THURSDAY, MAY 24, 1877

SCIENCE AND WAR

II.

AT no other time has there been so much want of unanimity among the Great Powers of Europe on the subject of Ordnance. There are to be found at the present moment cannon of a dozen different descriptions in the gun parks of European nations, differing from each other not only in respect to their construction, but in the metal of which they are made. So far as small arms are concerned, we know there is but one opinion; some nations prefer one breech-loader to another, but all agree in the employment of breech-loaders. In the case of cannon, however, it is different. Germany relies upon breech-loading ordnance, while Great Britain has forsaken the system and gone back to muzzle-loaders; Austria makes her guns of bronze, Germany of steel, Russia favours steel and brass, America cast iron, while England has cannon of steel encompassed with iron, and France weapons of iron girt with steel.

The balance of favour is beyond question with the breech-loader at the present moment. All the new artillery of the Russians and the Turks is of this kind, while the field-guns both of the Germans and Austrians are upon the same system. France has done nothing lately for the regeneration of its ordnance, and there remains but Great Britain and Italy to represent muzzle-loading artillery. But Italy, although she has adopted the British system for very heavy guns, is by no means a confirmed believer in it, and will doubtless hesitate before following our example very far, beset, as she is, with neighbours armed with breech-loaders.

Of all the Powers, it is, curiously enough, steady-going Austria, which has taken the boldest and most independent course in the matter of artillery. It was but at the end of 1875 that the Austrian War Office decided to adopt the Uchatius cannon for field artillery, and yet at this moment every artillery regiment of the vast Austro-Hungarian army is armed with the new weapon. Within eighteen months no less than 2,000 of these cannon have been cast and finished, and now the Vienna arsenal is engaged in the manufacture of heavy guns of the same character. Never was a more energetic step taken. A new cannon of some sort was held to be absolutely necessary to uphold the prestige of the army, and a Commission having been intrusted with the selection of an arm, pronounced without delay in favour of the scheme brought forward by Gen. von Uchatius. In October, 1874, the first round was fired from an Uchatius gun, and a twelvemonth afterwards the sweeping reform which was to introduce an entirely new artillery throughout the Austrian service was decided upon. Government sanctioned an expenditure of 1,800,000*l.* to be spent in two years, and Gen. von Uchatius was directed to give all the assistance in his power towards the fulfilment of the design.

The Uchatius gun is made of so-called steel-bronze. Chilled bronze would be a better name, since Uchatius casts his metal in a chilled, or metal mould, in the same manner, pretty well, as Sir William Palliser produces his famous chilled projectiles. Bronze, as everybody knows, has been a favourite metal with gun-founders from the earliest days, and in the East, especially, magnificent castings of this nature have been produced. About 90 per cent. of copper and 10 of tin is the mixture commonly employed in making ordinary bronze, but 8 per cent. of tin is the proportion preferred by Uchatius. The difficulty in casting bronze, as those who have any experience know full well, is that of securing homogeneity, soft particles of tin becoming isolated in the mass, and giving rise to the defect known as "tin-pitting." Whether we have lost the secret of bronze-casting, or whether in former times they were more skilful at the work, certain it is that founders of the present day are unable to secure so uniform an alloy as formerly. This was very apparent when some eight or ten years ago our own Government adopted, for a brief time, bronze artillery. The addition of a small percentage of phosphorus did not mend matters, and the highest authorities on the subject were at a loss to suggest an effective remedy. Our bronze guns, too, had another defect which could not be overcome. After firing the bore became affected, and the weapon, as it was termed, "drooped at the muzzle." These were the two defects indeed that led mainly to the abandonment of the bronze gun in this country, and they are, too, the difficulties which Gen. von Uchatius appears to have overcome. He has got rid of "tin-pitting" and his guns do not "droop at the muzzle."

Uchatius found that by subjecting the alloy in a liquid form to considerable pressure, he was enabled to secure a perfectly homogeneous mass, a result which was also furnished, he discovered when he had gone a step farther, if the molten metal was rapidly cooled. Steel-bronze is apparently made much in the same way as the toughened glass, of which we have heard so much lately. After being cast in a mould, the alloy is thrust into a reservoir of oil, heated to a high temperature, so that the metal suddenly cools, but only down to a certain point. Then the casting is withdrawn and allowed to get cold more gradually. A regular and crystalline structure is in this way produced, which has none of the defects of ordinary bronze. It is a moot point whether phosphorus enters into the composition at all. Chemists tell us they can find no trace of it, but this is no absolute proof that a small percentage of the element was not originally contained in the alloy, being burnt out after it had done its work of harmonising the two metals. The inventor is rather reticent on the point, but in any case, it is very certain that he produces a uniform and homogeneous alloy of a hard crystalline nature.

One other expedient Uchatius has recourse to in making his cannon. When he has cast his gun and chilled it, he proceeds to dilate the bore. Wedges of steel, shaped in the form of cones are forced into the tube of the gun one after another, until the calibre of the weapon has been increased by something like seven or eight per cent. This expansion or dilation of the tube has not only the effect of hardening or steeling the core, but also of rendering the gun more elastic and capable of resisting more effectually the strain put upon it at the moment of firing. The gun, after this process, is in a state of elastic tension, and it is said that there is a pressure from without, inwards, equal to that which was exerted to dilate the gun in the first instance; and that this is actually the case can scarcely be doubted, since it is a fact that a section of the gun before being quite severed, will tear itself loose with considerable violence, and will be found on separation to have partially returned to its former calibre.

So far as practical trials have been conducted with the weapon, the Austrian Government have every reason to be satisfied with the Uchatius gun, which compares favourably with the Krupp steel cannon in the matter of accuracy and durability; while as regards its cost, it is far cheaper than any other rifled ordnance. A steel field-piece costs upwards of 100*l.*, even when not protected with rings, while the iron-steel weapon manufactured in this country, costs about 70*l.* sterling; the steel bronze cannon of Gen. von Uchatius, on the other hand, are made for 35*l.* apiece.

In construction, the Austrian gun is so similar to that of Herr Krupp, of Essen, that the latter claimed compensation for an infringement of his patent when the manufacture of the Uchatius gun was first commenced. The Essen works, our readers may know, supply not only Germany with steel breech-loaders, but have provided the present belligerents with all their modern artillery. Russia has still many brass cannon on hand, and Turkey a goodly number of Armstrongs, but both powers mainly depend upon their steel Krupps. These stood the German army in such good stead during the last war that their reputation is firmly established. They are of crucible steel, and the breech, instead of being upon a hinge, or in the form of a block, moves round in a D-shaped socket, the escape of gas being further prevented by rings of phosphor-copper.

The manner in which the ordnance of this country is constructed is sufficiently familiar to our readers. A tube of steel is encompassed by jackets of wrought-iron, and in this way the toughness of the latter is combined with the hardness of the former. All our guns, as we have said, load at the muzzle, while those of Russia, Germany, Austro-Hungary, and Turkey, are breech-loaders. Italy, in the case of the 100-ton guns with which she intends to arm her two stupendous turret-vessels, the *Duilio* and *Dandolo*, has adopted our method of construction, except that she employs smooth, instead of studded, projectiles. With the employment of a gas-check at the base of the shot to prevent windage and so secure the full force of the exploding charge, the use of studs in a shot appears to be unnecessary, a sufficient spin being imparted to the projectile by the soft metal of the gas-check before-named, which causes the shot to rotate after the manner of a Snider bullet. So satisfactory, indeed, were the Italian trials of these projectiles last year that it is by no means improbable that we, too, may give up the use of studded shot.

As to the comparative value of breech-loaders and muzzle-loaders, we shall not offer an opinion. No doubt a muzzle-loader is the stronger weapon, because its breech is solid, but our cousins, the Germans, urge very justly that since their guns do not burst, they are quite strong enough. Advocates of the muzzle-loading system argue again that their weapon is more simple in construction and for this reason is to be preferred; but on the other hand the sponging and loading of a gun is more easy to effect, if it opens at the breech. Indeed, in the case of very heavy guns located in a casemate or on board ship, the Germans reproach us with the assertion that we must needs have recourse to all sorts of complicated and awkward machinery in loading, while in their case a simple pulley or crane is all that is necessary. Either, say they, we must expose our gunners through the open port when loading, or, as in the case of the *Thunderer*, rely blindly on hydraulic apparatus to work the guns for us. So stands the question; perhaps the present war will bring us a solution of it.

H. BADEN PRITCHARD

THE FORESTS OF PEGU

Preliminary Report on the Forest and other Vegetation of Pegu. By Sulpice Kurz, Curator of the Herbarium, and Librarian, Royal Botanical Gardens, Calcutta. (Calcutta: C. B. Lewis, 1875.)

INDIAN forest reports have of late years become as plentiful as the proverbial blackberries. The frequent appearance of them is a consequence that might be expected when we consider the wide range of country which comes under the supervision of the Forest Department of India. So far as bulk or quantity of printed matter is concerned, no one can say that these forests are not fairly represented in the Government papers which appear in the course of a year, but the quality of these reports is another question. They too often contain merely the dry details of work carried on during the year, and are interesting only to those immediately connected with the special department from which the reports emanate. Occasionally, however, a report is issued which in reality is something more, containing much valuable information on subjects connected with forest conservancy, and amongst such Mr. Kurz's may be classified. It is, in fact, rather a description of the vegetation of Pegu, to which are added appendices occupying quite two-thirds of the whole bulk of the volume. Taking the actual report itself, which, as indicated in the title, is of a preliminary character, the matter in which will be worked out in Mr. Kurz's forthcoming book, we find it divided into two parts, first, the "General Report," and second, the "Special Report." The general report is again divided into two sections—(A) A general aspect of the country, its geological and climatological features, in connection with the flora. (B) A botanical description of Pegu, with special reference to its forests. After a very brief topographical sketch of Pegu, Mr. Kurz considers the geological aspect of the country from a botanical point of view, which, unlike that of the true geologist, is not to consider the age of the rocks, &c., but simply their extent and quality, from which inferences may be drawn of the vegetation found growing upon each formation. The geology of Pegu is described as being very simple and uniform, the hills being composed solely of sandstone, skirted at their base by a strip of diluvium, "interrupted by a deeper or shallower alluvium wherever choungs come down from the hills, and succeeded by the vast alluvial plains, through which the Irrawaddy and Sittang flow." The laterite formation is described as being of the highest importance in the various floras of India. The term laterite, as generally used by foresters in Burmah, comprises several heterogeneous rocks and soils, all characterised by a more or less ferruginous appearance, but really connected in no other way than that they are all permeated by hyperoxide of iron. "No other formation," Mr. Kurz writes, "except metamorphic and volcanic ones, can boast of such a variety of species, in spite of its apparent sterility, as laterite. It is this rock that affects vegetation so much that the great difference between the floras of Malacca, Borneo, Sumatra, &c., on the one hand, and that of Java on the other side, is produced. It is also this formation which allows so many Australian genera, like *Melaleuca*, *Bæckea*, *Tristania*, *Leucapogon*, &c., to spread so far to the north-west, some of which, like *Tristania*, spread as far north as the Ava frontier. If all laterite plants were to be erased from a list of the plants of Pegu proper the flora would be rendered very uninteresting indeed."

The seasons of Pegu seem to be similar to those of Lower Bengal; the cold season, however, is shorter, and the hot, dry, as well as the rainy seasons, are earlier by a month than in Calcutta. The dry season ranges from about December to April, the cold part of which terminates usually, and sometimes, abruptly about the end of February; during this period the thermometer rarely rises above 88° in the shade, sinking as low as 57° and sometimes to 55° and 54° before sunrise. Heavy dews prevail and fogs are plentiful in the early hours of the day, the after parts of which are clear and bright. During this season rain is almost unknown. The hot part of the dry season comprises the months of March and April, during which time, chiefly in the former month, an occasional heavy thunderstorm moderates the intense heat. In the early part of May the regular monsoon rains, which cease during November, set in. The thermometer, during the hot season, rises rapidly to 95° and 100° in the shade. The nights, however, are cool and refreshing. In the hottest province of the country, at the height of the season, the thermometer never registered above 74° before sunrise. Great heat and dryness prevails in the open country, and on the ridges, "while in the narrow valleys of the eastern slopes of Yomah, and in the Martaban hills, where evergreen forests skirt the streams, dew often falls so heavily, that one becomes quite wet when marching in the early mornings through the herbage along their bank. But after an ascent of 100 or 200 feet, we meet with the same dryness again in the deciduous forests, as in the open lands. It is here that we can almost every morning observe a white sheet of vapour in the depths of the valleys, resting on the forests, which enables us to appreciate clearly the *rôle* which evergreen forests play in the attraction of the currents of vapour."

The foregoing abstracts will serve to show the nature of Mr. Kurz's report, a good deal of which is interesting. It might, however, have been considerably condensed without losing any of its value; indeed its value would have been much enhanced.

Whatever advantages or disadvantages Burmah may present to travellers generally, to a botanist the advantage must be very great in having specimens ready gathered, and only requiring to be picked up, for we are told that owing to the extreme violence of the gales which prevail at the end of April and the beginning of May, the amount of old trees, branches, &c., thrown down is often astounding, offering an easy and fruitful harvest of specimens of woody plants, otherwise quite out of reach on account of their height. At other seasons apes and squirrels are most useful agents for procuring flowers and fruits of lofty trees. Another advantage is that in the cold season there is an absence of mosquitoes. Space will not allow us to do more than mention the appendices which we have before referred to. In the introduction to the first, which is a list of Burmese trees arranged in their natural orders, with brief remarks as to the quality and appearance of the wood, &c., we are treated to some remarks on the value or otherwise, of native names of plants. Mr. Kurz says, "there are still very respectable botanists and practical men who look upon native names for plants as something absolutely reliable, some even believe that native names are preferable to scientific ones, because the former are permanent, and are not altered from one day to another, as is the case in science." Our own experience is that while a native name, is often a great help to the accurate determination of the genus or species, it is, on the other hand, often a delusion and a snare, for it frequently occurs that the same name is applied indiscriminately to plants even of distinct natural orders. This, perhaps, is not so much the case with Indian plants as with plants from other countries. Mr. Kurz points out the difficulty that must always present itself in India, where the same plant is known in different localities by different names, whereas the scientific name would be identical all the world over, or if not actually identical, certainly traceable. This lengthy report concludes with some extracts from the author's journal of his tours. It is illustrated with a sketch map of Pegu, and two plates of a new genus *Mayodendron*, named in honour of Lord Mayo, and one of the appendices contains a botanical description of this plant.

INFLUENCE OF CLIMATE ON PULMONARY CONSUMPTION

Influence of Climate in the Prevention and Treatment of Pulmonary Consumption. Lettsomian Lectures for 1876. By Charles Theodore Williams, M.A., M.D., Oxon. (London: Smith, Elder, and Co., 1877.)

THE fact that consumption is the great destroyer of men and women in the prime of life, nearly one-eighth of all the deaths which occur being due to it, is more than sufficient to warrant any amount of minute and patient inquiry which might result in the prevention and more successful treatment of this terrible disease. Dr. Williams treats the subject in its climatic relations, and gives what is, in many directions, an elaborate and able discussion of a large amount of fresh data adduced with reference to the therapeutic action of British, Mediterranean, African, Indian, Australian, sea-voyage, and other climates, differing widely from each other as regards temperature, humidity, elevation, and exposure to sudden changes of weather. In concluding the inquiry he draws some valuable conclusions as to those patients who ought to winter abroad, and those on the other hand who may remain at home, those who are most likely to be benefited by sea-voyages, and those most benefited by dry climates, and how far the temperature and elevation is to be taken into account. The question of moist climates, whether hot or cold, is also examined with the view of ascertaining whether such climates are desirable at all for consumptive patients. The results would have been rendered even more valuable if the author had availed himself more freely of the labours of others who have written on the same subject.

But it is the remarkable conclusion arrived at regarding the winter climates of the south of England which arrests attention. This conclusion is that Torquay, Bournemouth, Ventnor, and the whole of the western end of the English Channel possess climates less beneficial to consumptive patients than are the climates of Hastings, St. Leonards, and the eastern end of the Channel; and it is thence inferred "that it is the stronger influence of the Atlantic warm current and its accompanying winds on the shores of Devonshire and Cornwall, which though it raises their winter temperature many degrees, clothing their hillsides with verdure and causing what would otherwise be waste places to bloom with rare exotics, deprives them of the stimulating and bracing influence which is possessed by the less beautiful shores of Sussex with its breezy downs and colder winter climate."

It may be doubted if the facts warrant this very broad conclusion. It has been shown by Buchan and Mitchell in their discussion of the weather and mortality of London, published in *Jour. Scot. Meteorol. Society*, vol. iv., p. 205, that the three periods of the year most fatal to consumptive patients are November and the first half of December, when the temperature is rapidly falling and is at the same time low; in January when the temperature falls to its annual minimum; but chiefly from March to about the middle of June, when the air is driest. Now these results, which are based on the enormous population of London and the long period of thirty years, would have led to the expectation that the winter and spring climate of Torquay was certainly not less beneficial to consumptive patients than that of St. Leonards.

It seems not improbable, from an examination of the whole facts, that the discrepancy may be accounted for by the comparatively small number of patients whose cases have been discussed by Dr. Williams, viz., 243, of whom 100 were under medical treatment at Torquay, 58 at Ventnor, 57 at Hastings, and 20 at Bournemouth, and to an important point missed in the discussion, viz., the very different types of weather which have prevailed in the different years and the varying mortality from consumption attending on these types of weather. It is indispensable in such a discussion that tabular statements be prepared, showing the number of patients under medical treatment for consumption at each place during each month of each year, and the results of the treatment as respects each patient, in order that the results may be compared with the meteorology of the place and year to which they refer. Till this be done we cannot be said to be in a position to make any comparative statement of the therapeutic effects on consumptive patients of the climates of the different sanatoriums of the south of England; it being evident, for instance, that the relatively high position of Hastings as a sanatorium for consumption may be wholly due to a chance excess of patients sent there during exceptionally mild seasons, and the relatively low position of Bournemouth to the mere accident of one or two consumptive patients more than the average being there in a particular season when the weather happened to be peculiarly severe.

OUR BOOK SHELF

Annals of the Astronomical Observatory of Harvard College. Vol. xiii.

WE must congratulate the authorities of Harvard College on the publication of the Annals of the Observatory. The volume is the result of a large portion of the work of the institution during the time that it has been without a director, since the death of Prof. Winlock in June 1875. An account is given of the several funds available for purpose of publication, and then follow the biographical notes of W. C. Bond, G. P. Bond, and J. Winlock, the several directors since the foundation in 1815. We then come to the details of the instruments in use and plans of the observatory and grounds. The work done from the year 1855 appears to have been chiefly the measurement of binary stars, transit observations, investigation of lunar phenomena, drawings of nebula, photographs of the sun, and spectroscopic observations, the latter consisting largely of the examination and drawing of the chromosphere. In Part II. we find some thirty-four exquisitely-finished plates depicting the results of the foregoing observations, published at the expense of the Bache fund. These were made by or under the direction of the late Prof. Winlock. It seems a pity that the authorities do not publish from time to time a selection of these papers on special subjects. Part II. would be widely bought by astronomers if its contents were given separately, and the plates, which are, perhaps, the finest accessible, were practically not buried in an odd volume of a lengthy series of "annals."

Cultivated Plants; their Propagation and Improvement. By F. W. Burbidge. (Edinburgh and London: William Blackwood and Sons.)

THAT Mr. Burbidge possesses the pen of a ready writer no one can deny when it is borne in mind that in a very short time he has produced several books on horticultural or gardening subjects. His "Domestic Floriculture," published by the same firm as the present volume, was, up to that period, the best of his productions; for though it was not of a scientific character, it was of a nature calculated to elevate window gardening from the mere habit of simply allowing a few ordinary plants to struggle for an existence through adverse circumstances to a system in which all might take an interest.

The present volume is one of a different character from any of those which have preceded it. Mr. Burbidge, in fact, says in his Preface that the primary intention of the book was as a popular handbook on plant propagation and improvement, with a hope also that it might "serve young gardeners as a stepping-stone to works of a higher scientific character, and more especially to those of Charles Darwin." Nevertheless, the chapters or sections devoted to "Hybridising and Cross-breeding," "Natural Fertilisation and Cross-breeding," and "Artificial Fertilisation and Cross-breeding," will be useful as bringing together from various and widely scattered sources, what has been done in these cognate branches of scientific research. In these sections we think Mr. Burbidge has done his work well, the references to the quotations being fully given not only to English but also to French and German works.

The great bulk of the volume is devoted to a "General review of some of the most popular groups of cultivated plants, with notes on their propagation and natural affinities." In this the arrangement of the orders is somewhat novel, for instead of being classified in a scientific manner they are placed alphabetically. The habits and peculiarities of the principal plants in each order are briefly described as well as their economic uses, together with notes on the most general method of propagation.

The book will no doubt meet with a wide circulation; the chapters on propagation, grafting, and budding, being of a practical character, will be useful to other readers besides those of a purely scientific turn. As a further illustration of this we may point to the chapter on Seed-saving, in which we are reminded of the excellence of the produce of the Continental seed farms, especially those of Erfurt, which are noted for their Primulas, Stocks, Balsams, Asters, &c.; we are also told that the seed of such common plants as Cineraria and Calceolaria is, when of a "good strain," worth from 10*l.* to 15*l.* per ounce, and Primula seeds even more. The book has a good index, always a special point of value in one intended for reference.

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. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The "Hibernation" of Birds

I TRUST your correspondent "X" will not object to my pointing out that the "hibernation" story which he retails in NATURE (vol. xvi., p. 43) has in common with dozens, not to say scores, of others, the defect of being delivered at second-hand, without even the slightly confirmatory evidence which the name of the observer of the marvel and of the place where it occurred would supply. As such it may surely be dismissed to the limbo of (I will say) legends. That which has been furnished by the Duke of Argyll (vol. xv., p. 528) rests on a better base, and is almost the first I have met with for which a respectable man vouches. Far be it from me to deny the possibility of a miracle being wrought in Persia, but I confess that without impugning Sir John McNeill's veracity, I simply refuse to believe the story except I regard as miraculous the incident he relates. Now there is a common supposition that miracles are only performed with some worthy end in view, and, moreover, that unless a miracle is recognised as such by the witnesses thereto its effect is nullified. This supposition may, however, be wrong, for it is hard to conceive what may have been the end of this miracle, and its supernatural quality is clearly not acknowledged by the distinguished persons present. One would find greater satisfaction, perhaps, if the Duke of Argyll had expressed his belief in it, but he contents himself with saying that he had "frequently heard" his brother-in-law relate the circumstance. The identity of this phrase with that said to have been used by another great man on another occasion is very striking, and since the story, though old, is short, perhaps you will let me tell it. I have heard that King George IV., some years before his death, was strongly possessed with the notion that he had been present at the battle of Waterloo. Once as he was recounting his personal adventures on that eventful day, he perceived some uncourtly sign of incredulity shown by one of his audience. Turning to the late Duke of Wellington, who was of the company, he appealed to him—"Isn't that true, Arthur?" The adroit reply was—"I have *frequently heard* your Majesty say so."

May 19 THE REVIEWER OF PALMÉN'S WORK

Barlow's and Laslett's Determination of the Elasticity and Strength of Timber

ON comparing the results of Mr. Barlow's determination of the modulus of elasticity of Teak timber, given in his "Strength of Materials," p. 82, sixth edition, with the results of different Dutch investigators on Djati timber of Java, I was struck by the great difference between those results, those of Mr. Barlow being very much higher than the others, though Teak of British India and Javanese Djati are merely different names for the same species, *Tectona grandis*, L., of the family Verbenaceæ. My own experiments, which will, I hope, be afterwards published in my inaugural dissertation, show also the complete identity between Teak and Djati. This led me to detect a serious error in Mr. Barlow's calculations. He measures the deflection produced by a certain weight of a batten 7 feet by 2 inches square, supported at both ends on two props, the bearing distance being 6 feet, as is stated on p. 67 in the general description of his arrangements. The value of E is found in this case by the formula—

$$E = \frac{l^3 w}{16 a d^3 \delta}$$

in which l is the bearing distance; in the case of Mr. Barlow, equal to 6 feet. Now all Mr. Barlow's E's are calculated by introducing $l = 7$ feet instead of $l = 6$ feet, as should be; the consequence is that all those values are too great. So for Teak timber the value of E is found to be = 603,600 lbs., while the true value is $E = 380{,}023$ lbs. on the square inch. Also in the formula for the strength—

$$S = \frac{l w}{4 a d^2}$$

l is the bearing distance, Mr. Barlow calculates $S = 2{,}462$, taking $l = 7$ feet, the real value being $S = 2{,}110 \cdot 5$ for $l =$ 6 feet.

Mr. Thomas Laslett, in his "Timber and Timber-trees" (London: Macmillan and Co., 1875), following the arrangements of Mr. Barlow, commits the same error. On p. 42 he tells us that in all his experiments pieces were taken $2'' \times 2'' \times 84'' =$ 336 cubic inches, and that each piece was placed upon supports exactly 6 feet apart. But for l is taken, instead of the bearing distance 6 feet, the whole length 7 feet. So all the numbers for the moduli of elasticity of the different woods calculated by Mr. Laslett are too great in the proportion $7^3 : 6^3$, and the numbers for the strength in the proportion 7 : 6.

The reduced values for E from Mr. Laslett, namely, $E =$ 362,870 and $E = 305{,}876$, and that from Mr. Barlow, $E =$ 380,023, agree tolerably well with the mean results of Dutch investigators, $E = 404{,}210$, and much better than does the uncorrected value, $E = 555{,}180$. Other determinations of the E of Teak timber are not known to me.

The results of Mr. Barlow were already published in the year 1817. Since that time several editions of this valuable work have appeared; in the year 1867 the sixth edition, revised by his two sons. General Morin gives in his "Résistance des Matérieux" all the results of Mr. Barlow on timber, reduced to metric weights and measures. Also MM. Chevandier and Wertheim, in their "Mémoire sur les Propriétés Mecaniques du Bois." It is scarcely to be believed that none of these eminent men, nor any one else, have remarked this error in the calculation of Mr. Barlow's often used numbers. S. FIGEE

Haarlem, Holland

Basking Shark

I THINK it but just to Prof. Bocage to ask you to publish the inclosed letter, which only reached me on the first of this month, owing to its having been sent to a wrong address. I regret that I overlooked Signor Capello's memoir on *Selache maxima*, which was so plainly indicated in the *Zoological Record* for 1869. I had, indeed, the "Catalogo dos peixes de Portugal que existem no Museu de Lisboa, por F. de Brito Capello," which was published in No. vi. of the Lisbon *Journal of Science*; but No. vii., which the author (whose kindnesses to me when at Lisbon, in 1868, I cannot forget) sent to me, I never got, and hence one cause of my oversight. E. PERCEVAL WRIGHT

Trinity College, Dublin

Lisbonne, le 14 février, 1877

MONSIEUR ET HONORÉ CONFRÈRE,—Dans l'article que vous avez publié dans la NATURE sur le "Basking Shark," vous avez, comme le Professeur Paul Gervais et d'autres, attribué à M. Steenstrup la découverte des appareils tamisants ou fanons branchiaux du squale pèlerin ou *Squalus maximus*. Si vous vous donnez la peine de consulter No. vii. du *Journal des Sciences Math., Phys. et Nat. de Lisbonne*, vous y trouverez, à p. 236, la description de cet appareil; vous trouverez également ces appareils figurés sur la planche qui accompagne cet article et qui contient aussi la figure du poisson. La description et la figure des appareils branchiaux ou des fanons branchiaux du *Sq. maximus* (et des espèces congénères) ont été donc publiés par M. Capello, aide-naturaliste au Muséum de Lisbonne, en août de 1869; c'est-à-dire 4 ans avant la publication de l'article de M. Steenstrup. Si vous consultez le *Record of Zoological Literature* pour 1869 (vol. vi.), vous y trouverez, p. 139:—

"*Selache maxima.*—A *detailed* description and figure of an example from the coast of Portugal is given by M. Capello under the name of *Cetorhinus blainvillii*, *Jour. Ac. Sc. Lisbon*, No. vii., p. 233."

Je vous envoie par la poste le No. vii. du *Jour. des Sc. de Lisbonne.* J. N. BARBOSA BOCAGE

Gold in Carboniferous Conglomerate

MANY of your readers are aware that the fact of the occurrence of gold in Lower Carboniferous conglomerate as in New South Wales is not at all new. The Gay's River Gold Field of Nova Scotia, where the gold occurs in Lower Carboniferous conglomerate resting on the edges of Cambrian slates having small veins of auriferous quartz, was first pointed out by Prof. Hartt and elaborated by myself in a paper communicated to the Nova Scotian Institute of Natural Science in 1866. In Dawson's "Accadian Geology," of 1868, the same fact is referred to; also in Seluria, Prof. R. Jones received specimens of the conglomerate from me in Paris, 1867, to satisfy Sir R. J. Murchison of the fact. In the collection of ores and concrete minerals sent by H. S. Poole, Esq., Government Inspector of Mines to the Centennial Exhibition, in my charge, was a very instructive specimen of slate with a little of the conglomerate attached, having a beautiful display of gold. This was much admired. The conglomerate of Gay's River is overlaid by limestone with Lower Carboniferous fauna and gypsums. The conglomerate is worked still with good results. D. HONEYMAN

Provincial Museum, Halifax, Nova Scotia

Japanese Mirrors

A SHORT time ago a friend showed me a curious effect, which I had previously heard of, but had never seen. The ladies of Japan use, in making their toilet, a small round mirror about $\frac{1}{12}$ to $\frac{1}{8}$ inch in thickness, made of a kind of speculum metal, brightly polished and coated with mercury. At the back there are usually various devices, Japanese or Chinese written characters, badges, &c., standing in strong relief, and brightly polished like the front surface. Now if the direct rays of the sun are allowed to fall upon the front of the mirror and are then reflected on to a screen, in a great many cases, though not in all, the figures at the back will appear to shine through the substance of the mirror as bright lines upon a moderately bright ground.

I have since tried several mirrors as sold in the shops, and in most cases the appearance described has been observed with more or less distinctness.

I have been unable to find a satisfactory explanation of this fact, but on considering the mode of manufacture I was led to suppose that the pressure to which the mirror was subjected during polishing, and which is greatest on the parts in relief, was concerned in the production of the figures. On putting this to the test by rubbing the back of the mirror with a blunt pointed instrument, and permitting the rays of the sun to be reflected from the front surface, a bright line appeared in the image corresponding to the position of the part rubbed. This experiment is quite easy to repeat, a scratch with a knife or with any other hard body is sufficient. It would seem as if the pressure upon the back during polishing caused some change in the reflecting surface corresponding to the raised parts whereby the amount of light reflected was greater; or supposing that of the light which falls upon the surface, a part is absorbed and the rest reflected, those parts corresponding to the raised portions on the back are altered by the pressure in such a way that less is absorbed, and therefore a bright image appears. This, of course, is not an explanation of the phenomenon, but I put it forward as perhaps indicating the direction in which a true explanation may be looked for.

The following account of the manufacture of the Japanese mirrors is taken from a paper by Dr. Geerts, read before the Asiatic Society of Japan, and appearing in their *Transactions* for 1875-76, p. 39:—

"For preparing the mould, which consists of two halves, put together with their concave surfaces, the workman first powders a kind of rough plastic clay, and mixes this with levigated powder of a blackish "tuff-stone" and a little charcoal powder and water, till the paste is plastic and suitable for being moulded. It is then roughly formed by the aid of a wooden frame into square or round cakes; the surface of the latter is covered with a levigated half-liquid mixture of powdered "*chamotte*" (old crucibles which have served for melting bronze or copper) and water. Thus well prepared, the blackish paste in the frame receives the concave designs by the aid of woodcuts, cut in relief. The two halves of the mould are put together in the frame and dried. Several of these flat moulds are then placed in a melting box made of clay and "chamotte." This box has on the top an opening, into which the liquid bronze is poured, after it has been melted in small fire-proof clay crucibles. The liquid metal naturally fills all openings inside the box, and consequently also the cavities of the moulds. For mirrors of first quality the following metal mixture is used in one of the largest mirror foundries in Kiôto:—

Lead	5 parts.
Tin	15 ,,
Copper	80 ,,
	100

For mirrors of inferior quality is taken—

Lead	10 parts.
Natural sulphide of lead and antimony	10 ,,
Copper	80 ,,
	100

* * * * *

"After being cooled the melting-box and moulds are crushed and the mirrors taken away. These are then cut, scoured, and filed until the mirror is roughly finished. They are then first polished with a polishing powder called *to-no-ki*, which consists of the levigated powder of a soft kind of whetstone (*to-ishi*) found in Yamato and many other places. Secondly, the mirrors are polished with a piece of charcoal and water, the charcoal of the wood, *ho-no-ki* (*Magnolia hypoleuca*) being preferred as the best for this purpose. When the surface of the mirror is well polished it is covered with a layer of mercury amalgam, consisting of quicksilver, tin, and a little lead. The amalgam is rubbed vigorously with a piece of soft leather, which manipulation must be continued for a long time until the excess of mercury is expelled and the mirror has got a fine, bright, reflecting surface." R. W. ATKINSON

University of Tokio, Japan

THE DECENNIAL PERIOD OF MAGNETIC VARIATIONS, AND OF SUN-SPOT FREQUENCY

A CENTURY and half ago Graham discovered that the north end of a magnetic needle moved from morning till afternoon towards the west, returning thereafter to its most easterly position in the morning again. Van Swinden, who, half a century later, studied this phenomenon during several years, occupied himself greatly with the deviations from the diurnal law. One of these, the occurrence of the greatest westerly position before noon or after 4 P.M., he found to happen most frequently in 1776, the number of times increasing from 1772, and diminishing from the year of maximum till 1780. He then asked the question whether there was not a period of *eight years*. Van Swinden's results were greatly affected by imperfections of his instrument, and we can only consider that the excess of irregular days in 1776 was probably chiefly due to real causes.

Though several series of magnetic observations were made during the eighteenth century, and two series early in this (those of Beaufoy and Arago), yet, as far as I can discover, Kaemtz seems (in 1836) to have been the first to remark that the mean value of the diurnal oscillation of the magnetic needle was not constant, but varied from year to year: this conclusion he founded on Cassini's observations, which gave the mean oscillation 9′·71 in 1784, and 15′·10 in 1787. The illustrious Gauss drew more distinct attention to the fact, for, in studying the observations made at Göttingen in the years 1834 to 1837, he pointed out that the mean diurnal oscillation for each month in the second year was greater than that for the corresponding month of the first year; and that a similar increase was to be found in the third year compared with the second. This increase Gauss did not think could go on long, and he predicted that by continuing the observations for several years, an oscillation in the mean value would present itself. It is not a little curious that in discussing the Göttingen observations for the next three years, Dr. Goldschmidt should have failed to remark that the maximum was attained in 1837, and that thereafter the mean diurnal oscillation was diminishing. This was reserved for Dr. Lamont, the distinguished astronomer of Munich, who, in the end of 1845, by adding the mean oscillations obtained from his own observations in 1842-1845, to those already found for the preceding years at Göttingen, was able to state that the minimum was then attained, but that a longer series of observations was required, in order to determine the law of the oscillation.

It was only in the end of 1851, when the maximum oscillation (which occurred in 1848-49) was decidedly past, and the mean oscillation had again begun to diminish in value, that Dr. Lamont published his conclusion that the diurnal oscillation of magnetic declination (as well as of magnetic force) obeyed a law whose mean duration was nearly $10\frac{1}{3}$ years. For the determination of this mean he employed the epoch of maximum oscillations shown by Cassini's observations in 1787 (already noticed by Kaemtz), and he assumed that there were six periods from that date till 1849.

Schwabe had previously, from his persevering observations of the number of spots on the sun's surface, arrived at the conclusion that these obeyed a decennial law, so that the number was a maximum in 1828, 1837, and 1848, while it was a minimum in 1833 and 1843. The agreement of the epochs, 1843 and 1848, with those of minimum and maximum magnetic disturbance deduced by Sir E. Sabine from the observations made in the colonial observatories, was at once remarked by him, as well as that of Lamont's epochs with those of Schwabe.

This coincidence was also immediately afterwards, and quite independently, brought to public notice by Dr. Wolf, of Bern (now of Zurich), and M. Gautier, of Geneva. It is, however, with the important labours of the former of these philosophers that we are most concerned. Dr. Wolf began at once a systematic search for observations of sun-spots, and examined hundreds of volumes printed and in manuscript, dating from the first discovery of the existence of spots on the sun's surface. All the observations thus accumulated he has endeavoured to connect and to reduce to a common unit; and from the numbers thus obtained he has concluded that the sun-spot period, as well as that of the magnetic variations, occupies on the average $11\frac{1}{9}$ years.

One great cause of the difference between the results of the Munich and Zurich astronomers is to be found in the interval 1787 to 1818. According to the former, three periods *ought* to have occurred in this interval; according to the latter, only one maximum happened, *in fact*, between the two of 1787 and 1818. Dr. Wolf has concluded, from the magnetic observations of Gilpin (1786–1806), that a minimum of the diurnal oscillation of the magnetic needle occurred in 1796, and a maximum in 1803, and these epochs he has supported by the observations of the numbers of sun-spots, as well as of those of the aurora borealis, a phenomenon known to be associated with magnetic disturbance, and to have the same epochs of frequency. On the other hand, Dr. Lamont has maintained that Gilpin's observations are without value, as his needle was supported on a steel pivot, and sometimes did not move freely; he has also objected to the observations of sun-spot frequency made during the time in question, that they were made rarely, without any common system, and by few observers, some having at times seen no spots when others saw many.

If we could assume with the astronomer of Munich that Gilpin's observations and those of sun-spot and auroral frequency made at the same time are worthless, all our knowledge of the epochs of magnetic oscillations since 1818, and of sun-spot frequency since 1826, would induce us to conclude that there were really three periods during the thirty-one years 1787–1818. If, however, any value can be given to the observations during that interval, it is not allowable to assume that the durations of the periods have always been the same, the more especially that we know the period has varied in length from eight to twelve years within the last half century. That some value is due to observations of three different phenomena has been allowed by most writers, and Dr. Wolf's period of $11\frac{1}{9}$ years has, in consequence, been accepted by many of the most eminent men of science who have had occasion to allude to the subject.

Having had to study this question in connection with the results of observations made during twenty-three years at Trevandrum, I have examined with care the magnetic observations of the last and the present century, determined the exact times for which the yearly mean diurnal oscillation of the magnetic needle was a maximum or minimum, and have arrived at the following conclusions:—[1]

1st. That there are not sufficient grounds for rejecting the observations of Gilpin, which appear to be in general trustworthy as regards the change of mean position of the needle from year to year, and of the diurnal range from winter to summer.

2nd. That these observations should, according to the mean law, show a maximum near 1797, and another should have occurred near 1807. I have found that they do indicate a maximum in the former year; and though another maximum appears in 1803, that there are grounds for believing the maximum may really have occurred after 1806, when Gilpin's series terminated.

It has to be stated, however, that the maximum shown by Gilpin's observations in 1797 is very small; that the whole interval between the preceding and following minimum is not six years; and that no such short period and small maximum have been observed during the last half century. Since, however, the shortness of the period and the smallness of the maximum are both confirmed by the observations known to us of the frequency of sun-spots and of the aurora borealis, I can only conclude, in conformity with the facts, that both these were real phenomena, which may yet be repeated and aid in the determination of the cause of the decennial period. The mean duration of the period at which I arrive is therefore almost exactly that which Dr. Lamont had previously obtained, or 10·45 years.

For this result the facts have been taken as they present themselves; since it would be difficult to conclude that the observers of all the three phenomena could have erred in the same way during nearly twenty years. In addition to this, after a careful study of Dr. Wolf's sun-spot numbers, I find it impossible to accept his period of $11\frac{1}{9}$ years. How ill the facts satisfy this result may be shown by two comparisons in which the epochs accepted by the Zurich astronomer are employed.

Thus a maximum of the magnetic oscillation occurred in 1787 by the observations of Cassini and Gilpin; this epoch has been confirmed nearly by Dr. Wolf's sun-spot numbers, and by Prof. Loomis for the auroral frequency. We have then the last observed maximum 1870·9, about which there can be no doubt. In the interval between these two maxima there were, according to Dr. Wolf, only seven periods, consequently we have—

$$\frac{1870{\cdot}9-1787{\cdot}3}{7} = \frac{83{\cdot}6}{7} = 11{\cdot}94 \text{ years},$$

a period which differs as much from his mean period as that does from Dr. Lamont's. If on the other hand we take one of Dr. Wolf's sun-spot epochs about eighty years

[1] See "On the Decennial Period," &c., *Trans. Roy. Soc. Edin.*, xxvii., pp. 563-594.

before 1787, and employ the number of periods he has himself given for the interval, we find—

$$\frac{1787\cdot3-1705\cdot5}{8} = \frac{81\cdot8}{8} = 10\cdot23 \text{ years.}$$

If, then, we commence with the epoch of 1787 and compare it with any epoch of maximum since, we shall always find for the mean duration at the least 11·9 years according to Dr. Wolf; and if we compare it with any of the epochs given by him upwards of eighty years before, we shall never find a greater mean than 10·75 years, and this result includes an interval of 172 years before 1787, with all the uncertainty of the earlier epochs. This great difference of more than one year in the mean duration, as derived from eighty-four years after 1787, and eighty-two to 172 years before, disappears to a great extent if we admit three periods between 1787 and 1818.

It has been already remarked that the duration of a period is not constant, but varies within certain limits. The question naturally presents itself—Does this variation follow any law, or is it accidental, increasing one year and diminishing the next? The number of periods for which we have the epochs of maxima and minima of the diurnal oscillation of the magnetic needle accurately determined, is not sufficient for any very sure reply. At the same time the results I have obtained indicate a period of nearly forty-two years for the repetition of the variations in question; and if this conclusion is confirmed by next maximum, that should occur in the year 1879. It may also be pointed out that according to the law of forty-two years a maximum should have occurred in 1818 − 42 = 1776. Now this year, according to Dr. Wolf, was a year of minimum. The variation of his sun-spot numbers for that period, it appears to me, is not sufficient to give his conclusion much weight; while, on the other hand, Van Swinden's result, which it is extremely probable was a consequence of the decennial law, gives 1776 for the year of maximum; and that it was so is further supported by the magnetic observations of Cotte, at Montmorency. The exceptional period about 1797 shows, however, that any definite conclusion from observations during the last sixty years may be impossible, since causes of variation exist with which we are insufficiently acquainted as yet.

When we compare the mean range of the diurnal oscillation of the needle for the year in which it is a maximum with that for the year of minimum at any station, we find that the ratio of the two is very nearly constant for places so widely separated as Toronto, Dublin, Trevandrum, and Hobarton. I have also found that the *law* of the diurnal movement is the same in the year for which the range is least, and in that for which it is greatest. This shows that it is the same cause which is acting, the variation being one of intensity only. Since few or no sun-spots are visible in the years of minimum range, we perceive that the sun-spots happen only when the intensity of the force producing the magnetic variations exceeds a given value. It also appears that considerable variations in the amount of magnetic disturbance may exist near the equator when there are few or no sun-spots; and, on the other hand, that the spotted surface of the sun may be a maximum, and no corresponding increase of the magnetic oscillations be visible. The latter are, however, exceptional cases, since increases of sun-spots and of magnetic movements occur frequently near the same time; the increase of the one, however, bears no constant proportion to that of the other.

It has been already stated that the ratio of the diurnal oscillation of the needle in the year of maximum to that in the year of minimum is very nearly constant for places very widely separated from each other; there are, however, slight variations in the ratio shown at some places; thus, although it is nearly the same at Toronto, Dublin, Trevandrum, and Hobarton (1·55), it is slightly greater for Munich and Lisbon (1·71). This is probably due to the action of disturbances which are known to obey local laws. I have also found for Trevandrum, nearly on the magnetic equator, that the disturbances, or the deviations of the magnetic needle from the mean position, do not show exactly the same epochs of maximum and minimum in the decennial period when different hours are considered. Thus, though the cause is cosmic, the actions appear to be influenced, though but slightly, by circumstances of locality.

When we seek for the cause of the decennial period, we are met at first by the three phenomena which obey this law: the magnetic variations, the sun-spots, and the aurora borealis. The connection between the first and third is so marked, that if a magnetic disturbance commences during the day in a high latitude, it is quite certain that the aurora will be seen as soon as the disappearance of sunlight permits. This is a fact I have verified during several years' observations in the south of Scotland. Both these phenomena are results of electrical motions. It did not seem improbable then that the solar spots might be connected with disturbances of electrical equilibrium, and that these might be due to the different electrical states of the sun and of the planets.

We do not know, however, of any planet with a period of ten and a half years, nor of any combination of planetary positions which would produce such a period. My own researches have failed in connecting the variations of the sun's spotted surface with the time of revolution of any planet by a law which holds for different decennial periods. This fact, however, does not disprove a planetary action. We are unacquainted with the nature of the medium through which the electrical actions producing the magnetic variations are conveyed. Physicists seek to reduce the phenomena of nature to the fewest possible factors: many then have been induced to believe that electrical and magnetical actions are conveyed by the same ethereal medium which we believe transmits heat and light. The facts do not appear to be easily explained by such a hypothesis; thus I have found that certain electrical actions of the sun producing marked diminutions of the earth's magnetic force happen exactly at successive intervals of twenty-six days; when one point or meridian of the sun returns to the same position relatively to the earth; this action, similar to that of a beam of light reflected from a revolving mirror, which illuminates a particular point only at the same part of its revolution, has no resemblance to that of light and heat, which are propagated equally in all directions.

If, then, we can suppose that the electrical medium is disposed unsymmetrically around the sun, that the disposition and extension varies, it is obvious that the supposed planetary actions would also vary, and might be quite different for different parts of their orbits, in different decennial periods. This suggestion may explain why I have not been able to find a law remaining the same in the different periods; and it is not opposed to the conclusions of Messrs. De la Rue, Stewart, and Loewy, who have found very remarkable relations between certain positions of the planets and the amount of the sun's spotted surface during a single decennial period

Any hypothesis which seeks to explain the mode of production of the sun-spots (by cyclones or otherwise) must also explain why the causes become insufficient for their production every ten and a half years. M. Faye, the distinguished French astronomer, considers that the prime cause of sun-spots is to be found in the excess of heat radiated; so that the spots are the symptoms of a dying sun; that we have in fact here a phenomenon like the flickering of an expiring lamp which may have a periodical character. Such a hypothesis will scarcely satisfy the demands of science, but we must evidently wait for more facts before any satisfactory theory can be proposed.

JOHN ALLAN BROUN

HOW TO DRAW A STRAIGHT LINE[1]

THE great geometrician Euclid, before demonstrating to us the various propositions contained in his "Elements of Geometry," requires that we should be able to effect certain processes. These *Postulates*, as the requirements are termed, may roughly be said to demand that we should be able to describe straight lines and circles. And so great is the veneration that is paid to this master-geometrician, that there are many who would refuse the designation of "geometrical" to a demonstration which requires any other construction than can be effected by straight lines and circles. Hence many problems—such as, for example, the trisection of an angle—which can readily be effected by employing other simple means, are said to have no geometrical solution, since they cannot be solved by straight lines and circles only.

It becomes then interesting to inquire how we can effect these preliminary requirements, how we can describe these circles and these straight lines, with as much accuracy as the physical circumstances of the problems will admit of.

As regards the circle we encounter no difficulty. Taking Euclid's definition, and assuming, as of course we must, that our surface on which we wish to describe the circle is a plane, we see that we have only to make our tracing-point preserve a distance from the given centre of the circle constant and equal to the required radius. This can readily be effected by taking a flat piece of any form, such as the piece of carboard I have here, and passing a pivot which is fixed to the given surface at the given centre through a hole in the piece, and a tracer or pencil through another hole in it whose distance from the first is equal to the given radius; we shall then, by moving the pencil, be able, even with this rude apparatus, to describe a circle with considerable accuracy and ease; and when we come to employ very small holes and pivots, or even larger ones turned with all that marvellous truth which the lathe affords, we shall get a result unequalled perhaps among mechanical apparatus for the smoothness and accuracy of its movement. The apparatus I have just described is of course nothing but a simple form of a pair of compasses, and it is usual to say that the third Postulate postulates the compasses.

But the straight line, how are we going to describe that? Euclid defines it as "lying evenly between its extreme points." This does not help us much. Our text-books say that the first and second Postulates postulate a ruler. But surely that is begging the question. If we are to draw a straight line with a ruler, the ruler must itself have a straight edge; and how are we going to make the edge straight? We come back to our starting-point.

Now I wish you clearly to understand the difference between the method I just now employed for describing a circle, and the ruler method of describing a straight line. If I applied the ruler method to the description of a circle I should take a circular lamina, such as a penny, and trace my circle by passing the pencil round the edge, and I should have the same difficulty that I had with the straight-edge, for I should first have to make the lamina itself circular. But the other method I employed involves no begging the question. I do not first assume that I have a circle and then use it to trace one, but simply require that the distance between two points shall be invariable. I am of course aware that we do employ circles in our simple compass, the pivot and the hole in the moving piece which it fits are such; but they are used not because they are the curves we want to describe (they are not so, but are of a different size), as is the case with the straight-edge, but because, through the impossibility of constructing pivots or holes of no finite dimensions, we are forced to adopt the best substitute we can for making one point in the moving piece remain at the same spot. If we employ a very small pivot and hole, though they be not truly circular, the error in the description of a circle of moderate dimensions will be practically infinitesimal, not perhaps varying beyond the width of the thinnest line which the tracer can be made to describe; and even when we employ large pivots and holes we shall get results as accurate, because those pivots and holes may be made by the employment of very small ones in the machine which makes them.

It appears, then, that although we have an easy and accurate method of describing a circle, we have at first sight no corresponding means of describing a straight line; and there would seem to be a substantial difficulty in producing what mathematicians call the simplest curve, so that the question how to get over that difficulty becomes one of a decided theoretical interest.

Nor is the interest theoretical only, for the question is one of direct importance to the practical mechanician. In a large number of machines and scientific apparatus it is requisite that some point or points should move accurately in a straight line with as little friction as possible. If the ruler principle is adopted, and the point is kept in its path by guides, we have, besides the initial difficulty of making the guides truly straight, the wear and tear produced by the friction of the sliding surfaces, and the deformation produced by changes of temperature and varying strains. It becomes therefore of real consequence to obtain, if possible, some method which shall not involve these objectionable features, but possess the accuracy and ease of movement which characterises our circle-producing apparatus.

Turning to that apparatus we notice that all that is requisite to draw with accuracy a circle of any given radius is to have the distance between the pivot and the tracer properly determined, and if I pivot a second "piece" to the fixed surface at a second point having a tracer as the first piece has, by properly determining the distance between the second tracer and pivot I can describe a second circle whose radius bears any proportion I please to that of the first circle. Now, removing the tracers, let me pivot a third piece to these two *radial* pieces, as I may call them, at the points where the tracers were, and let me fix a tracer at any point on this third or *traversing* piece. You will at once see that if the radial pieces were big enough the tracer would describe circles or portions of circles on *them*, though they are in motion, with the same ease and accuracy as in the case of the simple circle drawing apparatus; the tracer will not however describe a circle on the *fixed* surface but a complicated curve.

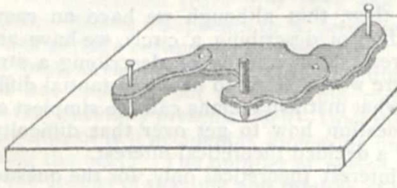
This curve will, however, be described with all the ease and accuracy of movement with which the circles were described, and if I wish to reproduce in a second apparatus the curves which I produce with this, I have only to get the distances between the pivots and tracers accurately the same in both cases, and the curves will also be accurately the same. I could of course go on adding fresh pieces *ad libitum*, and I should get points on the structure produced, describing in general very complicated curves, but with the same results as to accuracy and smoothness, *the reproduction of any particular curve depending solely on the correct determination of a certain definite number of distances.*

These systems, built up of pieces pointed or pivoted together, and turning about pivots attached to a fixed base, so that the various points on the pieces all describe definite curves, I shall term "link-motions," the pieces being termed "links." As, however, it sometimes facilitates the consideration of the properties of these structures to regard them apart from the base to which they

[1] Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus, by A. B. Kempe, B.A.

are pivoted, the word "linkage" is employed to denote any combination of pieces pivoted together. When such a combination is pivoted in any way to a fixed base, the motion of points on it not being necessarily confined to fixed paths, the link structure is called a "linkwork:" a "linkwork" in which the motion of every point is in some definite path being, as before stated, termed a "link-motion." I shall only add to these expressions two more: the point of a link-motion which describes any curve is called a "graph," the curve being called a "gram."

Fig. 1.



The consideration of the various properties of these "linkages" has occupied much attention of late years among mathematicians, and is a subject of much complexity and difficulty. With the purely mathematical side of the question I do not, however, propose to deal to-day, as we shall have quite enough to do if we confine our attention to the practical results which mathematicians have obtained, and which I believe only mathematicians could have obtained. That these results are valuable cannot, I think, be doubted, though it may well be that their great beauty has led some to attribute to them an importance which they do not really possess; and it may be that fifty years ago they would have had a value which, through the great improvements that modern mechanicians have effected in the production of true planes, rulers and other exact mechanical structures, cannot now be ascribed to them. But linkages have not at present, I think, been sufficiently put before the mechanician to enable us to say what value should really be set upon them.

The practical results obtained by the use of linkages are but few in number, and are closely connected with the problem of "straight-line motion," having in fact been discovered during the investigation of that problem, and I shall be naturally led to consider them if I make "straight-line motion" the backbone of my lecture. Before, however, plunging into the midst of these linkages it will be useful to know how we can practically construct such models as we require; and here is one of the great advantages of our subject—we can get our

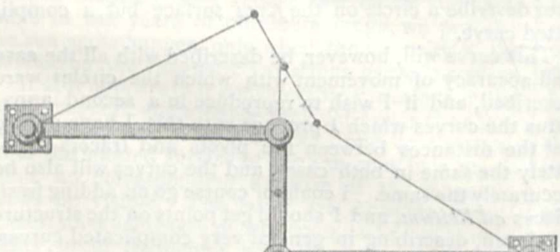


Fig. 2.

results visibly before us so very easily. Pins for fixed pivots, cards for links, string or cotton for the other pivots, and a dining-room table, or a drawing board if the former be thought objectionable, for a fixed base, are all we require. If something more artistic be preferred, the plan adopted in the models exhibited by me in the Loan Collection can be employed. The models were constructed by my brother, Mr. H. R. Kempe, in the following way. The bases are thin deal boards painted black; the links are neatly shaped out of thick cardboard (it is hard work making them, you have to sharpen your knife about every ten minutes, as the cardboard turns the edge very rapidly); the pivots are little rivets made of catgut, the

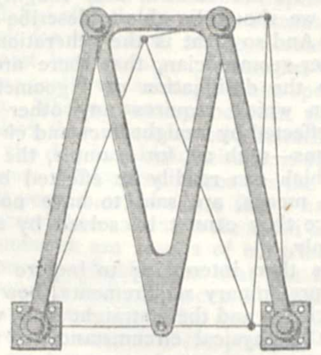


Fig. 3.

heads being formed by pressing the face of a heated steel chisel on the ends of the gut after it is passed through the holes in the links; this gives a very firm and smoothly working joint. More durable links may be made of tin-plate; the pivot-holes must in this case be punched, and the eyelets used by bootmakers for laced boots employed as pivots; you can get the proper tools at a trifling expense at any large tool shop.

Now, as I have said, the curves described by the various points on these link-motions are in general very complex. But they are not necessarily so. By properly choosing the distances at our disposal we can make them very simple. But can we go to the fullest extent of simplicity and get a point on one of them moving accurately in a straight line? That is what we are going to investigate.

To solve the problem with our single link is clearly impossible: all the points on it describe circles. We must therefore go to the next simple case—our three-link motion. In this case you will see that we have at our disposal the distance between the fixed pivots, the distances between the pivots on the radial links, the distance between the pivots on the traversing link, and the distances of the tracer from those pivots; in all six different

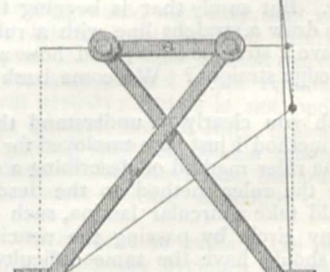


Fig. 4.

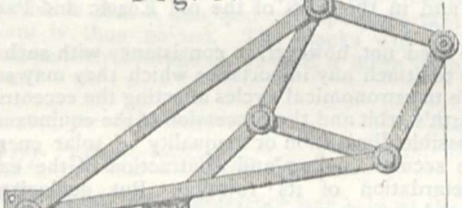
distances. Can we choose those distances so that our tracing-point shall move in a straight line?

The first person who investigated this was that great man James Watt. "Watt's Parallel Motion," invented in 1784, is well known to every engineer, and is employed in nearly every beam-engine. The apparatus reduced to its simplest form is shown in Fig. 2.

The radial bars are of equal length—I employ the word "length" for brevity, to denote the distance between the pivots, the links, of course, may be of any length or shape,—and the distance between the pivots or the traversing link is such that when the radial bars are parallel the line joining those pivots is perpendicular to the radial bars. The tracing-point is situate half-way between the pivots on the traversing piece. The curve described by the tracer is, if the apparatus does not deviate much from its mean position, approximately a straight line. The reason of this is that the circles described by the extremities of the radial bars have their concavities turned in opposite directions, and the tracer being half-way between, describes a curve which is concave neither one way nor the other, and is therefore a straight line. The curve is not, however, accurately straight, for if I allow the tracer to describe the whole path it is capable of describing, it will, when it gets some distance from its mean position, deviate considerably from the straight line, and will be found to describe a figure 8, the portions at the crossing being nearly straight. We know that they are not quite straight, because it is impossible to have such a curve partly straight and partly curved.

For many purposes the straight line described by Watt's apparatus is sufficiently accurate, but if we require an exact one it will, of course, not do, and we must try again. Now it is capable of proof that it is impossible to solve the problem with three moving links; closer approximations to the truth than that given by Watt can be obtained, but still not actual truth.

Fig. 5.



I have here some examples of these closer approximations. The first of these, shown in Fig. 3, is due to Richard Roberts of Manchester.

The radial bars are of equal length, the distance between the fixed pivots is twice that of the pivots on the traversing piece, and the tracer is situate on the traversing piece, at a distance from the pivots on it equal to the lengths of the radial bars. The tracer in consequence coincides with the straight line joining the fixed pivots at those pivots and half-way between them. It does not, however, coincide at any other point, but deviates very slightly between the fixed pivots. The path described by the tracer when it passes the pivots, altogether deviates from the straight line.

The other apparatus was invented by Prof. Tchebicheff of St. Petersburg. It is shown in Fig. 4. The radial bars are equal in length, being each in my little model five inches long. The distance between the fixed pivots must then be four inches, and the distance between the pivots or the traversing bar two inches. The tracer is taken half-way between these last. If now we draw a straight line—I had forgotten that we cannot do that yet, well, if we draw a straight line, popularly so called—through the tracer in its mean position as shown in the figure, parallel to that forming the fixed pivots, it will be found that the tracer will coincide with that line at the points where verticals through the fixed pivots cut it as well as at the mean position, but, as in the case of Roberts's parallel motion, it coincides nowhere else, though its deviation is very small as long as it remains between the verticals.

We have failed then with three links, and we must go on to the next case, a five-link motion—for you will observe that we must have an odd number of links if we want an apparatus describing definite curves. Can we solve the problem with five? Well, we can, but this was not the first accurate parallel motion discovered, and we must give the first inventor his due (although he did not find the simplest way), and proceed in strict chronological order.

In 1864, eighty years after Watt's discovery, the problem was first solved by M. Peaucellier, an officer of Engineers in the French army. His discovery was not at first estimated at its true value, fell almost into oblivion, and was rediscovered by a Russian student named Lipkin, who got a substantial reward from the Russian Government for his supposed originality. However, M. Peaucellier's merit has at last been recognised, and he has been awarded the great mechanical prize of the Institute of France, the "Prix Montyon."

M. Peaucellier's apparatus is shown in Fig. 5. It has, as you see, seven pieces or links. There are first of all two long links of equal length. These are both pivoted at the same fixed point; their other extremities are pivoted to opposite angles of a rhombus composed of four equal shorter links. The portion of the apparatus I have thus far described, considered apart from the fixed base, is a linkage termed a "Peaucellier cell." We then take an *extra* link, and pivot it to a fixed point whose distance from the first fixed point, that to which the cell is pivoted, is the same as the length of the extra link; the other end of the extra link is then pivoted to one of the free angles of the rhombus; the other free angle of the rhombus has a pencil at its pivot. That pencil will accurately describe a straight line.

I must now indulge in a little simple geometry. It is absolutely necessary that I should do so in order that you may understand the principle of our apparatus.

(*To be continued.*)

FOSSIL FLORAS AND GLACIAL PERIODS

A RECENT notice in NATURE (vol. xiv. p. 336) of certain inferences of Prof. Heer in connection with the Arctic fossil plants obtained by the Swedish Expeditions of 1870 and 1872, suggests some thoughts on the relations of fossil plants to climate, which, though I have discussed them elsewhere, deserve to have attention again directed to them. In my Bakerian Lecture before the Royal Society in 1870, and in my "Report on the Pre-carboniferous Flora of Canada," published by the Canadian Survey in 1871, I deduced from the generalisations of Prof. James Hall as to the growth of the American Continent from the north-east, in connection with the distribution of the fossil plants of the Upper Silurian, Erian, and Carboniferous systems, the conclusion that these assemblages of plants entered North America from the north-east, and propagated themselves southward and westward. Prof. Asa Gray had, as early as 1867, stated similar conclusions with reference to the modern floras of America and Eastern Asia, and has more recently extended them to the Tertiary floras on the evidence of Heer and Lesquereux.[1]

The further conclusion that all the old floras appeared suddenly and abruptly in the temperate regions, and with a great number of species, I have illustrated in the Report above referred to, as far as regards the Palæozoic plants, and have referred to the evidence of it in the case of the Cretaceous and Tertiary floras in my address to the American Association in 1875.

With regard to the succession of these floras, it is true that it has been the fashion with certain European palæontologists to regard our rich Devonian or Erian flora

[1] Address to the American Association, 1872.

of America as of Carboniferous age, simply because it contains forms not found as yet in rocks so old in Europe. But this notion is at variance with stratigraphy and animal fossils, and quite as wide of the mark as the often-repeated dictum of some of the same authorities that the Cretaceous flora of Vancouver Island and the Eocene flora of the North-western plains are equivalents of the European Miocene. In point of fact, we have in America distinct floras of Erian and Carboniferous age with an intermediate sub-flora of Lower Carboniferous date,[1] and succeeding them the Triassic and Jurassic flora, that of the Cretaceous, that of the Eocene, and that of the Miocene; and there is good reason to believe that all of these invaded the Continent from the northward and lingered longest in the south. There may no doubt have been counter migrations from the south, but these seem to have left less trace in the geological record. The special Lower Carboniferous or "Culm" flora, and that of the Eocene in Europe, may be of this character.

If we compare these facts with those known from other sources as to the alternation of cold and warm climates in the northern hemisphere, it would seem that they harmonise most closely on the hypothesis advocated by Sir Charles Lyell, that these changes of climate have depended mainly on the distribution of land and water and of the ocean currents.

Assuming a condition in which much tropical land existed, along with islands in the Arctic and sub-Arctic regions, surrounded with deep water over which warm currents were distributed, a rich flora might extend as far northward as the supply of light would permit. Further, if such condition of equatorial protuberance were coincident with a less obliquity of the ecliptic, there might be less difficulty with regard to a continuous supply of light than under present circumstances. Succeeding elevation of northern and temperate land and depression of that nearer the equator, would destroy the more southern flora and cause that of the north to advance over the newly-elevated continental plateaus. This would more especially be the case if, as we may infer from the possible connection of equatorial subsidence with the retardation of the earth's rotation, the depression of the northern land was very slow and gradual, and that of the equatorial land more sudden and paroxysmal.

Invasions of plants from the north would thus result from continental elevation in the temperate regions, and these invasions would go on contemporaneously with the introduction of less equable and cooler climatal conditions. These might not, however, advance so far as to cause extreme glacial phenomena, except in those, perhaps rare, circumstances, when Arctic land was elevated while the greater part of the tropical and temperate areas remained under shallow seas with little heating and radiating surface and invaded by much northern ice. Further, when we take into consideration the growth of the continents in later geological times, it is evident that such periods of exceptional cold would be more likely to occur in these later times, and that they might be less intense in earlier geological periods, and might attain their maximum in the last glacial period. They would also be irregular as to the intervals between them, and might through long periods be absent altogether. We have proof of the efficacy of such causes in the contrast between the climates of Labrador and England at the present day, and also in that sameness of the climate of those regions in the Post-pliocene period, of which I long ago pointed out the evidence in my "Acadian Geology." Such moderate views as to glacial periods may also serve to render more explicable the facts as to the absence of evidence of glacial action in Arctic Tertiary formations as observed by Nordenskjöld.

It will of course be understood that my conception of glacial periods is not that of continental ice-caps; but rather such conditions as would cover great breadths of shallow sea in the northern hemisphere with a permanent and continuous ice-pack, accompanied of course with "bordage" and "anchor ice" and with glaciers descending to the sea from high lands; the whole resembling that now occupying large areas of the Arctic seas, and occurring in winter in the Gulf and River St. Lawrence. To such agencies I have been accustomed for the last twenty years to refer our Canadian boulder clay and glaciated rocks. Further, to this extent we have evidence, locally at least, of ice-action in temperate latitudes (in non-fossiliferous conglomerates with boulders) as far back as the Huronian age, while the evidence of alternate submergence and emergence of the northern land extends down to that of the Post-pliocene, whose greatness geologists are only beginning to realise.

It is a corollary from these views that there can have been no change within geological time in the position of the earth's axis of rotation. The distribution of sediment by the polar currents, and the lines of plication and upheaval of the crust, as well as the distribution of successive floras, prove that the poles have remained since the Laurentian period where they now are. I need here merely refer to the fact, well known to all American geologists, that the earthy matter of the thick Appalachian sediments lies parallel to the line of the modern Arctic currents, which seem in all geological time to have been potent agents in carrying the *débris* of the disintegrated Arctic rocks to the south, and filling up the voids caused by equatorial subsidence. Further, the great organic limestones, which represent the contemporaneous food-bearing warm currents from the equator, lie on the plateaus and in the bays of the old Eozoic and Palæozoic land.

We need not, however, in consistency with such views, refuse to attach any importance which they may seem to require to astronomical cycles affecting the eccentricity of the earth's orbit and the precession of the equinoxes, or to the possible diminution or inequality of solar energy, or to the secular cooling and contraction of the earth or the retardation of its rotation. But geologists and palæontologists, in speculating on past conditions of the earth, should endeavour in the first instance to gauge the value of the causes indicated by their own sciences; and where climate is in question no evidence can be more important than that of continental elevation and depression, in connection with the appearance and diffusion of those assemblages of land plants which furnish so sure testimony as to climatal influences.

I should perhaps apologise for throwing out these suggestions with so little of illustration or proof. For much of this I may refer to my published memoirs;[1] and I have now before me a mass of additional evidence, collected in all the great regions from Newfoundland to British Columbia by several recent observers, which I have not at the moment time or opportunity to throw into a connected form. My present object is to invite the attention of the many young and active geologists now working at these subjects to lines of investigation from which they may be deterred by some of the theoretical views now current.

J. W. DAWSON

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[1] Tweedian of North of England, Culm of Germany.

[1] Especially the Report above referred to, and "Notes on the Post-pliocene Geology of Canada," *Canadian Naturalist*, New Series, vol. vi.

A NEW STIMULANT—PITURY

BARON VON MUELLER writes to the *Australian Medical Journal* on the origin of the Pitury, a stimulant said to be of marvellous power, and known to be in use by the Aborigines of Central Australia. After years of efforts to get a specimen of the plant, he had obtained leaves, but neither flowers nor fruits. He can almost with certainty, after due microscopic examination, pronounce those of the Pitury as derived from his *Duboisia Hopwoodii*, described in 1861 (*Fragm. Phytogr. Austr.* II., 138). This bush extends from the Darling River and Barcoo to West Australia, through desert scrubs, but is of exceedingly sparse occurrence anywhere. In fixing the origin of the Pitury, a wide field for further inquiry is opened up, inasmuch as a second species of *Duboisia* (*D. myoporoides, R. Br.*) extends in forest land from near Sydney to near Cape York, and is traced also to New Caledonia, and lately by him also to New Guinea. In all probability this *D. myoporoides* shares the properties of *D. Hopwoodii*, as he finds that both have the same burning acrid taste. Baron Mueller adds: "Though the first known species is so near to us, we never suspected any such extraordinary properties in it as are now established for the later discovered species. Moreover, the numerous species of the allied genus *Anthocercis*, extending over the greater part of the Australian continent and to Tasmania, should now also be tasted, and further the many likewise cognate *Schwenkeas* of South America, should be drawn into the same cyclus of research, nothing whatever of the properties of any of these plants being known. The natives of Central Australia chew the leaves of *Duboisia Hopwoodii*, just as the Peruvians and Chilians masticate the leaves of the Coca (*Erythroxylon Coca*), to invigorate themselves during their long foot journeys through the deserts. I am not certain whether the Aborigines of all districts in which the Pitury grows are really aware of its stimulating power. Those living near the Barcoo travel many days' journeys to obtain this, to them, precious foliage, which is carried always about by them broken into small fragments and tied up in little bags. It is not improbable that a new and perhaps important medicinal plant is thus gained. The blacks use the *Duboisia* to excite their courage in warfare, a large dose infuriates them."

THE ANTIQUITY OF MAN

ON Tuesday evening last a conference was held at the rooms of the Anthropological Institute on the Present State of the Question of the Antiquity of Man. The chair was taken by the president, Mr. John Evans, F.R.S. There were also present Lord Talbot de Malahide, Prof. Huxley, Prof. Prestwich, Prof. Rolleston, Prof. Busk, Prof. Boyd Dawkins, Prof. McK. Hughes, Rev. Prof. Sayce, Mr. J. Heywood, Col. Lane Fox, Mr. A. W. Marks, Capt. Douglas Galton, Rev. E. W. Edgell, and many other gentlemen.

The President in opening the conference alluded to the altered position of the question since it was first brought before the British public in 1859, and pointed out the extreme caution which was necessary in dealing with the subject as it lay within the domain of the archæologist, the anthropologist, and the geologist, neither of whom alone was sufficient by himself to offer a very strong opinion on the subject. Great care was also necessary with regard to the facts of the discoveries themselves, as the objects discovered were liable to get mixed with other objects below them, and this was important in the case of cave deposits in which there might be interments of a later date than the human skeletons deposited in the caves. The question was now very much within the province of the geologist, whose business it was to determine the antiquity of the deposits in which the discoveries may have been made. After alluding to several recent discoveries in France, Spain, and Switzerland, the President remarked that each successive discovery or presumed discovery must be received in a cautious but candid spirit, and looking to the many sources of doubt and error which attached to isolated discoveries, their watchword must for the present be "caution, caution, caution."

The debate was opened by Prof. Boyd Dawkins by an inquiry into the value of the evidence offered by the bone-caves of Great Britain. The antiquity of man is not to be measured by the system of chronology used by the historian, but by the physical and biological changes familiar to the geologist. Beyond historical record time past cannot be estimated in terms of years, because of our ignorance of the length of the intervals, and of the time necessary to produce the changes which mark the hour on the geological dial. The caves of Cresswell Crags, recently brought before the Geological Society, were taken as types, showing the strange association of human implements and remains of animals. Bones and teeth of species now found only in the south, such as the spotted hyæna and lion, were lying side by side with those of northern habit, such as the reindeer, while some are extinct, such as the mammoth and woolly rhinoceros, and others, such as the stag, horse, and bison, still live in the temperate regions. This mixed fauna is universal in British bone-caves, and in those of France and Germany, and it cannot be accounted for by the supposition of Messrs. James Geikie and Croll that the southern animals inhabited Britain in a warm period inter-glacial, while the northern were here at another time after, with an interval between them of from 5,000 to 12,000 years; not only because they are closely associated together in the same strata, but because we have full proof that northern and southern species coexisted at the same time on the same place, in the fact that the reindeer formed an important portion of the prey of the hyæna. It may, however, be accounted for by the overlapping of faunas according to the ever-varying summer heat and winter cold over what was then a vast continent, extending from Northern Africa as far as the 100-fathom line off the coast of Scotland and Scandinavia. The palæolithic man of the caves belongs to the northern group of the pleistocene animals, and his remains are therefore of late pleistocene age. This northern group invaded Europe as the glacial cold came on, was pushed down as far south as the Mediterranean, the Alps, and Pyrenees, as the ice-sheet advanced southwards, and on its retreat passed again northwards. It therefore follows that they are both pre- and post-glacial in Britain. Some caves have been inhabited by man in post-glacial times, as, for example, that of Pont Newydd, near St. Asaph, but it does not follow that all palæolithic caves are post-glacial. The Victoria Cave offers no evidence as to the antiquity of man, because fibula found in association with the pleistocene mammalia, and supposed to be human, is most probably ursine. Further the relation of the deposit in which it was found to the glacial strata of the district is a matter of dispute.

The facts brought forward by Mr. James Geikie, that all palæolithic remains are of earlier date than post-glacial times may be interpreted otherwise. The "something like perpetual summer" which he considers necessary for the presence of the southern animals in the mixed fauna of the caves and of which there is no trace in post-glacial times, is inconsistent with the abundance of reindeer invariably associated with the palæolithic remains of the caves. The barren areas in Great Britain, in which no pleistocene species are found, may be reasonably accounted for by the fact that they were covered with ice, *while* the species were living in more glaciated regions in the south, than by the view that they were equally distributed over the whole area, and afterwards removed by ice for the glaciated regions. The glacial phenomena are no guide to age in non-glaciated districts. In fine, the evidence of the caves is decisive that these palæolithic inhabitants are of late pleistocene age, post-glacial, and possibly pre-glacial, and glacial.

Prof. Hughes, after a few remarks on some foreign cases in which man had been referred to periods more remote than was generally included under the term glacial, commenced by explaining that in using the word glacial he meant the period in which conditions prevailed in the area in question such as must have caused glacier ice, or in adjoining areas which, by supplying berg or coast ice or influencing the climate, must have affected the area in question. He then proceeded to criticise the cases adduced from the neighbourhood of Brandon and Thetford. By an appeal to sections he showed that the beds in which the flint implements had been found were remains of valley deposits resting on older deposits which he referred to the middle glacial.

He explained the various divisions of the middle glacial beds and correlated them with deposits of the same age in Hertfordshire, pointing out that there were several horizons at which loams occurred. He then showed that the beds in which the flint implements had been found rested upon various members of the middle glacial series and occurred in troughs and hollows scooped out of the middle glacial beds. In the case of the

Beeches Pit, opposite Culford, he said that the implements were found in a deposit which seemed to be the end of a terrace of valley gravel which, further down as it was followed towards Icklingham, became more clearly marked, consisting of gravel and brick-earth with pupa, pisidium, and mammalian remains.

The only deposits at all like boulder clay which either in the Beeches Pit or at Botany Bay overlaid these implement-bearing loams, he considered to be the wash either from boulder clay or directly from the chalk as the case might be.

Mr. R. H. Tiddeman then read "Some Observations on the Hyæna Bed at the Victoria Cave, and its Bearing on the Antiquity of Man." After some remarks on the disputed fibula formerly determined to be human, which had been found at a great depth in the cave deposits in the hyæna bed, the author went on to call attention to two bones, one certainly of goat, and another a rib of a small ruminant probably belonging to the same species, on both of which are cuts or hacks which appear to be the result of human workmanship. These were also found in the same bed at the depths respectively of 25 and 15 feet. In considering the age of these it was remarked that the condition of a bone is not necessarily a test of age, and in many instances might be a most fallacious guide. It was admitted that the goat has not been usually considered as introduced into Britain before Neolithic times, but it certainly had appeared in the Victoria Cave in association with the remains of hyæna, *Elephas antiquus*, *Rhinoceros leptorhinus*, and *Hippopotamus*. In the caverns in the neighbourhood of Dinant-sur-Meuse, in Belgium, M. E. Dupont records the goat as occurring in the lower beds in the same association. It seems, therefore, not improbable that it should have occurred in Britain at the same time.

If these are human workmanship, as appears probable, this cave holds only in common with other caves, the works of man so accompanied, and the actual finding of man or his works in the cave is a secondary question compared to the correlation of the beds with certain great and widespread physical changes.

The hyæna bed contains amongst others besides hyæna, the following—*Elephas antiquus*, *Rhinoceros leptorhinus*, and *Hippopotamus*. These were chosen as a well-marked fauna, about which no doubt was entertained that they were contemporary. They occur in non-gravels in France and Switzerland and in the south and east of England, and in each of these countries are associated with man's bones or handiwork. The geologists who have worked chiefly at the drifts of the south of England maintain, and rightly, that these remains are then post-glacial; but to infer that they are so in the north of England may lead to error. Their remains appear to have been removed from the open country there by glaciation, although from their existence in the Victoria Cave and another near Skipton they must at one time have been as abundant in the valleys as they are in the south. The author considered that this later glaciation was on the wane during its maximum at about the parallel of Derbyshire, and it appeared probable from authorities quoted that it had not extended over the southern end of the Pennine Chain. The glacial drifts further south and of earlier age than the animals referred to appeared to be the relics of an earlier glaciation than that of the north country, and extended further south. The author believed that the acceptance of two great and well-marked periods of glaciation differing in their extent would reconcile many of the differences which now exist amongst geologists as to the age of man and the drifts of this and other countries.

An interesting discussion followed, of which we hope to be able to give some account next week.

OUR ASTRONOMICAL COLUMN

The Revolving Double Stars.—Dr. Doberck, of Col. Cooper's Observatory, Markree, has published elements of ξ Bootis, calculated from measures extending over ninety-five years, which interval appears to be about two-thirds of a complete revolution. In this second computation for the same star he has followed a suggestion made in this column (Nature, vol. xiv. p. 475), with regard to the probable interpretation of Sir William Herschel's measures in 1792 and 1795, and his results prove the necessity for the alteration proposed.

We are now indebted to Dr. Doberck for orbits of thirteen of the revolving double stars, calculated in every case in the most complete manner possible from the available data, and which have been communicated from time to time to the Royal Irish Academy. They form collectively a very valuable contribution to this department of astronomy. Col. Cooper may be congratulated on such work emanating from his observatory, and Dr. Doberck likewise on the success which has attended his efforts. We subjoin the periods and eccentricities for Dr. Doberck's stars, omitting only ζ Aquarii, which from the great length of period is open to more uncertainty than the others :—

	Period. Years.	Eccentricity.
γ Coronæ Borealis	95·5 ...	0·350
ξ Scorpii	95·9 ...	0·077
ω Leonis	110·8 ...	0·536
ξ Bootis	127·4 ...	0·708
τ Ophiuchi	185·2 ...	0·582
η Cassiopeæ	222·4 ...	0·576
λ Ophiuchi	241·0 ...	0·493
44 Bootis	261·1 ...	0·710
μ^2 Bootis	290·1 ...	0·617
36 Andromedæ	349·1 ...	0·654
γ Leonis	402·6 ...	0·739
σ Coronæ Borealis	843·2 ...	0·750

The number of binary stars of which the orbits have been determined by various calculators with a greater or less degree of precision, now amounts to twenty-five. The shortest period of revolution hitherto detected belongs to 42 Comæ Berenices, which, according to M. Dubiago, of Pulkowa, in a communication from M. Otto Struve to the St. Petersburg Academy in May 1875, amounts to only 25·71 years. The star was single in 1845 and 1870–71; in 1829 and 1854–55 the distance of the components slightly exceeded six-tenths of a second, which is the greatest separation. The inclination of the orbit to the tangent-plane of the heavens is 90°, or so nearly so that the measures appear to be represented upon this assumption within their possible errors; thus the apparent orbit is a right line, with the direction 11° – 191°. Notwithstanding the difficulty of the case, M. Dubiago has been able to assign the other elements of the orbit with a fair degree of probability as follows :—peri-astron passage 1859·92, angle between the peri-astron and the node, 99° 11′, eccentricity 0·480, semi-axis major 0″·657. The distance of the components at the present time will therefore be 0″·50, with the smaller star on an angle of 11°.

From the elements of ξ Bootis by Dr. Doberck, to which reference is made above, the following appear to be the angles and distances, up to about the epoch of the approaching peri-astron passage, 1898·04 :—

	Pos. °	Dist. ″		Pos. °	Dist. ″
1882·0	271·7	3·81	1896·0	191·5	1·57
86·0	„ 259·7	„ 3·29	97·0	„ 174·0	„ 1·36
90·0	„ 242·8	„ 2·71	98·0	„ 150·8	„ 1·21
92·0	„ 231·1	„ 2·38	1899·0	„ 123·5	„ 1·17
94·0	„ 215·3	„ 2·00	1900·0	„ 98·2	„ 1·30

Physical Observations of Mars.—Mr. Marth has communicated to the Royal Astronomical Society an elaborate paper intended to facilitate physical observations of the planet Mars during the favourable opposition of the present year, when it is much to be desired that observations tending to improve our knowledge of the planet may be undertaken by those who are provided with adequate instruments. Mr. Marth has calculated the areographical longitude and latitude of the centre of the disc for the times of about ninety sketches of Mars, by Dawes, von Franzenau, Harkness, Kaiser, Lassell, Lockyer, Rosse, and Secchi, and with the aid of a table applicable to the interval June 9—December 14, with very little trouble the observer will be enabled to refer to the particular drawing which applies the most nearly to the time of any proposed observation, and will thereby be assisted in fixing upon the details of the surface to which it may be desirable to direct his attention. The table contains the angle of position of the axis of Mars, no doubt from Bessel's elements, or rather those deduced by Oudemanns from the observations of the Königsberg astronomer, the areographical western longitude and the latitude of the centre of the disc, the apparent diameter, the amount and position of the greatest defect of illumination, and the areocentric angle between the earth and sun, all quantities for Greenwich alternate noon. Vol. xxxii. of the "Memoirs of the Royal Astronomical Society," contains the sketches of Lassell, Lockyer, and Rosse, and this volume alone would be of considerable assistance to the intending observer, as will appear from Mr. Marth's second table.

CHEMICAL NOTES

CRYSTALLISATION UNDER GALVANIC CURRENTS.—A recent number of the *Journal* of the Russian Chemical and Physical Society (vol. ix., fasc. 2) contains an interesting report, by M. Shidlovsky, on observations he has made as to the microscopical crystallisation of various metals under the influence of a galvanic current. Placing on the object-glass of the microscope two fine metallic plates, the edges of which are about a quarter millim. distant, immersing them in a drop of water and passing a current through, M. Shidlovsky watched the growth of small ramified threads of crystals of metal transported from the cathode to the anode plate. The growth of these tree-like agglomerations goes on very speedily; their branches spread out to the anode plate, vibrate on reaching it, and collapse, whilst another ramified tree grows from the cathode spreading out to the anode; this goes on until the space between the plates is filled with a spongy metallic mass. Each of the metals experimented on (lead, silver, zinc, tin, copper, and iron) gives its own characteristic ramifications, and if the two plates be of different metals the tree has ramifications characteristic of the metal of which the anode plate is made. Gold and platinum do not exhibit any appearance of crystalline trees, nor does the crystallisation appear when the anode is gold or platinum. Iron submitted to a continuous current does not show a transport of crystals, but the phenomenon appears immediately when the currents are originated by a Ruhmkorff's coil or by a Holtz's machine. Iron-powder suspended in water undergoes a rapid motion under the influence of a strong inductive current, forming threads which spread out from the cathode to the anode plate.

ISODIBUTYLENE.—The same volume contains the second part of the important paper by Prof. A. Butlérof on the polymerisation of hydrocarbures from the ethylene series:—On isodibutylene.

ON THE THERMIC FORMATION OF OZONE.—M. Berthelot has recently investigated this question by subjecting pure and dry oxygen to the influence of the silent discharge, whilst passing the gas into a flask containing 500 c.c. solution of titered arsenious acid. At the end of thirty minutes, six to nine litres of oxygen had passed through the flask, the temperature being raised one-third of a degree; then by passing the oxygen current without the action of the discharge for an equal time, the thermal data were rendered complete. The arsenious acid solution was then treated with potassium permanganate, and redetermined with a solution of oxalic acid. By this means the quantity of arsenious acid oxidised, and consequently ozone absorbed, was determined. The amount of oxygen absorbed was found to be 30·3 and 51·9 milligrams, corresponding to 90·9 and 155·7 m.m. ozone, the heat set free being 118·2 and 223 calories respectively. Hence for one molecule the heat is equal to + 68·8 calories. Subtracting from this the heat formed in the oxidation of a molecule of arsenious acid + 39·2 calories (Favre and Thomsen), we have + 29·6 calories for the heat set free in the condensation of one molecule ozone into oxygen, and consequently − 29·6 in the reverse process. Ozone therefore is a body in which heat is absorbed in its formation, its activity in combination being probably due to this heat being set free. This is worthy of note when it is remembered that it is condensed oxygen, condensation generally setting free heat.

CHLOROPHYLL IN CONIFERÆ.—Coniferæ are remarkable amongst other plants for developing their chlorophyll even in places which seem perfectly dark. In the Reports of the *Naturforschende Gesellschaft* of Leipzig, Herr R. Sachsse publishes the results of some investigations he made in order to ascertain whether the chlorophyll formed under these circumstances is quite identical with ordinary chlorophyll. He extracted the chlorophyll from young Coniferæ, which had germinated in the dark, by boiling them in alcohol. He obtained a solution which showed the ordinary chlorophyll spectrum; all bands were in the right position and showed the correct grades of intensity. When the solution was concentrated the absorption at the end of the spectrum was continuous, when more diluted the absorption was resolved into the well-known three bands. The only peculiarity in this spectrum, when compared with that of chlorophyll of ordinary origin, was the somewhat lesser intensity of band V. According to Kraus's idea this would prove a predominance of cyanophyll over xanthophyll. The solution of Coniferæ chlorophyll very readily turns to modified chlorophyll.

CHEMISTRY OF THE GRAPE.—In several treatises lately presented to the Royal Academy of Physical and Mathematical Sciences of Naples, Prof. G. Licopoli gives an account of some recent micro-chemical researches upon oranges, lemons, and grapes. The latter are of special interest, as Prof. Licopoli tried to determine the time and place at which, in the grape, the different chemical substances which are contained in it (such as tartaric acid, chlorophyll, albuminoid matter, sugar, colouring matter, &c.) first begin to form. The conclusions which the author draws from his labours are the following: Tartaric acid and chlorophyll first show themselves in the tissue of the pistils in course of formation. Oxalate of lime next shows its presence in the sub-epidermic tissue in the form of raphides, in the endocarpic epidermis in the shape of conglomerated crystals (dumb-bells?), and in the kernels in raphides. The albuminoid matter first appears spread over the whole of the fruit, but predominates in the mesocarp. Colouring matter results from the metamorphosis of chlorophyll, its appearance and diffusion showing the growth of the fruit, and the progress the chlorophyll has made at the time of its formation. The growth of this colouring matter begins in the peripherical tissues, and continues towards the central ones. Sugar is found in the pericarp wherever there is tartaric acid present. Resinous matter or wax appears first on the surface of the epicarpic epidermis. Tannic acid is principally formed in the seed, and particularly in the hard and friable part of the episperma; the fibro-vascular fascicles of the pericarp, however, also contain this acid.

NOTES

WE regret to hear that the state of health of M. Leverrier, the distinguished director of the Paris Observatory, is causing great anxiety to his friends. He has been entirely prostrated by his enormous labours, which have been almost unceasing for the last twenty years.

M. BELGRAND read, at the last sitting of the Council of the Paris Observatory, a report on the necessity of extending telegraph warnings to Algeria, and taking advantage of the documents collected by the Algerine Meteorological Service. The necessary steps will be taken by M. Leverrier, and observations extending from Marocco to Tunis, and from the Mediterranean coasts to Laghouat and Biskra will be sent to and from Paris to every Meteorological Office in connection with the meteorological system. At the same sitting M. Leverrier announced that he had taken steps to utilise the weather telegrams sent from America by the *New York Herald*.

THERE is no ground whatever, we are informed, for the rumour that M. Krantz will resign his office of Director of the International Exhibition, or that the Exhibition will be postponed. The works are progressing favourably, and will not be interrupted. The British Commission have secured a large plot of ground close to the Champ de Mars for their private use. This ground measures more than 5,000 square yards.

THE Emperor of Brazil, who is now in Paris, has been assiduously attending the meetings of various scientific societies. On Friday he was present at the meeting of the Zoological Society. Several communications were read on fishes, insects, and worms from Brazil. He was also present at the last sitting of the Geographical Society of Paris. A paper was read on the Pampas by an American gentleman, whose flattery of the Emperor was so high that his Majesty left the room to show his disapproval. The lecturer stated that the Argentine Republic was building a strong wall to protect the Pampas against incursions from uncontrollable Indians, and that in doing so not less than 20,000 square miles of excellent grazing ground will be reclaimed.

THE Congrès Scientifique of France is holding its present session at Versailles on the occasion of a floral meeting, as we intimated two months ago. The principal attraction is a series of excursions held in the vicinity of Versailles.

THE Russian Council of State has granted a yearly sum of 2,000 roubles to the West Siberian Branch of the Russian Geographical Society at Omsk.

RUSSIAN newspapers announce that Prof. Ahlquist had reached, on April 10, Kondinsk, 530 miles north of Tobolsk. His companion, M. Bergroth, remained at Tobolsk.

SCIENCE in Italy has suffered a heavy loss through the death of Prof. Dr. C. L. Rovida, formerly first physician at the Ospedale Maggiore of Milan, and for the last three years Professor of Special Pathology and Clinical Medicine at the University of Turin. Next to Prof. Moleschott in rank, if not in fame, he was one of the few Italians who follow a rigidly scientific method of investigation and instruction.

THE fourth number of the *Bulletin* of the Geographical Society of Egypt contains an interesting account of a journey to Harar with a plan of that place, published by two Arabian officials; one of whom, Fayous Effendi, accompanied the Italian expedition to Zeilah.

MOUNT VESUVIUS shows signs of fresh activity. The crater is continually sending forth clouds of smoke which at night assume a fiery aspect from the deep-lying glowing lava masses.

CAPT. R. GESSI, the Italian explorer of Albert Nyanza under Col. Gordon, is now preparing at Cairo to undertake, on his own account, a new expedition in company with a naturalist and a photographer. He intends to push forward to the Equatorial Lakes, studying, on his route, all the principal facts of meteorology, anthropology, and natural history, taking sketches and photographs of men, animals, plants, and interesting geological features.

A CORRESPONDENT writes to us that the medal "of the first class" of the Paris Acclimatisation Society was presented, not to Mr. Alfred Mosenthal, but to Messrs. Julius de Mosenthal and J. E. Harting, the joint authors of the work recently published by Messrs. Trübner and Co., on "Ostriches and Ostrich Farming," and reviewed by us in vol. xv. p. 176. This work was published some time *after* the experiments, to which we referred last week, were made at Algiers. These experiments will be found to be fully detailed in the work in question.

A LETTER received at Rome from the commander of the *Scilla* states that the Italian expedition for the exploration of Central Africa were occupied at Zeilah on April 27 with the final preparations for their then imminent departure to Shoa.

IN the last two numbers of the *Bolletino della Società Geografica Italiana* Prof. Gio. Beltrame publishes an interesting paper on the language of the Akkás—an African tribe of which two individuals were brought over to Italy some years ago. This first attempt at giving an idea of the Akká language and its grammar will prove a valuable contribution to the study of comparative African linguistics

AN immense quantity of locusts have shown themselves in the Algerian provinces, and are travelling from the south towards the Mediterranean. The number of these insects was so prodigious that the trains from Blidah to Algiers were almost stopped in the beginning of May.

MR. JOHN F. DOLLEY writes to the *Times* under date Uitenhage, Cape of Good Hope, South Africa, March 19:—"In this part of South Africa we have just witnessed a magnificent sight, such as a person can hardly expect to see more than once in a lifetime, if even then. It was on the beautiful clear starlight evening of the 16th of March, at about eight o'clock, when suddenly every one was startled with a bright lightning, like a flash, and on looking for the cause discovered a large meteor coming out of the eastern horizon, and which travelled slowly across the firmament, in an oblique direction to the westward, when it burst, sending forth streams of fire, as if from a hundred rockets, and then was heard a low rumbling noise as of thunder in the distance. The meteor appeared to be nearly, if not quite, as large as the full moon, but not round, more of an oblong shape, and while travelling through the air it very much resembled a large turpentine ball. It gave forth a bright bluish light which lit up the whole sky, and you could distinguish everything around you for miles as plainly as in the daytime. . . . A party of Hottentots who were coming in from 'Hankey,' a station belonging to the London Missionary Society, state that the driver of the waggon was struck down in the road, and that they all felt a glow of heat as the fireball passed them. The illumination lasted for nearly a minute, and the light was such that it dazzled the eyes of all who saw it."

A TELEGRAM from New York on the 16th states that forest fires are making great ravages in North-eastern New York, Long Island, Massachusetts, New Hampshire, Maine, Pennsylvania, Canada, and New Brunswick. A large part of the White Mountains is in flames. The summer hotels are in danger, and railways are interrupted. A great number of mills and dwellings have been destroyed, and hundreds of persons have been rendered destitute.

STEAM at ordinary pressure sent into saline solutions on which it has no chemical action, gives a rise of temperature that seems at first sight paradoxical, the temperature produced being always higher than that of the steam. M. Müller, of the Berlin Chemical Society, has been studying the phenomenon. Chloride of sodium is one of the best salts to use. A solution of it sufficiently concentrated to have a boiling point of 127° may be raised to 125° simply by sending steam into it at 100°. Here, then, the steam produces a rise of 25° above its own temperature. The more concentrated the solution the higher is the rise. M. Müller points out, in explanation, that saline solutions at 100° absorb the steam at the same temperature, and the result is a rise analogous to that produced when a gas, like ammonia, is dissolved in water. These experiments throw new light on the controverted question, what is the temperature of the steam which escapes from a concentrated and boiling solution? Is it 100° or a temperature near that of boiling of the solution? The new results seem to be against the latter, and common, view.

INTELLIGENCE has been received from Quebec stating that ships which have recently arrived at that port have encountered unusually large fields of ice and remarkably high icebergs in the Atlantic. The *Una*, from Leith, passed through eighty-five miles of heavy ice.

AT the meeting of the Royal Society of Edinburgh on Monday night, reports were read from four lighthouse keepers on the west coast of Scotland, detailing their experiences of earthquake shocks on March 11 and April 23. The keeper of the Pladda lighthouse says the tower by his dwelling-house shook very much; the Lismore keeper reported that everything in his lighthouse shook at an alarming rate and awoke all the inmates. Mr. Stevenson, C.E., said these observations were valuable because of their trustworthiness.

THE engineers of the French Northern Railway have been making experiments with the vacuum brake, which has been found to work satisfactorily owing to its simplicity of construction. MM. Sartiaux and Lartique have devised some ingenious arrangements for bringing it into action automatically if any mistake has been made respecting the crossings. Should distressed passengers want to call for help they can also put the continuous break into operation instead of ringing a bell as is customary.

THE Superintendent's Report on the Botanic Garden and Public Plantations for 1875-76 has recently been officially published in Jamaica. It deals almost entirely with plants of economic value, foremost of which is the coffee, the ordinary kind (*Coffea arabica*), apparently giving way to its formidable rival *Coffea liberica*, which was introduced to Jamaica in 1874, and is now thriving, especially in some districts. In one situation, at a height of about 1,000 feet above the sea, a plant that had only been planted out a little over a year has already fruited. This seems to indicate that in the course of a few years the new coffee may be widely cultivated in Jamaica from plants raised from seeds ripened in the island. Amongst other important plants treated of in the report which have received special attention, may be mentioned cocoa, sugar canes, pine apples, cinchonas, jalap, &c.; of this last we learn that nearly two acres are under cultivation, producing during the year under review a crop of 1,700, and it was estimated at the time the report was written that an additional 3,000 would be obtainable in the course of a few months, all of which would find its way to England.

DR. LAUDER LINDSAY of Perth has for years been forming a collection of lichens, which, although frequently broken up and distributed, still forms the nucleus of a good type collection. The collection consists of—1. Herbarium, the main object of which is to illustrate the Variations of the Commoner Species: and so to encourage (1) the establishment of Typical, Comprehensive, or Aggregate Species; (2) the abolition of Named Trivial Variations; and the (3) consequent Reduction of Names, and Simplification of Synonymy and Classification. 2. Museum of Illustrations of the Economic Properties and Applications of Lichens. 3. Library of Lichenological Works, Foreign and British. 4. Drawings (original) of Microscopical Structure, several thousands unpublished. 5. Correspondence with Lichenologists. 6. Unpublished Manuscripts—of (1) Outlines of Lichenology:" (2) "Lichenographia Britannica:" and of other works or papers illustrative of Lichens in various aspects. The collection has been offered—with all the cabinets and fittings in which it is contained—as a donation to the Royal Botanic Gardens of Edinburgh, on the simple conditions—(1) That a small room is provided for its accommodation in connection either with the Herbarium, or Museum of Economic Botany; and (2) That it is kept in proper order either by the Curators of said Herbarium or Museum, or by any of the numerous students of the Edinburgh School of Botany. But it is understood that no such donations can be accepted by the said school, by reason of the very inadequate accommodation provided by Government for the more essential requirements equally of students and teachers. As has been repeatedly pointed out—officially and otherwise—there is at present urgent need—(1) of a new commodious class or lecture room; (2) of a new commodious museum; (3) of botanical laboratories; and (4) of extra special rooms for such herbarian or other purposes as the reception and maintenance of such donations as that now referred to.

A NEW burner for obtaining high temperatures in laboratories has recently been described by M. Godefroy. It consists of four metallic cylinders one within another; the first and the third are pierced with lateral holes at their base. The intervals between the cylinders communicate, one set with two vertical pipes uniting in a horizontal pipe below, the other set with another similar system. A piece of metallic net at the lower part regulates the entrance of air.

AT Tabor, in Bohemia, 423 metres above the sea, in a house out from the town, M. Farsky made observations of the amount of carbonic acid in the air from October 10, 1874, to the end of August, 1875. The average obtained was 3·43 volumes in 10,000 volumes of air; a number smaller than that of Saussure and Boussingault (4·15), and higher than those got by Schulze in Rostock and Fittbogen in Dahme. The most numerous variations are in November, December, February, March, and April, the least in October. M. Farsky says that the more variable the weather, and the sharper the transition from one weather to another, the greater are the variations in proportion of carbonic acid in the air. The strong north-west and south-west winds reduce the amount of carbonic acid, while the cold north and north-east winds, which are always thought the heralds of clear weather, cause an increase in the carbonic acid. Further, the carbonic acid is increased descending mist, and continuous dust-rain. No other connection with atmospheric precipitates was perceptible. These results agree, in the main, with those formerly obtained by Angus Smith and Roscoe. Smith found that the air in the suburbs of Manchester contained on the average 3·69 volumes in 10,000 volumes of air. The amount appeared to diminish slightly during long-continued westerly winds; on the other hand it increased when easterly winds prevailed. From a large number of analyses of air collected from the hill-districts of Scotland, Smith obtained, as an average, 3·36 volumes in 10,000 volumes; the extremes recorded are 3·00 and 3·60. These comparatively low numbers are probably due to the proximity of the sea, the air over which has been shown by Thorpe, from a large number of analyses made over the Atlantic Ocean and Irish Sea, to contain about 3·00 volumes of carbonic acid in 10,000 volumes of air. This amount was constant, or nearly so, in different latitudes, and, contrary to the statements of Lewy, exhibited no perceptible diurnal or seasonal variations.

THE additions to the Zoological Society's Gardens during the past week include two tigers (*Felis tigris*) from Jahore, presented by Rear-Admiral Rowley Lambert, C.B.; two Javan Chevrotains (*Tragulus javanicus*) from Java, presented by Messrs. Hill and Isaac, Lieuts. R.N.; a Malayan Bear (*Ursus malayanus*) from Sumatra, presented by Dr. F. Wicksteed; a Phatagin Manis (*Manis tricuspis*) from West Africa, a Humboldt's Saki (*Pithecia humboldti*) from the Amazons, a Red and Yellow Maccaw (*Ara chloroptera*) from South America, a Sooty Mangabey (*Cercocebus fuliginosus*) from West Africa, deposited; a Concave Casqued Hornbill (*Buceros bicornis*), three Yellow-billed Blue Magpies (*Urocissa flavirostris*) from India, a Pin-tailed Whydah Bird (*Vidua principalis*) from West Africa, received in exchange; two Impeyan Pheasants (*Lophophorus impeyanus*) from the Himalayas, a Mantchurian Crossoptilon (*Crossoptilon mantchuricum*) from North China, purchased.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At a Congregation on May 17, the Vice-Chancellor, the Master of Emmanuel, Professors Cayley, Adams, Clerk-Maxwell, Stuart; Messrs. P. Frost, St. John's; J. Todhunter, St. John's; H. W. Besant, St. John's; N. M. Ferrers, Caius; E. J. Routh, St. Peters; A. Freeman, St. John's; H. H. Taylor, Trinity; W. D. Niven, Trinity; R. T. Wright, Christ's; C. H. Prior, Pembroke; W. Garnett, St. John's; and Lord Raleigh, Trinity, were appointed a Syndicate to consider the higher Mathematical Studies and Examinations of the University.

The Chancellor's gold medal, given annually to a resident undergraduate who shall compose the best English poem, has been adjudged to Edmund Whytehead Howson, Scholar of King's College. The subject of the poem is "The Heroism of Arctic Exploration."

The Moderators and Examiners for the Mathematical Tripos have presented a report of the results of the examination to the Board of Mathematical Studies, from which it appears that 110 candidates were examined. Of that number 36 were classed as Wranglers, 29 as Senior Optimes, 30 as Junior Optimes, one obtained an Ægrotat degree, and 14 were found to be not worthy of mathematical honours. The full marks were 18,643; the average obtained by the first ten Wranglers was 5,748; of the last ten Wranglers, 1,794; of the first ten Senior Optimes, 1,506; and of the first ten Junior Optimes, 721. The Additional Examiner (Mr. H. W. Watson) reports that the work done by the best men in the higher physics was very satisfactory, and proved the interest and success with which those subjects are now being studied in the University. At the same time there were indications of the tendency of the reading in this part of the course to become too diffuse and unmathematical. This tendency may be kept in check, in the first place, by framing the papers in such a manner—whether by the adoption of alternative questions or otherwise—as shall make too diffuse reading in the higher subjects unprofitable or even impossible; and, in the second place, by providing that every question set in these subjects shall be accompanied by a rider of strictly definite mathematical character, and of a difficulty proportioned to the weight assigned to the bookwork.

The Board for admitting and superintending non-collegiate students give notice that an exhibition of 50*l*. a-year, tenable for three years, granted by the Worshipful Company of Clothworkers for the encouragement of proficiency in physical science, will be awarded by means of the Certificate Examination, to be held next December, under the authority of the Oxford and Cambridge Schools Examination Board. Candidates must be either non-collegiate students in their first term of residence, or persons who have not commenced residence in the University. Full information may be obtained from the Censor of non-collegiate students, the Rev. R. B. Somerset, Cambridge.

GILCHRIST TRUST PRIZES.—The first (in London) presentation of prizes in physiology under the direction of the Gilchrist Trustees was made on Tuesday, last week, at the large room of the Society of Arts. The chair was occupied by the Rev. J. Rodgers, M.A., Vice-Chairman of the London School Board, supported by Dr. Carpenter, C.B., F.R.S., secretary to the Gilchrist Trust Fund, and others. The prizes were awarded to students who, as elementary school teachers, had attended the course of lectures on physiology recently delivered by Dr. B. W. Richardson, at St. Thomas's School, Charterhouse, and who had submitted to the examination with which the course was brought to a close. A large number of students entered into competition, and in the end prizes and certificates were awarded in the following order to four competitors:—John Pilley, George Price, W. R. Cory, and Maria J. Menzies; and certificates to Mary C. Menzies and Messrs. C. E. Marks, C. W. Shreeve, H. Steadman, J. F. Adcock, and G. Garland. In the course of the proceedings Dr. Carpenter gave a very interesting account of the origin of the Gilchrist Trust and of its founder, and the chairman delivered a very earnest and admirable address on the progress of education and on the value of the lectures such as had been delivered, and which he had himself attended.

WORKING MEN AND SCIENCE.—On Saturday afternoon the members of the Working Men's Clubs, under the auspices of the Working Men's Club Union, paid a visit, by permission of the Royal College of Surgeons, to the magnificent museum founded by John Hunter, and attached to the building of the College in Lincoln's Inn Fields. Prof. Flower, the Curator, received the visitors, who were conducted into the first great hall, where Prof. Flower gave a general description of the Museum. The visitors manifested an unmistakably genuine interest in the collection, and in Prof. Flower's descriptions, and at the conclusion of the visit one of the party, on behalf of his comrades, gave hearty thanks to the Professor and to the Council of the College, for the treat which had been afforded to them, and said it was altogether a mistake to suppose that the working men took no interest in science. The Professor said it afforded him much pleasure to show the museum, and especially so when he found his labours thus appreciated.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 3.—"On the Temperature-correction and Induction-coefficients of Magnets," by G. M. Whipple, B.Sc., Superintendent of Kew Observatory. Communicated by Robert H. Scott, F.R.S.

"Distribution of the Radicals of Electrolytes upon an Insulated Metallic Conductor," by Alfred Tribe, Lecturer on Chemistry in Dulwich College. Communicated by Dr. Gladstone, F.R.S.

May 17.—"On Hyperjacobian Surfaces and Curves," by William Spottiswoode, M.A., Treas. R.S.

Royal Astronomical Society, May 11.—Dr. Huggins, F.R.S., president, in the chair.—A gift of 500*l*. was announced from Mr. C. J. Lambert, being part of the sum bequeathed by his late father to scientific societies. The special thanks of the meeting were voted to Mr. Lambert.—The Astronomer-Royal pointed out an inaccuracy in a description of meteors by a certain "J. W. M."—Mr. Penrose read a paper (and explained a diagram) on the correction for the spheroidal figure of the earth.—Lord Lindsay spoke upon the two comets B and C of 1877. Winnecke's showed three bright lines on a weak continuous spectrum which he described. The president made some remarks thereon: there were two distinct spectra shown by comets; one was limited to two particular comets; the carbon spectrum was common to all the rest.—Lord Lindsay described the 4-inch heliometer which he had placed at the disposal of Mr. Gill for his expedition to Ascension to measure the parallax of Mars. The object glass was made by Mertz, and cut and mounted as a heliometer by Repsold; the pillar and equatorial mounting being that provided by Messrs. Cooke, of York, for an 8-inch telescope. The halves of the object-glass were moved in circular grooves by means of a handle near the eye-piece, so that they could be separated without putting the object out of focus. Several other details were pointed out, one of them being a movable wire screen adapted to equalise the light of two objects of different brightness under measurement. Mr. De la Rue admired the stability and rigidity of the instrument. The Astronomer-Royal would have had the declination axis and the polar axis twice as large. Mr. Gill vindicated the steadiness of Lord Lindsay's heliometer, and described an accident by which it narrowly escaped being smashed; which accident occurred through the instrument having been represented as an "universal equatorial," whereas it was nothing of the kind; the elevating screw having run out whilst being set to the latitude of ascension, the polar axis was shot out of its collar on to the floor. Mr. Gill then spoke upon the positions of the planets Ariadne, Melpomone, and Iris, and their special merit of having no sensible disc.—Dr. De la Rue again referred to the axes of the heliometer, which he said were eight times as strong as Repsold thought sufficient. The Astronomer-Royal said it was perfectly adapted to the purpose intended.—A note was read from Padre Secchi on an alleged fall of a meteorite in Italy which turned out to be untrue.—Mr. Lecky related how he had made a good artificial horizon by filling a blackened trough with glycerine.—A catalogue of double stars was presented by Messrs. Wilson and Seabroke, of the Temple Observatory, measured with a parallel wire micrometer and a power of 400. Mr. Dunkin said the only fault was the omission of the R.A. and N.P.D., which necessitated the use of two catalogues.—A note was read from Mr. Proctor referring to his chart of 3,976½ stars.—Mr. Green laid before the Society some paper impressed with blank discs to aid observers in drawing the features of Mars, so prepared that lights could be taken out with ease and precision.—Mr. Christie described Prof. Zenger's solar eye-piece, which consisted of a prism with one lenticular surface, cemented to another prism of different density; the quantity of light reflected at the junction being in proportion to the difference of the density of the two glasses.

Linnean Society, May 3.—Prof. Allman, F.R.S., president, in the chair.—Three foreign members were elected, and Mr. James Paton, of the Kelvin Grove Museum, Glasgow, was likewise elected an ordinary fellow.—Specimens of abnormal primroses were exhibited and commented on by Mr. Alf. W. Bennett.—A paper on the perfoliate penny-cress (*Thlaspi perfoliatum*) was read by Mr. G. S. Boulger. This little plant has a very limited British area, viz., the neighbouring districts of Oxfordshire and Gloucestershire, and according to our author Wilts; thus equally belonging to the Thames and Severn Valley provinces. The altitude attained is from 360 to 500 feet above the sea-level.—A conjoint memoir by Prof. St. G. Mivart and the Rev. R. Clarke on the sacral plexus and sacral vertebræ of lizards was brought forward and discussed. They stated that it has of late been recognised that in any attempt to reply to the question, which vertebra of any lower animal answers to the first sacral one of man, the nervous quite as much as the bone relations require consideration. Our authors pass in review the researches of Gegenbaur and Hoffmann, and then proceed to describe their own dissections of the parts in question in the chameleon, green lizard, iguana, monitor, and others. Instituting a comparison of the parts in the Batrachia, and of the sacral region in birds, they, in a somewhat technical summary, announce that although often puzzling and complicated from occasional variations in species and otherwise, the true sacral vertebræ may be defined in all vertebrates above fishes, where hind limbs are well developed.—The Secretary read a paper on the genus *Alveolites* and some allied palæozoic corals, by Prof. Nicholson and Mr. R. Etheridge, jun. It seems from their researches that the name Alveolites covers many forms whose affinities, to say the least, are obscure. Discussing the characters and essential attributes of the genus in a historical *résumé*, they proceed by comparisons, microscopic and otherwise, to define certain groups coming under previous definitions of *Alveolites*. These are several species of the above and others of genera such as *Cœnites*, *Brachypora*, *Chætites*, &c. But moreover they state that in several instance there appears much in common between certain groups of *Alveolites* and *Favosites*, so that future investigation may further necessitate the breaking down of what at present may be regarded as but meagre lines of demarcation.

Chemical Society, May 17.—W. Crookes, F.R.S., vice-president in the chair.—The chairman announced that an extraordinary general meeting would be held on May 31 at 8 P.M. The following papers were read:—On a slight modification of Hofmann's vapour density apparatus, by M. M. P. Muir and S. Suguira. The authors propose to omit the india-rubber plate of the original apparatus, and mark off the height of the mercury by a cathetometer and a slip of gummed paper.—Note on the fluid contained in a cavity in fluorspar, by J. W. Mallet. The cavity was 6 mm. by 2·5 mm. by 1 mm.; it contained water and a bubble. On heating, the bubble became less mobile and the crystal showed signs of incipient splitting.—Examination of substances by the time method, by J. B. Hannay. The author has determined the loss sustained by various hydrates in equal and successive intervals of time, when submitted, in a Liebig's drying tube, to a current of air at various temperatures, and thus obtains evidence of the existence of hitherto unknown hydrates. Magnesium sulphate, when treated as above, loses 8 per cent. of water in five minutes at 100° C.; the loss is then much slower and regular up to 29 per cent., when the rate of loss decreases somewhat suddenly from the formation of a lower hydrate, which loses water much more slowly.—On the dehydration of hydrates by the time method, by W. Ramsay. The author examined the hydrates of alumina, iron, copper, and lead.—On the transformation of aurin into rosaniline, by R. S. Dale and C. Schorlemmer; by heating sulphuric acid and pure phenol, and gradually adding oxalic acid, pure aurin is formed; by the action of ammonia on aurin, red aurin is produced, which, by the action of alcoholic ammonia at 150° for several days is converted into rosaniline. The authors consider aurin to be identical with rosolic acid.—On certain bismuth compounds, Part VI., by M. M. P. Muir. The author describes the preparation, &c., of hypobismuthous oxide, bismuthous oxychloride and oxybromide, and sulphbismuthyl chloride.—On the theory of the luminous and non-luminous flame by J. Philippson. The author states what he considers to be the causes of the luminosity and non-luminosity of flames.

Zoological Society, May 15.—Prof. Mivart, F.R.S., vice-president, in the chair.—Mr. Sclater made some remarks on the progress and condition of the Zoological Gardens of Rotterdam, Amsterdam, Antwerp, Brussels, and Ghent, which he had just visited.—A communication was read from Mr. G. S. Brady, C.M.Z.S., containing a monograph of the fossil Ostracoda of the Antwerp Crag.—A communication was read from Dr. F. Day containing a notice of the capture of a specimen of *Coregonous oxyrhynchus*, on the coast of Lincolnshire.—A communication was read from the Marquis of Tweeddale, F.R.S., containing a memoir on the birds of the genus *Batrachostomus*. The author came to the conclusion that there were seven recognisable species of this difficult group inhabiting the Indian region, one of which yet undescribed, was from the Philippines. The rule appeared to be that the females were rufous from the nest, while the males are brown and somewhat spotted.—Mr. Edward R. Alston read the description of a shrew from Guatemala, which had been indicated without being characterised by the late Dr. Gray, and for which the name of *Sorex veræ-pacis* was now proposed.—Mr. A. H. Garrod, F.R.S., read the second portion of a series of papers on the anatomy of passerine birds.—A communication was read from Mr. T. E. Buckley containing remarks on the past and present geographical distribution of the larger mammals of South Africa.

Entomological Society, May 2.—J. W. Dunning, F.L.S., vice-president, in the chair.—Messrs. H. J. Adams, Charlestrom, Adams, and J. W. Slater were elected members of the Society.—Mr. Jenner Weir exhibited a large silken cocoon from the Cape of Good Hope, supposed to be a spider's nest. On being opened it was found to contain, among other *débris*, the skins of a number of small spiders and the elytra of beetles of the genus *Moluris*. Mr. Weir also exhibited a spider's nest from Montserrat.—Mr. F. Grut exhibited a large species of *Chelifer* from North Spain.—Sir Sydney Saunders exhibited a spider (*Atypus sulzeri*) taken on Hampstead Heath, where it is found inhabiting tubes concealed under bushes. The tubes are about fourteen inches in length and extend about ten inches beneath the surface of the ground, the remainder projecting above the surface. The same or an allied species had been observed by Mr. Jenner Weir on the South Downs.—Mr. Champion exhibited a series of *Alaus pareyssi* from Thaso Island.—Mr. C. O. Waterhouse exhibited specimens of the following insects from Tasmania:—*Dohrnia miranda* (Heteromerous beetle), *Creophilus erythrocephalus* (*Staphylinidæ*), and *Forficula erythrocephala*. The two last species bore some mimetic resemblance to each other.—A paper was read from Sir Sydney Saunders on the adult larvæ of the *Stylopidæ* and their puparia, the author exhibiting specimens in illustration.—Mr. H. W. Bates communicated a paper on *Ceratorhina quadrimaculata*, Fab., and description of two new allied species. Specimens of the new species and also of *C. morgani*, Westw., were exhibited.

Physical Society, May 12.—Prof. G. C. Foster, president, in the chair.—The following candidates were elected members of the society:—Capt. R. Y. Armstrong, R.E., Mr. W. H. M. Christie, Lieut. N. Darwin, R.E., Prof. E. Frankland, D.C.L., F.R.S., Mr. H. F. Morley, Capt. R. G. Scott, R.E., and Mr. Angus Weiss. Mr. S. P. Thompson read a paper on the chromatic observation of the eye in relation to the perception of distance. He discussed the various means of estimating distances by the eye, showing that when data for forming a judgment by the associations of visible form or visible magnitude fail, the judgment is founded on "aerial perspective," or else upon the muscular sensation of adjustment to focus. As the eye is, however, not achromatic, it cannot be in focus at the same time for red rays and blue rays proceeding from one object, but may be in focus if the blue rays come from a more remote object. This gives a definite basis to the axiom of painters that blue is a retiring and red an advancing colour. Experiments were described demonstrating the truth of this fact, and illustration was afforded of the chromatic aberration of the eye by casting beams of light through a solution of permanganate of potash upon a silvered ball, the illuminated point appearing red with a blue surrounding halo to an eye adjusted to short focus, but blue with a red halo to long focus.—Prof. Guthrie referred to the theory by which the apparent size of an object depends on the amount of nervous excitement which it occasions, whether this be due to the extent of the illuminated area or the intensity of its illumination, and he pointed out that an object always appears larger when looked at with two eyes than with one eye.—Mr. Roberts drew attention to the fact that the system ordinarily adopted in mechanical drawing of assuming the light to fall from the left hand top-corner gives an appearance of solidity, whereas if this be reversed, and the light falls from the right-hand bottom corner the object appears hollow.—The president referred to the well-known fact that if two stereoscopic pictures are taken, representing the same object in complementary colours, most people have a great difficulty in combining them so as to see a single picture of a neutral tint.—Mr. S. P. Thompson then described a curious observation of change of pitch occurring when a tuning-fork is caused to rotate rapidly round its axis; the nodal interferences at each quarter rotation ceasing to be separately heard when recurring more than about thirty times in a second. He has attempted various ways of estimating the amount of this change of pitch, including a method founded on the binaural estimation of interference beats.

Institution of Civil Engineers, May 8 and 15.—Mr. George Robert Stephenson, president, in the chair.—A paper by Sir G. W. Armstrong, C.B., F.R.S., V.P. Inst. C.E., on the history of the modern development of water-pressure machinery was read.

CAMBRIDGE

Philosophical Society, May 7.—Prof. Clerk Maxwell, president, in the chair.—Mr. J. W. L. Glaisher read a paper on expressions for the theta functions as definite integrals.—Mr. Warren's fourth "Exercise in Curvilinear and Normal Coordinates" was presented to the Society by Prof. Cayley, and will appear in the next issue of the Society's *Transactions*.

PARIS

Academy of Sciences, May 14.—M. Peligot in the chair.—The following papers were read:—Isoperimetric triangles having one side of constant size and the summit at a fixed point, by M. Chasles.—Rotatory action of quartz on the plane of polarisation of obscure calorific rays, by M. Desains. He has examined the action of six groups of dark rays of decreasing refrangibility. The law of thicknesses still applies to the least refrangible rays. In the symmetrical dark region of nearly extreme violet the polarised heat is so little sensible to the action of quartz that transmission through a plate 0·015 m. thick, gives hardly a rotation of 5 degrees, or $\frac{1}{3}$ degree per millimetre. This is 132 times less than for the violet of M. Biot's table. M. Desains describes his apparatus fully.—Analysis of an ancient wine, preserved in a glass vessel sealed by fusion, by M. Berthelot. This was found on the site of an old Roman cemetery near Arles, and probably dates back some sixteen hundred years.—Analysis showed in a litre, 45 c.c. alcohol, 3·6 gr. fixed acids, 0·6 bitartrate of potash, 1·2 acetic acid. There were also tartrate of lime and traces of acetic ether. It is a weakly alcoholic wine, which must have entered on acetification before being put in the tube.—M. Serret presented tome vii. of the "Œuvres de Lagrange," completing the series.—On the new navigation, by M. Villarceau.—On the origin and nature of the fever called typhoid, by M. Guerin. The object of this third memoir is to show that the toxical principle produced by stercoral fermentation causes what are looked on as the anatomical characters of the fever. M. Guerin has proved that the liquids specially poisonous are those which accumulate near the end of the ileum, and are permanently retained by the ileo-cœcal valve. The poison passes into the mesentery and to the ganglions contained in it.—New exposition and generalisation of the method of Gauss for calculating approximately a definite integral, by M. Pujet.—New meteorological maps of the South Atlantic, giving at once the direction and the intensity of the winds, by M. Brault. The general movement of the summer winds there is that of an immense cyclone with its centre about 30° or 35° south latitude, and 10° or 20° west longitude. It turns in opposite directions to the hands of a watch, and gives off the south-east trades towards Africa, &c. There is not a zone of tropical calms, nor a zone of weak and arbitrary breezes.—On a new type of simple monstrosity, omphalocephaly or umbilical hernia of the head, by M. Dareste. The head seems to come from the aperture of the umbilicus. The form has been observed in the hen, but not in man or mammalia.—Experiments made at the vitreolar station of Cognac with the view of finding an efficacious remedy against phylloxera, by M. Mouillefert. This is in favour, specially, of sulpho-carbonate of potassium.—On a modification of the pneumatic mercury machine, by M. Serret. This consists in substituting a simple valve for the glass stopcocks. In another form, even the valve is suppressed, and vacuum obtained by free circulation of mercury in simple tubes.—On solar spots, by M. Tacchini. He thinks the sun's surface at present in a true state of repose relatively to the great phenomena observed at a time of maximum sun-spots. He shows the contrast in metallic eruptions and spots between 1871 and 1876 by figures. In the first four months of 1877, he adds, there has been a diminution in the visibility of magnesium, so that the line 1474 κ has had a marked superiority.—On the otheoscope, (a new arrangement of the radiometer), by Mr. Crookes.—Direct transformation of mechanical work into electricity, by M. Guignet. In an electromagnetic machine, having six electromagnets and a drum with six bars of soft iron, the wires are connected with a galvanometer and the drum is rotated by the hand. A continuous current is thus produced, and it is reversed on reversing the rotation. This experiment also shows the induction taking place under the influence of the earth; the soft iron is magnetised, and the magnetisation increases with the speed of rotation.—Note on work in chemistry at the Polytechnical School of Rio de Janeiro, by M. Guignet.—On work of the School of Mines of Ouro Preto, by M. Gorceix.—On some monochlorised acids of the amylic series, by M. Demarçay.—On the salts of sesquioxide of chromium, by M. Etard.—Researches on pseudopurpurine; continuance of researches on the colouring matters of madder, by M. Rosenstiehl. The remarkable instability of pseudopurpurine (he says) is a happy circumstance; as neither it nor alizarine could give a plant the extraordinary importance of madder.—On an application of the microscope to ceramic art, by MM. Fouqué and De Cessac. This refers to vases found at Santorin.—On a new larval form of Cestoides, by M. Villot.—On granular conjunctivitis in Egypt; *résumé* of observations on ophthalmias of North Africa, by M. Gayat. Eye diseases are endemic in North Africa, and have their common origin in granular conjunctivitis, which is brought on by atmospheric and terrestrial conditions.—Treatment of hypopyon, by M. Fano.—On M. Maumené's gas hydrometer, by M. Dumas.—A new arrangement of the electromagnetic induction apparatus with automatic interruption, by M. Becquerel.—On the glaciers of Greenland, by M. Malland.

ROME

R. Accademia dei Lincei, April 15.—The Age of Bronze in the piles of Peschiera in the Lago di Gardo, by M. Ferri.—On the use of the reversed siphon in the ancient conduits of water, by M. Lanciani.

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