. It was opened on Monday in St. James's Hall, where an interesting collection of photographic appliances and specimens of the best photographic work had been brought together. In his inaugural address, Mr. Andrew Pringle, the President, reminded his hearers that the present year was the fiftieth of practical photography, and traced the history of the art from its birth to its jubilee.

At the meeting of the Linnean Society of New South Wales on June 26, Dr. Oscar Katz read a paper giving an account of experimental researches with the microbes of chicken-cholera. In this paper the author describes the investigations undertaken by him since last year with regard to the microbes of chicken-cholera. The larger portion of the experiments were made on wild rabbits; in addition, the action of the microbes on domestic poultry, as well as on indigenous birds, on hares, guinea-pigs, and ferrets was studied. A number of other points in the life-history of the chicken-cholera bacteria are dealt with in the paper: for instance, their behaviour when transmitted through the bodies of rabbits in successive generations; towards desiccation, putrefaction, &c. Rabbits were repeatedly protected against a virulent infection, in consequence of having previously partaken, at intervals, of cultures of the microbes in which the latter had been killed by moderate heat.

On August 17, at 1 o'clock a.m., a severe earthquake shock of an undulatory character, lasting six seconds, was felt at Jablanica, Bosnia. The direction of the seismic wave was from south-east to north-west. According to a telegram from Mostar, a shock, lasting ten seconds, was felt there also on August 17. Some damage was caused to the railway line between Mostar and Ostrojac, while at Konjica a wall collapsed. The duration of the shock at the latter town was only five seconds.

The Pilot Chart of the North Atlantic Ocean for the month of August shows that fine weather prevailed generally over the greater part of the North Atlantic during July. The only storms worthy of notice were two which prevailed in the western part of that ocean; the first was situated in latitude 40° N., longitude 58° W., on the 10th, and moved across Newfoundland on the 16th; the next day it was joined by the second storm, which passed out to sea from the coast of New Jersey on the 15th. The storm then travelled about north-north-east, and disappeared off the south coast of Greenland. Much fog has been experienced along the Transatlantic routes, and icebergs have been numerous, within the usual limits.

The Report of the Meteorological Commission of the Cape of Good Hope for the year 1888 contains monthly summaries and results for 38 stations and rainfall statistics at 292 stations, including returns from Basutoland, Orange Free State, &c. In addition to the tables, there are some extremely interesting diagrams showing the actual rainfall in 1888 at a number of stations throughout the colony, as compared with the average rainfall. They show that in nearly all months the fall in 1888 was much above the average. Storm warnings are not yet issued, but weather telegrams are received from a number of stations, and after correction and collation, reports are sent to coast stations and entered on charts, for the information of seamen and others.

Vols. ix. and x. of *Aus dem Archiv der Deutschen Seewarte* contain reports of the activity of that institution during the years 1886 and 1887. The collection of observations at sea, and at distant stations, has been carried on with vigour, while weather telegraphy and the verification of chronometers and nautical instruments also receive great attention. Dr. Neumayer regrets that the ever-increasing routine work curtails the time for scientific investigation, yet the volumes contain many valuable researches by members of the staff, to some of which we can only very briefly refer, although all are worthy of careful study. In vol. ix. Dr. van Bebber, dealing with typical weather-conditions, investigates the passage of barometric depressions over Europe during the years 1881–85, in continuation of a former paper referring to 1876–80. The object of the discussion is to trace the influence of the depressions upon the weather, with a view to the discovery of the laws of the changes of direction of their tracks, and of their rates of progression. The author shows that the depressions move along certain tracks with greater velocity than the motion of minima generally. The text is illustrated by twenty plates. The other articles are: on the determination of the refraction constants; and remarks from ships' logs relating to weather, &c., in Eastern Asiatic waters. In vol. x. Dr. Köppen contributes a useful paper on the determination of air-temperature. The author investigates the influence of radiation on different thermometers and screens, and gives a *résumé* of the experiments with regard to the latter in various countries, and of the observations on local differences of temperature (including the influence of radiation). These experiments seem to show that screens through which the air can freely pass are better than large shelters, and that the effect of radiation is lessened by the free circulation of the air, and by the smallness of the thermometer-bulbs. M. Möller contributes an article on the circulation of the atmosphere between the equator and the Poles. The results arrived at differ from those of Prof. Ferrel, especially with regard to the force of westerly winds in latitude 38°, in the lower and upper strata. The volume concludes with an article on Combe's apparatus for testing chronometers.

FROM the statistics of the Education Department in India during the past year, it appears that the percentage of those of school-going age who actually attended was 11·8 as compared with 10·7 the preceding year. The total attendance increased about 120,000 on the preceding year, but the numbers at the training-schools fell from 5716 to 4761. Year by year the Mahometan pupils have increased, and during the past year their numbers were 804, 485 as against 752,441 in 1886–87. The total expenditure on education has increased from 2,52,41,414 rupees in 1886–87 to 2,61,91,280 rupees in 1887–88; but the indirect expenses—that is, the cost of inspection, buildings, scholarships, &c.—fell from 54,11,098 rupees in 1886–87 to 52,81,471 in 1887–88. The fees increased from 65,29,958 rupees to 72,94,023 rupees. Except in the circumstance that the numbers attending the training-schools have declined very much, the Government thinks that everywhere marks of great progress are shown. The expenditure by local bodies on education has also increased from 37,14,579 rupees in 1886–87 to 46,41,551 rupees in 1887–88.

A SWEDISH paper gives an account of some experiments made by Captain R. von Mühlenfels in Carlskrona in storing live fish, his object being to discover the maximum degree of confinement they could bear without deteriorating in quality. With this aim he placed 1300 kilogrammes of live cod in a cask, covered within and without with asphalt tar, and of about 52 cubic metres in content. The cask was firmly fixed in a stream of fresh running water. During the first few weeks of their confinement the fish grew thinner, and had deteriorated in quality. At the end of six weeks, however, it was found that those which remained had much improved by their captivity.

THE French *Journal Officiel* recently contained a report which Prof. Edmond Perrier had sent in to the French Government on the subject of the best means of protecting fishermen against porpoises. Although the depredations of these creatures have been exaggerated, it is certain that they do considerable damage, especially on the Mediterranean coasts of France, by tearing the nets. For at least a quarter of a century past, efforts have been made to lessen the numbers of the porpoises by offering a reward per head, and by other remedies. In 1865 the Government invited the fishermen themselves to organize a seine-net fishery for porpoises, and they were offered special nets, and sums of from 5 to 25 francs for each animal. It was, however, found in practice that as soon as the porpoises felt themselves to be surrounded, they simply jumped over the seine-nets and were at large again. Some years later the fishermen of Cannes, Saint Tropey, and La Ciotat petitioned Government to lend them a gun-boat, filled with torpedoes, for the purpose of firing at the porpoises. This was done, and the cannon and the torpedoes scared away the porpoises for about eight days, but they scared away the fish as well, so that there was no fishing for at least a week. The porpoises, moreover, are too numerous and too agile to be shot, one by one, in an effective manner. The report sums up that the employment of artillery against porpoises is perfectly useless, that a reward for killing them singly is equally unavailing, and that the only thing to do is to encourage the fishermen to unite in chasing the porpoises, and in forming a mutual insurance guarantee against their depredations. In the meantime the Department of the Marine might continue to indemnify, to a certain extent, the proprietors of any nets that have been very seriously injured.

A FRUITFUL study may be made of the sociology of small sections of a community. M. Dumont finds in the rural communes of France much individuality as regards density, wealth, mobility, birth, marriage, and death rates, &c. We will give a few facts from his interesting paper (*Rev. Scient.*). Unlike what usually occurs near large towns, the eleven rural communes in the outskirts of Caen are with one exception (Mondeville) being depopulated. Great mobility in the rural populations is generally associated with a low birth rate, great fixity with a high one. Examples of a low birth rate occur in the communes near Caen (13 to 17); on the other hand, the natality is high in some of the poor and miserable communes of the Côtes-du-Nord (40 to 41). Side by side in the same department, and even the same canton, are very different birth rates. For the nine communes of Paimpol (Côtes-du-Nord), some 20,000 inhabitants, the rate varies from 20 to over 30. In Bretagne, with a generally high birth rate, there are isolated communes with a very low one; and two communes are cited, which are apparently much alike, and where subsistence is drawn from the sea, but the birth rate of one is 35·1, that of the other 21·7. In the canton of Isigny (Calvados) the birth rate was in general steadily low throughout the century till ten or twenty years ago, when there was a remarkable rise. The marriage rate in Ile-de-Ré is considerably above the average for France, while the fecundity of marriage has of late been going down. But one commune, the small town of Saint Martin, shows a recent decline of its marriage rate (25 per cent. in three decades) while the constancy of its low birth rate points to an increased fecundity of marriage. The death rate in many communes (those of Douvres, *e.g.*) is now much greater than early in the century. The increase of illegitimacy and high mortality of infants are often obviously the cause of a rising death rate.

CONSIDERING the progress of biology, Dr. Düsing remarks (in a recent number of *Humboldt*) that for 25 years the effort of zoology has been mainly to determine the descent of animals by means of their morphological features. The flourishing of morphology has been favoured by the discovery of a large number of lower forms of animal life. But the time is not far off when it will become more and more difficult to discover new animals, and those known will have been efficiently described. Investigation will therefore be directed more to the mode of life, which, in the case of most animals, is but little known. This demands long observation and a special talent of observation, which is quite different from that required by morphology. Several investigators have entered this sphere, studying especially the higher animals; but most continue in the direction impressed on zoology by Darwin. The prevailing tendency is exemplified in the founding of a Professorship of Phylogeny at Jena. And it is a remarkable fact that the present occupant, Prof. A. Lang, is one of those who are leaving the older paths for biology, as shown by his recent work, which is entitled "On the influence of a fixed mode of life on animals, and on the origin of non-sexual propagation through fission and budding."

THE *Kew Bulletin* for August consists of a full account of the fluted scale-insect (*Icerya purchasi*, Maskell), which is described as one of the most destructive pests injurious to plants. Of late years it has made its appearance in South Africa, New Zealand, and California. The best account of the insect has been published by Prof. Riley, and the results of his inquiries are made easily accessible in the *Bulletin* to a wide circle of readers.

MR. WRAY, Curator of the Museum of Perak, in the Malay Peninsula, communicates to the *Perak Government Gazette* a note on the mango weevil, a pest which has lately excited some alarm in various parts of the East. Mr. Wray describes it as a small beetle infesting the mangoes in Perak, particularly the introduced varieties, which has been examined and found to be identical with the mango weevil of India, known to science as *Cryptorhynchus mangnifera* of Fabricius. It is a small dark rusty-grey insect about a quarter of an inch long, and, though it and its ravages have long been known, little or nothing has up to the present time been discovered of its habits. It is believed that it lays its eggs in the flower or very young fruit, for in the ripe fruit there is no external mark to show where it gained an entrance, and it is not until the perfect insect eats its way out of the mango that it is possible to tell whether any particular fruit is sound or diseased. Some varieties of the mango enjoy complete immunity from the attacks of this insect, and it has been noticed that even particular trees of varieties which are not so favoured always escape. This fact seems to hold out the hope that, by careful selection, good varieties of the fruit could be raised which would not be subject to the attacks of this destructive pest. The character which renders the fruit unsuitable as food for the weevil is, and probably always will remain, unknown, as our senses may not be keen enough to detect the particular taste or smell which prevents the female from laying her eggs in the fruit of the naturally protected trees.

Dr. Henry C. McCook is about to issue an elaborate work on "American Spiders and their Spinning-work." It embraces studies extended over more than fifteen years, and will be printed in three volumes. Volumes I. and II. will contain the author's personal observations, studies, and illustrations of the habits and industry of spiders. The studies are particularly directed to the spinning habits of the great group of spiders known as orb-weavers; but these are expressed in their relations to all the other tribes in both hemispheres. Volume III. will contain the systematic part of the work, and embrace descriptions of the orb-weavers of the United States, illustrated by a number of lithographic plates, painted by hand in the colours of Nature. The volumes will be illustrated, wholly from Nature, the number of engravings in the first volume alone exceeding two hundred.

Messrs. J. and A. Churchill hope to publish in September a work on "Fuel and its Applications," by Mr. E. J. Mills, F.R.S., and Mr. F. J. Rowan. It will be the first volume of a large work on chemical technology, of which Mr. C. E. Groves, F.R.S., will be the general editor, and which will be founded on one written by Richardson and Ronalds, familiarly known as "Knapp's Technology." Messrs. Mills and Rowan's work on fuel is in reality a new work, dealing with the applications of fuel to arts and manufactures as introduced by the most modern discoveries. The volume is profusely illustrated.

Messrs. George Philip and Son have published a new edition of Mr. T. Rhodes's useful "Steamship Guide and Holidays Afloat." The work has been thoroughly revised, and much new information is embodied in the present edition.

A work on "The Microscope in the Brewery and Malt-house," by C. G. Matthews and F. C. Lott, will be published early next month by Messrs. Bemrose and Sons.

A Calcutta firm has published a book entitled "The Game, Shore, and Water Birds of India," by Colonel A. Le Messurier, which is an attempt to describe the miscellaneous water-fowl of India. The author treats of the birds under three heads—namely, "Scratchers," "Waders," and "Swimmers,"—and, besides classifying them under these heads, he gives the native names of the birds in the various languages. There are in all 121 illustrations. The book might well be enlarged, particularly by the addition of a list of places where the various species may be found.

The additions to the Zoological Society's Gardens during the past week include a Great-billed Touracou (*Corythaix macrorhyncha*) from West Africa, presented by Lady Charlotte Blandford Griffith; a Water Chevrotain (*Hyomoschus aquaticus*) from West Africa, a Blue and Yellow Macaw (*Ara ararauna*) from Brazil, deposited.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *AUGUST* 25-31.

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

At Greenwich on August 25

Sun rises, 5h. 4m.; souths, 12h. 1m. 50·6s.; daily decrease of southing, 16·4s.; sets, 19h. 0m.: right asc. on meridian, 10h. 17·6m.; decl. 10° 37′ N. Sidereal Time at Sunset, 17h. 17m.

Moon (New on August 26, 14h.) rises, 3h. 21m.; souths, 11h. 16m.; sets, 18h. 59m.: right asc. on meridian, 9h. 32·0m.; decl. 18° 0′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	6 33 ..	13 3 ..	19 33 ...	11 18·5 ..	5 16 N.
Venus ...	1 20 ..	9 17 ..	17 14 ..	7 32·4 ..	20 35 N.
Mars ...	3 1 ...	10 43 ...	18 25 ...	8 58·2 ...	18 21 N.
Jupiter ...	15 43 ...	19 36 ...	23 29 ..	17 53·2 ...	23 25 S.
Saturn ...	4 16 ...	11 35 ...	18 54 ...	9 50·6 ...	14 19 N.
Uranus ...	9 28 ...	14 56 ...	20 24 ...	13 12·6 ...	7 4 S.
Neptune..	22 7* ..	5 57 ...	13 47 ...	4 11·5 .	19 26 N.

* Indicates that the rising is that of the preceding evening.

Aug.	h.	
25 ...	5 ...	Jupiter stationary.
25 ...	20 ...	Saturn in conjunction with and 2° 28′ south of the Moon.
27 ...	22 ...	Mercury in conjunction with and 4° 42′ south of the Moon.

Variable Stars.

Star.	R.A.	Decl.		
	h. m.	° ′		h. m.
Algol	3 1·0 ...	40 32 N. ...	Aug. 27,	21 10 *m*
δ Libræ	14 55·1 ...	8 5 S. ...	,, 29,	0 48 *m*
R Scorpii	16 11·0 ...	22 40 S. ...	,, 29,	*M*
S Scorpii	16 11·1 ...	22 37 S. ...	,, 31,	*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 28,	3 57 *m*
			,, 29,	0 5 *m*
X Sagittarii... ...	17 40·6 ..	27 47 S. ...	,, 26,	1 0 *M*
Y Sagittarii... ...	18 14·9 ...	18 55 S. ...	,, 28,	2 0 *M*
R Scuti	18 41·6 ...	5 50 S. ...	,, 29,	*m*
U Aquilæ	19 23·4 ...	7 16 S. ...	,, 29,	1 0 *m*
T Vulpeculæ ...	20 46·8 ...	27 50 N. ...	,, 30,	22 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
From Pisces	6 ...	11 N. ...	Swift.
Near β Trianguli	30 ...	36 N.	
,, α Camelopardalis ...	70 ...	65 N. ...	Swift; streaks.
,, δ Cephei	336 ...	57 N. ...	Swift.

GEOLOGY IN RUSSIA.[1]

The Geological Committee of St. Petersburg has made an important contribution to Russian geology by bringing out a new sheet of the Geological Map of Russia, covering the Southern Urals.[2] A volume of explanatory text accompanies the map. It appears from the recent explorations of the members of the Committee that, contrary to the current opinion as to the Southern Urals consisting of a number of chains radiating from Mount Yurma, the great chain consists in its southern parts of a number of chains parallel to one another, and all running from the south-west to the north-east. The main water-parting is built up of granites, syenites, and gneisses, considerably worn down by denuding forces; it has a steep slope towards the east, where its base disappears beneath the Tertiary deposits, while towards the west it is over-

[1] *Memoirs* (vols. ii. and iii.) and *Izvestia* (vols. iv. and v.) of the Russian Geological Committee.

[2] "Carte géologique générale de la Russie," Feuille 139; "Description orographique," by A. Karpinsky and Th. Tchernysheff; "Hauteurs absolues de l'Oural méridional," by A. Tillo; and "Explication de la Carte," by A. Karpinsky and Th. Tchernysheff, in *Memoirs*, vol iii. No. 2. (In Russian, with summaries in French.)

lain by thick beds of Devonian, Permian, and Carboniferous deposits, and these strata are so folded as to make several parallel chains rising more than 3300 feet above the sea, and containing the highest summits of the region. Further west the country assumes the character of a plateau which is built up of nearly horizontal strata of the formation—so characteristic of the Urals—which has a fauna intermediate between the Permian and Carboniferous of Western Europe. Above this there are Triassic deposits.

Several other contributions will be devoted to the same region. One, already published, contains a most elaborate work "On the Lower Devonian Fauna of the Western Slopes of the Urals," by Th. Tchernysheff. Until 1880, the bituminous, gray, and dolomitic limestones of the Urals—very poor in fossils as a rule—were thought to be Silurian, but M. Tchernysheff describes 107 species recently found in these limestones, and shows that their fauna is of Lower Devonian age. This conclusion is of great importance, as it throws light on the age of the very same series of limestones, quartzites, and slates in Siberia and Turkestan (also arrayed in ridges running south-west to north-east).

Another sheet of the geological map covers the most interesting region on the right bank of the Lower Volga.[1] Upper Carboniferous strata appear in that region in the deeper ravines only; the Cretaceous formation is represented by beds belonging to the *étage Aptien* and Neocomian groups of the Lower Cretaceous, as also by the Cenomanian, Turonian, and Senonian groups of the Upper Cretaceous. Nearly the whole of the region is covered, however, by Eocene clays and sands; boulders, partly of local origin, and partly carried from North-Western Russia, are strewn over the surface, and the manner of their distribution is such as to exclude the possibility of transport by floating ice. Prof. Sintsoff concludes, therefore, that the ice-sheet of Russia extended as far south-east as the Volga under the 50th degree of latitude.

Finally, an important contribution to the palæontology of Russia—"The Ammonites of the *Aspidoceras acanthicum* Beds of East Russia"—is published in the same *Memoirs* (ii., 3) by Prof. Pavlow. These beds, which are met with in Simbirsk and the Southern Urals, have a mixed fauna, the characters of which may be best explained by the statement that during the Jurassic period Central Asia was under the sea, and that this basin was in connection with the Jurassic basins of both Tibet and Central Europe.

Besides the above larger works, the Geological Committee has brought out two volumes of *Izvestia* (*Bulletins*), which contain a mass of valuable information. Prof. Mushketoff's notes upon the Kalmuck Steppes are, as usual, rich in most suggestive remarks about the activity of wind and water in the desert. He shows also that during the Quaternary period the Caspian Sea did not extend further west than the Ergheni Hills, communicating with the Black Sea through the Manytch Valley only. In a paper on the limits of glaciation in Central Russia and the Urals (vol. iv.), M. Nikitin shows that the ice-sheet extended in Russia as far south as 48° 30′ N. latitude on the Dnieper, and 50° on the Volga.[2] In a subsequent paper (vol. v.), devoted to the post-glacial deposits of Germany, the same author distinguishes two different kinds of loess, one of which may be due to the agency of wind. Many papers are devoted to the Urals—their crystalline rocks, the traces of glaciation (M. Krotoff, in vol. iv. fasc. 9), and the intermediate Permian-Carboniferous beds, the fauna of which, according to Prof. Stuckenberg, contains forty-one Carboniferous species, thirty-four Permian, seven species belonging to both, and twenty-three characteristic of the Permian-Carboniferous groups. Four papers are devoted by M. Mikhalsky to Poland, and it appears that the beds of Poland, which were formerly thought to be Jurassic, must be regarded as Neocomian—that is, Cretaceous. M. Lagusen describes a new sub-genus, *Lycophoria*, of the *Strophomenidæ* family; Prof. Schmidt deals with the glacial and post-glacial deposits of the Baltic provinces; M. Pavlow describes the *Exogira virgula* beds, as also some Cretaceous and Tertiary deposits of South-East Russia; and M. Nikitin gives a sketch of the Carboniferous deposits and the loess of Samara.

Another work, issued by the Russian Geological Survey, deserves especial mention on account of its general interest and value. This is the annual bibliography of works on geology, mineralogy, palæontology, &c., published in Russia, or works published elsewhere which refer to Russia ("Bibliothèque géologique de la Russie"). Brief abstracts, in Russian with a French translation, are given of the more important papers. The titles are given in the original language; if the original is Russian, then a French version is given; if the original is not Russian, a translation into this language is added.

The publications for the year 1885 number 256; for 1886, 356; for 1887, 405; and for 1888, 390. But the later series include omissions in earlier numbers. The editor of this useful annual is M. S. Nikitin; his chief assistant in the work is Mdlle. Marie Tzwetaev.

[1] "Carte géologique générale de la Russie," Feuille 93; "Kamyschin," by I. Sintsoff, in *Mémoires du Comité Géologique*, vol. ii. No. 2.

[2] An abridged translation of this paper has been published in *Petermann's Mitteilungen*.

SCIENTIFIC SERIALS.

American Journal of Science, August.—On the observation of sudden phenomena, by S. P. Langley. The paper deals with the apparently inherent defects of human observation, especially in recording unexpected natural phenomena, its object being to reduce this personal error to a minimum. The author believes that a means may be found by which any person, skilled or ignorant, may make not only meridian observations, but an observation of any sudden visible event, of whatsoever nature, so accurately that no correction need be applied. An instrument constructed for the purpose, and here illustrated, has been tried by various observers in various ways, the probable error for any single observation being rather less than one-twentieth of a second.—A spectro-photometric comparison of sources of artificial illumination, by Edward L. Nichols and William S. Franklin. These experiments, made in Cornell University during the summer of 1888, consist in the spectro-photometric comparison of various artificial sources of light and of daylight with that emitted by a sixteen candle-power incandescent lamp. The sources of light subjected to measurement were a standard candle, various petroleum and illuminating gas flames, a lime-light, two electric arc lights, clear daylight, an incandescent lamp of high resistance at various temperatures, and an incandescent lamp of low resistance at normal candle-power. The general result is that candle-power as determined by means of the Bunsen photometer affords no correct measure either of light-giving energy or of the luminosity of the source of light, the direction of the error always being such as to favour sources of a low degree of incandescence when compared with those of higher temperature.—On the possibility of hemihedrism in the monoclinic crystal system, with especial reference to the hemihedrism of pyroxene, by George H. Williams. A fresh study of the remarkable crystals of pyroxene from Orange County, New York, recently described by the author as hemimorphic, seems to show that they should rather be regarded as hemihedral, and that they are by no means an isolated instance of this peculiar development in pyroxene.—On the earlier Cretaceous rocks of the north-western portion of the Dominion of Canada, by George M. Dawson. The purpose of this paper is to call attention to certain facts recently brought to light respecting the equivalency of the Queen Charlotte Islands and Kootanie formations, and to the importance of the earlier Cretaceous rocks, of which they are representatives, over great areas of the western and extreme north-western portion of the continent. These facts are just now specially interesting from their analogy to those lately developed by Mr. R. T. Hill respecting a similar earlier Cretaceous formation in the south-western region of the United States.—A new occurrence of gyrolite, by F. W. Clarke. This specimen, from the New Almaden quicksilver mine, California, is shown on analysis, and by comparison with How's figures for a Nova Scotia gyrolite, to be a somewhat impure gyrolite associated with apophyllite, and agreeing approximately with the formula $Ca_2Si_3O_8 . 3H_2O$—On action of light on allotropic silver, by M. Carey Lea. The author's further studies of this subject show that light can convert yellow or red-yellow allotropic silver to white, and cause the blue-green modification to pass to the gold-yellow.—Papers were contributed by J. F. Kemp, on certain porphyrite bosses in North-Western New Jersey; by W. B. Dwight, on recent explorations in the Wappinger Valley limestones and other formations of Dutchess County, New York; by George F. Becker, on silicic acids; and by O. C. Marsh, on gigantic horned Dinosauria from the Cretaceous. Mr. Marsh also continues his memoir on the discovery of Cretaceous Mammalia, illustrating the subject with two plates of the teeth of American Cretaceous mammals.

SOCIETIES AND ACADEMIES.

London.

Royal Society, May 2.—"On the Spectrum, Visible and Photographic, of the Great Nebula in Orion." By William Huggins, D.C.L., LL.D., F.R.S., and Mrs. Huggins.

I have added the name of Mrs. Huggins to the title of the paper, because she has not only assisted generally in the work, but has repeated independently the delicate observations made by eye.

In the year 1882 I had the honour to lay before the Royal Society a note on the photographic spectrum of this nebula, in which I described a new bright line in the ultra-violet, to which I gave a wave-length of about 3730. In addition to this new line, the lines of hydrogen, $H\beta$ and $H\gamma$, which I had discovered by eye in my early observations on the visible spectrum, were to be seen upon the plate.

On account of the faintness of the object the slit had been placed rather wide, and for this reason the character of the line and its position, as I stated in the paper, could not be ascertained with the accuracy which I desired.

On February 5, 1888, a photograph of the spectrum of this nebula was obtained with a narrow slit; the same apparatus, so far as the essential parts, which were described in my paper, on the "Photographic Spectra of the Stars" (Phil. Trans., 1880, p. 672) being employed.

In this photograph, in addition to the strong line about λ 3730, a pair of less conspicuous lines is seen on the less refrangible side of the strong line.

The continuous spectra due to the two of the four bright stars of the Trapezium which fell upon the slit are present.

Across these continuous spectra at least four groups of bright lines can be seen, of which the greater number can be traced into the nebula for some little distance from the stellar spectra.

It is scarcely necessary to state the importance of this observation as showing that these stars of the Trapezium are not merely optically connected with the nebula, but are physically bound up with it, and are very probably condensed out of the gaseous matter of the nebula. This observation would seem also to show that the nebula, as a whole, may not be at a distance from us greater than that which we should attribute to such stars, if they occurred alone in the heavens.

The first group, of six lines, occurs between λ 4116 and 4167. The lines of this group do not extend far from the continuous star spectra, with the exception of two lines. These can be seen faintly in another photograph taken in 1889. Beyond there is a fainter group, probably of four lines a little beyond *h*. I am pretty sure that these lines extend into the nebula. The third group from λ 3896 to 3825, of which I have endeavoured to measure ten lines, is faint, but here there is no doubt that the same lines are present in the adjoining nebular matter. There are two lines a little more refrangible than the strong line seen in 1882, at about λ 3709 and λ 3699. I have a suspicion of a faint group about this place, and also of another group on the less refrangible side of G.

I shall discuss further on, the probable chemical significance of these lines.

During the time that Orion was favourably situated for observation in the season of 1888 and in that of the present year, the unusual continuance of bad weather made it impossible for me to give so complete an account of the spectrum of the nebula in the photographic region as a few really fine nights would have enabled me to do. However, on February 28 of the present year I obtained another photograph, the slit being very narrow, which gives some more new information of the nature of its spectrum. I was astonished on looking at the photograph not to see the strong line about λ 3730, which was by far the most conspicuous feature of the photograph taken in 1888. The pair of lines near it on the less refrangible side, which I found for the first time in 1888, are present; and on a further scrutiny of the plate I discovered two other pairs of lines, most probably rhythmically connected with them, in the still more refrangible region, the last pair, accompanied by a third line, being near the ultra-violet limit of ex-terrestrial light.

I was also able to see faintly two of the bright lines which I have described as present across the continuous spectra of the brighter stars of the Trapezium in my photograph of 1888. It is not quite certain whether these very faint and short lines are really due to the matter of the nebula proper, or have come upon the plate in consequence of the stars of the Trapezium having fallen accidentally upon the slit for a time too short to impress the continuous part of their spectra. No trace of a continuous spectrum can be seen upon the plate, but these lines in the plate of 1888 do extend beyond the continuous spectra of the stars of the Trapezium.

I regret extremely that bad weather has made it impossible for me to work out the circumstances on which depended the disappearance of the strong line about λ 3730. Both the photographs which show this line include two stars of the Trapezium, and it may possibly be that this strong line is associated with the groups near it in the spectra of the stars, and may therefore come out in those parts of the nebula only which are more condensed. A few photographs with the slit differently placed upon the nebula would doubtless have thrown light upon this point. The suggestion presents itself strongly that the mottled and broken-up character of the nebular matter, shown in Lord Rosse's drawing from eye observations, and much more strikingly brought out in the recent photographs of Mr. Common and Mr. Roberts, may be connected with differences of spectrum in the photographic region, though in the visible region there is no known alteration of the spectrum of the four bright lines, except, it may be, some small differences of relative brilliancy of the lines.

Until next winter we cannot go beyond the new information which these photographs give to us. On the plate of the photograph of 1889 two pairs of spectra for comparison were taken: two spectra, one above and one below the nebular spectrum, of burning magnesium; and two spectra, similarly placed, of the light of the sky.

From the photographs of 1888 taken with a narrow slit, the position which I gave in 1882 to this line is shown to be, as I expected from the wide slit then used, approximate only. I find from the later photograph that the wide slit had caused the strong line to unite with a line near it, and that in 1882 I measured the middle of the broad band produced by the union of the wide images of two lines. Its position is about six tenth-metres more refrangible. It does not therefore agree, as I then suggested, with the hydrogen line ζ in my spectra of white stars. A statement of the position of this line relatively to the magnesium-flame triplet will be given further on, when I come to discuss the comparison of this spectrum with that of the nebula.

The position of the pair of lines a little less refrangible than this strong line, seen with it in the photograph of 1888, and present without the strong line in the photograph of 1889; and the positions of the two other more refrangible pairs, presumably connected with the first pair, are given in the following table:—

1st pair about	λ 3752·0 3741·0
2nd pair about	3285·0 3275·0
Line at about	3060·0
3rd pair about	3053·0 3047·0

In both photographs I suspect the indications of other lines, which are too faint to permit any certain conclusion to be formed about them, whether they are true lines, or imperfections only of the film.

[The continuous spectra of the stars of the Trapezium can be seen on the plate from about F to about λ 3570, but they are very faint beyond λ 3560.—*May* 18.]

The Visible Spectrum.

a. Brightest line.—In 1872 ("On the Spectrum of the Great Nebula in Orion, &c.," Roy. Soc. Proc., vol. xx. (1872) p. 383), I stated, as the result of numerous direct comparisons of this line with the brightest line in the spectrum of nitrogen, that the nebular line was "sensibly coincident with the middle of the less refrangible line of the double line of nitrogen." To avoid repetition I will call this line N_1. Except where it is otherwise stated, I use this line of nitrogen simply as a fiducial point in the spectrum, without any reference to its chemical significance.

In a still more critical examination of the position of the nebular line for the purpose of determining whether there was any indication of relative motions of the gaseous nebulæ in the line of sight, I found some experimental difficulty from the circumstance that the nebular line is narrow and defined, while N_1 is nebulous. I was fortunate to find a more suitable fiducial line of comparison in a narrow line of lead, which falls almost upon the middle of N_1 ("On the Motions of some of the Nebulæ towards or from the Earth," Roy. Soc. Proc., vol. xxii. (1874) p. 252). In December 1872, I compared this line directly with N_1, and found it sufficiently near in position to serve as a fiducial line of comparison.

Six other gaseous nebulæ were also examined, each on several nights, with the result that "in no instance was any change of relative position of the nebular line and the lead line detected" (*ibid.*, p. 253).

In the simultaneous observation of the nebular line and the lead line, it was found if the lead line was made rather less bright than the line of the nebula, the small excess of apparent breadth of this latter line appeared to overlap the lead line to a very small amount on its less refrangible side, so that the more refrangible sides of the two lines appeared to be in a straight line across the spectrum. The clearness of position of the two lines was shown by the observation that when the line of the nebula passed across the field of the spectroscope, and the lead line was thrown in, the lead line was not seen, but only an increase in brightness of the nebular line. By comparing the end of the nebular line near the Trapezium where it is refined to a point, I estimated that the difference of position of the middle of the lead line and that of the nebular line might be possibly from λ 0000·2 to λ 0000·3 (*ibid.*, p. 252). Some recent measures of the position of the lead line with the middle of N_1 show that the lead line is about λ 0000·12 more refrangible.

These direct comparisons of the nebular line with the lead line confirmed, therefore, my former conclusion, that the brightest line in the gaseous nebulæ is very near N_1, when seen under a dispersion equal to nearly eight prisms of 60, namely 36° 25′ from A to H.

This result is based on direct comparisons, on twenty-four different nights, with N_1 or with the line of lead.

The wave-length of N_1 has been determined by Kirchhoff, Thalén, and by myself. Watts's reduction of my measure to wave-lengths is clearly not accordant with my measures of air lines immediately preceding and following this line. I have therefore reduced my original measure to wave-lengths, and find for N_1 the value λ 5004·5.

Kirchhoff	5004·6
Thalén...	5005·1

Thalén's value is clearly too high, as Thalén gives for the lead line, coincident nearly with N_1, λ 5004·6, and N_1 is seen on the more refrangible side of the solar iron line given by Ångström as λ 5004·9. In Ångström's map N_1 is laid down on the more refrangible side of the iron line 5004·9, at about 5004·5. The same position is given to N_1 in Kirchhoff's map.

I have made a new determination of the position of N_1, using the second spectrum of a grating 17,300 to the inch, relatively to the solar iron line at 5004·9 according to Ångström.

The value came out λ 5004·6, which agrees with Kirchhoff's value, and with Thalén's measure of the lead line which falls upon it, and also with the maps of Ångström and of Kirchhoff.

The wave-length of the brightest nebular line may therefore be taken at from

λ 5004·6 to λ 5004·8 . . . . (1)

The micrometric measures of this line, given by D'Arrest, Vogel, and Copeland, agree closely with this value.

D'Arrest's[1] mean value	... ...	5004
Vogel's	,, ...	5004
Copeland's[2]	,, ...	5004

b. Second line.—In 1872 (Roy. Soc. Proc., vol. xx., 1872, p. 385), I stated that I had found this line, by comparison with a line of barium, and subsequently with an iron line, to have

A wave-length of	λ 4957·0 . . (2)
D'Arrest's mean value from micrometric measures	λ 4956·6
Copeland's mean value from micrometric measures	4958·0

c. Third line.—In my original paper (Phil. Trans., 1864, p. 437), "On the Spectra of some of the Nebulæ," in 1864, I showed, by direct comparison with hydrogen, that this line is undoubtedly the line of that gas at F of the solar spectrum. This observation was afterwards repeated (Phil. Trans., 1868, p. 545), and has been confirmed by the photographs of 1882 and 1888.

[1] "Undersögelser over de nebulose Stjerner" (Copenhagen, 1872, p. 23).
[2] *Monthly Notices R.A.S.*, vol. xlviii. p. 361.

The wave-length of this line is therefore	λ 4860·7 . . (3)
D'Arrest's value from measures	4860·6
Copeland's ,, ,, ...	4861·0

d. Fourth line.—In 1872 (Roy. Soc. Proc., vol. xx., 1872, p. 385) I stated that I had satisfied myself of the coincidence of this line with Hγ, but, on account of its faintness, it is very satisfactory to find this observation of coincidence confirmed by the photographs taken in 1888 and 1889. There can be no doubt that this is a line of hydrogen, and that

The wave-length therefore is	4340·1 . . (4)
Copeland's mean value	4342·0

Dr. Copeland gives the measures of two still fainter lines which he has seen in this nebula—namely, one at λ 5874, possibly coincident with D_3, and a line at λ 4476 (see also Mr. Taylor, *Monthly Notices R.A.S.*, vol. xlix. p. 125). I defer the consideration of these and other faint lines which I have often suspected in the faint continuous spectrum of the nebula, as in consequence of the great strain upon the eyes from my recent direct comparison of the spectrum of the nebula with the spectrum of burning magnesium, I was not able, during the very few fine nights when Orion was favourably situated, to undertake an examination for these very faint lines.

Comparisons with the Magnesium-flame Spectrum.

In 1882, Dr. Copeland, in his paper on Schmidt's Nova Cygni (*Copernicus*, vol. i. p. 109), remarked in a foot-note, "that it is worthy of note that this line (λ 5006·5 of burning magnesium) almost absolutely coincides with the brightest line in the planetary nebulæ."

This line—namely, the bright edge of the first band in the magnesium-flame spectrum—is very near in position to the brightest nebular line. We have seen that the wave-length of this line in the nebulæ (1) is 5004·6 to 5004·8; now the wave-length of the end of the magnesium-flame band is 5006·5, consequently it does not coincide with the nebular line, but falls on the less refrangible side at a distance of λ 0002 nearly from that line.

The wave-length of the termination of the magnesium-flame band is, as determined by—

Lecoq de Boisbaudran	5006·0
Watts	5006·5
Liveing and Dewar[1]	5006·4

I have recently determined the position of the end of the band, by direct comparison with the solar iron line given by Ångström at λ 5006·58.

My result places the magnesium-flame band line at λ 5006·5 . . . . (5)

In a paper read before the Royal Society in 1887,[2] Mr. Lockyer says:—"Only seven lines in all have been recorded up to the present in the spectra of nebulæ, three of which coincide with lines in the spectrum of hydrogen, and three correspond to lines in magnesium. The magnesium lines represented are the ultra-violet low temperature line at 373, the line at 470, and the remnant of the magnesium fluting at 500, the brightest part of the spectrum at the temperature of the bunsen burner." At p. 137 (*loc. cit.*) Mr. Lockyer says:—"In the nebulæ we deal chiefly with lines seen in the spectrum of magnesium at the lowest temperature."

In a later paper in 1888 ("Suggestions on the Classification of the various Species of Heavenly Bodies," Roy. Soc. Proc., vol. xliv. p. 21) Mr. Lockyer states:—"In a paper communicated to the Royal Society on November 15, 1887, I showed that the nebulæ are composed of sparse meteorites, the collisions of which bring about a rise of temperature sufficient to render luminous one of their chief constituents—magnesium. This conclusion was arrived at from the facts that the chief nebular lines are coincident in position with the fluting and lines visible in the bunsen burner when magnesium is introduced, and that the fluting is far brighter at that temperature than almost any other spectral line or fluting of any element whatever."

Although the number of direct comparisons which I had made of the brightest line in the nebulæ with N_1 and with the lead line, not to speak of the accordant results of the micrometric measures of other observers, left little doubt in my mind that this line could not be coincident with "the remnant of the mag-

[1] Roy. Soc. Proc., vol. xliv., 1888, p. 245.
[2] "Researches on the Spectra of Meteorites: a Report to the Solar Physics Committee." Communicated to the Royal Society at the request of the Committee. Roy. Soc. Proc., vol. xliii. p. 118.

nesium fluting at 500," really at 5006·5, yet I thought it desirable to undertake the laborious task of comparing, with the necessary care and precautions, the nebular line directly, in the spectroscope attached to the telescope, with the spectrum of burning magnesium.

Arrangements were made by which the light from burning magnesium was thrown into the telescope from the side and then reflected down, under conditions similar with the light from the nebula, upon the slit of the spectroscope. By this arrangement any flexure in the tube connecting the spectroscope with the telescope would affect both spectra alike. The coincidence in position of the spectrum from burning magnesium with that of a heavenly body to which the telescope was so directed that its light fell upon the slit of the spectroscope, was tested with great care on several occasions by comparing the three bright lines of magnesium with the corresponding lines, b_1, b_2, b_4, in the spectrum of the moon. Indeed, to prevent any possible error in the observation of apparent want of coincidence of the nebular line, if the light from the burning magnesium should by an accident so come upon the slit as to bring its spectrum in a very minute degree on the less refrangible side of its true position relatively to the nebular line to be observed with it, the arrangement was purposely made that the lines of magnesium were seen to fall upon the corresponding dark lines at *b* in the moon, a very little on the more refrangible side of the middle of those lines. This state of things would diminish a little the interval which should be seen between the nebular line and the edge of the magnesium-flame band, and so make the determination more difficult; but if under such circumstances the nebular line was seen on the more refrangible side of that of magnesium, the observation would be much more trustworthy, for in the case of coincidence with magnesium the line would have appeared towards the opposite and less refrangible side of the magnesium line, broadening the magnesium line on this side. I considered that the comparison could be made most satisfactorily by the complete superposition of the two spectra, that from burning magnesium being gradually reduced in brightness by the interposition of coloured glass screens, until the ground of the spectrum between the successive bright lines of the band of the magnesium-flame spectrum was sufficiently subdued to allow of the nebular line being seen upon it.

Under these circumstances, if the nebular line had the position which my direct comparisons and the micrometric observations of other observers assign to it, it would be seen as a bright line at a very small interval within the line ending the band, and to the observer the band would appear to commence with a double line.

This direct comparison was first successfully made on March 6, 1889. The observations were made with the 15" refractor belonging to the Royal Society. The spectroscope used has two compound Grubb prisms, each with 5 square inches of base, and giving nearly twice the dispersion of a single prism of 60°, namely, 9° 20' from A to H; and collimator and telescope of 1·25-inch aperture. An eye-piece magnifying eighteen times was employed. The nebular line was brought upon the cross-wires, and when carefully focussed and clearly seen, the light from burning magnesium was thrown in. This observation is one of great difficulty, especially as the interval to be observed had been purposely reduced by causing the magnesium to fall, for the sake of the greater trustworthiness of the observations, on the more refrangible side of its true position. Although I consider the results to be satisfactory, I prefer to say that I, and Mrs. Huggins independently, believed fully at the time that we saw the appearance which all former observations of this line led me to expect—namely, the nebular line to fall within the termination of the magnesium band, and to form with the band-boundary a double line. The relative positions of the two spectra are represented in diagram across the page. The line at the end of the magnesium band was then brought upon the cross-wires, without any attention being given to the nebular line; when the burning magnesium went out, the nebular line was seen to be at a measurable distance to the left of the intersection of the wires—namely, on the more refrangible side.

When the object glass of the telescope was covered, the magnesium band presented its usual appearance—namely, terminating in a single line. These comparisons were repeated and confirmed generally on March 9, March 11, and March 16. On March 9 a single successful comparison was made with a more powerful spectroscope, giving a dispersion equal to nearly eight prisms of 60°. [Comparisons have been made since with the planetary nebula in Hydra. The short line of the nebula was found to fall within the termination of the magnesium band at about the small distance which corresponds to the known position of the two lines.—*April* 26.] On all these nights the comparisons were repeated independently and fully confirmed by Mrs. Huggins.

These comparisons can be successfully imitated in the laboratory by directing a spectroscope of sufficient power to the line of lead which the nebular line is sufficiently near, the slit being narrow, and the electrodes of lead near each other; and then causing, with the necessary precautions, the light of burning magnesium to fall also upon the slit. The lead line will be seen to fall within the end of the band, and to form with it a double line.

It may be mentioned in this place that this line of lead, and the iron line at 4957 at the position of the second nebular line, can be conveniently used in the laboratory in any chemical research on the nature of the nebulæ. No terrestrial line which does not fall almost exactly at these positions in the spectrum can have any claim to further consideration.

(*To be continued.*)

PARIS.

Academy of Sciences, August 12.—M. Des Cloizeaux, President, in the chair.—Remarks in connection with the "Introduction à l'étude des Races humains," Part II., presented to the Academy by M. A. de Quatrefages. In this second part the author passes from the general questions touching the evolution of man to those touching the evolution of the human races, of which he reckons at least one hundred and seventy-two, exclusive of minor varieties, all however reducible to the three fundamental black, yellow, and white stems. Adhering to the natural method of classification, as understood by Jussieu and Cuvier, he divides these stems into *branches* corresponding to primary and secondary divisions, under which come the *families* and *groups*. Much stress is laid on the early migrations of man, resulting in crossings of all sorts, and the general displacement of pure by half-caste races. The position of fossil man in the general scheme of classification is also studied, the five or six known varieties discovered in Europe being divided into two distinct branches allied to the white stock. Two distinct Quaternary types are also recognized in America, that of the Pampas affiliated to the Siberian, and that of the Lagoa Santa to the Eskimo branch of the yellow stock. Some bold speculations are indulged in regarding the primæval homes of the three fundamental groups and their subsequent dispersion from common centres over the face of the globe. In this scheme the north of Asia is considered the cradle of mankind, whose three primary divisions grouped themselves round the great central tableland, whence they gradually spread over the continents during Tertiary and Quaternary times. In the Oceanic world the Eastern Polynesians are affiliated to the white, the Melanesians (Papuans) to the black, and the Malays to the yellow stock, each division migrating from the mainland in the order already indicated by Prof. Keane ("Indo-Chinese and Inter-Oceanic Races and Languages"). The work is illustrated with 441 figures inserted in the text, four plates, and seven maps.—On a general law of induction in circuits devoid of resistance, by M. G. Lippmann. In this study the author proceeds exactly as in rational mechanics, where the fundamental laws are established, apart from the disturbing element of friction, which is afterwards considered as a particular force. He seeks the most general law of induction by assuming resistance to be null, and justifies this procedure *a posteriori* by the simplicity of the results, by their easy application and agreement with recent experiments.—On the vascular apparatus in animals and plants, studied comparatively by the surgical and thermochemical methods, by M. Sappey. In previous communications the author showed that in the vertebrates the thermochemical method is not only useful but preferable to the older process for the study of certain organs, especially the harder parts. In the present paper he directs attention to the structure of the vascular apparatus in plants and animals studied both by the surgical and thermochemical methods. The latter process is shown to yield as complete, accurate, and satisfactory results as could be desired; it is unquestionably superior to the former in the analysis of the veins and arteries of animals, and of the woody and other vessels of plants.—The virus of diphtheria, by M. C. H. H. Spronck. The author communicates the results of the researches on human diphtheria made by him, jointly with MM. Wingtens, Van den Brink, and Van Herwerden, in Utrecht. The disorder is studied chiefly in connection with its action on the region of the kidneys.

These researches seem to leave no doubt that the bacillus of Klebs is the real active principle in diphtheria.—On the rotatory polarization of quartz, by M. H. Le Chatelier. Having already shown (*Comptes rendus*, May 20, 1889) that towards 570° C. the dimensions of quartz undergo a rapid increase, if not an absolutely sudden change such as that noticed in dimorphic transformations, the author here endeavours to place beyond doubt this sudden change by resuming the study of the rotatory polarization of quartz already begun by M. Joubert. From his investigations it seems evident that at the specified temperature this body really undergoes a sudden allotropic transformation while retaining under both states its rotatory power as well as its crystalline symmetry.—On the production of crystallized cobaltous and ferrous hydrates, by M. A. de Schulten. The author has obtained these hydrates in the crystallized state by using the same method that has enabled him to prepare artificial brucite and pyrochroite, as explained in the *Comptes rendus*, ci. p. 72, and cv. p. 1265.—Papers were contributed by M. C. Patein, on a source of error in the search for, and quantitative analysis of, albuminoid substances in the animal organism; by M. Maupas, on the agamous multiplication of some rotifers and other lower Infusoria; and by M. Th. Moureaux, on the cause of certain disturbances in the curves of the magnetographs.

Berlin.

Physiological Society, July 26.—Prof. Munk, President, in the chair.—Prof. Zuntz spoke on heat-regulation in man, basing his remarks on experiments made by Dr. Loewy. The store of heat in the human body at any one time is very large, equal in fact to nearly all the heat produced by the body during twenty hours, hence the heat given off to a calorimeter during a given period cannot be taken as a measure of the heat-production; this determination must be based rather upon the amount of oxygen consumed, and of carbonic acid gas given off. The purpose of the experiments was to ascertain what alteration the gaseous interchange of the body undergoes by the application of cold, inasmuch as existing data on this point are largely contradictory. The observations were made on a number of men whose respiratory gases were compared, during complete rest, when they were at one time clothed, at another time naked, at temperatures from 12° to 15° C., and in warm and cold baths. Each experiment lasted from half an hour to an hour, during which period the gases were repeatedly analyzed. As the result of fifty-five experiments, twenty showed no alteration of oxygen consumption as the result of cooling, nine gave a lessened consumption, while the remaining twenty-six showed an increased using up of oxygen. This diversity of result is explicable on the basis of observations made by Prof. Zuntz, who was himself experimented upon, as to his subjective heat-sensations during the experiments. He found that after the first impression due to the application of cold is overcome, it was quite easy to maintain himself in a perfectly passive condition; subsequently it required a distinct effort of the will to refrain from shivering and throwing the muscles into activity, and finally even this became no longer possible, and involuntary shivering and muscular contraction supervened, as soon as the body-temperature (*in ano*) had fallen ½° to 1° C. During the first stage of cooling, Zuntz's oxygen consumption showed a uniform diminution; during the period also in which shivering was repressed by an effort of the will, cooling led to no increased consumption of oxygen, but as soon as shivering became involuntary there was at once an increased using up of oxygen and excretion of carbonic acid. This explains the differences in the results of Dr. Loewy's experiments, and may be taken to show that in man, and presumably in all *large* animals, heat-regulation as directly dependent upon alteration (fall) in temperature of the surrounding medium does not exist; the increased heat-production is rather the outcome of the movements resulting from the application of cold to the body. In *small* animals, on the other hand, there undoubtedly exists a heat-regulation dependent upon an increased activity of chemical changes in the tissues set up by the application of cold to the surface of the body, and in this case the thermotaxic centres in the brain most probably play some part.—Dr. Herter gave an account of experiments made by Dr. Popoff on the artificial digestion of various and variously cooked meats. Lean beef and the flesh of eels and flounders were digested in artificial gastric juice; the amount of raw flesh thus peptonized was in all cases greater than that of cooked meat similarly treated. The flesh was shredded and heated by steam to 100° C. The result was the same for beef as for fish. When compared with each other, beef was on the whole the most digestible, but the amount of fish-flesh which was peptonized was sufficiently great to do away with the evil repute which fish still has in Germany as a proteid food. Smoked meat differed in no essential extent from raw meat as regards its digestibility.—Dr. Cowl described his experiments on the mechanical latent period of a muscle. The muscle was hung up by one end, and its movements were recorded by a lever passing through the middle of the muscle, and writing on a spring-myograph. When both electrodes (using a "breaking" induction shock) were applied to the half of the muscle above the lever, the curve obtained showed a short latent period, after which it rose above the base-line. When the electrodes were applied below the lever, the same latent period was observed, after which the curve first fell slightly below, and then rose above the base-line. When one electrode was applied above and the other below the lever, then the elongation of the muscle was now present, now absent; in the latter case the length of the latent period was equal to the latency plus the duration of the visible elongation. From this it follows that the elongation of the muscle accounts for part of the latent period.

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