THURSDAY, SEPTEMBER 26, 1878

THE SUPERFICIAL GEOLOGY OF SOUTH-WEST LANCASHIRE

The Superficial Geology of the Country adjoining the Coasts of South-West Lancashire. By C. E. De Rance, F.G.S. 1877.

THE memoir of the Geological Survey by Mr. De Rance recently published is an interesting contribution to our knowledge of the superficial deposits of the area between the Mersey and the Ribble, which carries the classification of the Cheshire Plain as far to the north as Morecambe Bay. The whole of this district is covered with glacial drift and recent sands, gravels, and peat-bogs, except here and there where the solid rocks come to the surface in the hills. The drift forms an inclined plane dipping from the hills towards the sea, and probably deposited during subsidence upon an old rocky plain of marine denudation, bounded to the east by a line passing from Eccleston to Euxton and Ribchester, and thence through Broughton, Garstang, and Cockerham to the present sea-margin. Were the superficial deposits stripped off this area, the rock-surface would be seen to be not very far from the present sea-level, although the surface of the ground is often 170 feet above it. This plain also dips gently seaward, and has been worn into hollows by the denuding forces before the glacial period. Very generally it has been cut up into hills and valleys by pre-glacial streams, as, for example, the buried valley of the Mersey described by Mr. Mellard Reade, now filled with 200 feet of sands, gravels, and clays. These buried valleys may be traced inland, rising nearer to the present surface of the ground as we approach the high ground, until at last their tributaries come to the active surface in the higher hills, and are traversed by the same streams as those now finding their way to the surface, and through the accumulation of drift filling their ancient lower courses. It seems tolerably certain that the hill and valley system of Lancashire and Cheshire was produced by sub-aërial agents before the glacial period, and that the ice merely acted on the solid rock by rounding off and smoothing the raw edges left by the streams and rivers. Indeed, as a rule, it may be said that the relative importance of the agency of rain and rivers, and of ice in scenery making is precisely that of chisel and sand paper, the one carves, while the other rounds off, smooths, and polishes. But whatever view may be held of the cause of this uneven surface below the mantle of the drift, it is a most important fact to be noted, that the surface configuration bears little or no relation to the rock-surface below, as engineers have frequently found out by experience in making reservoirs. In one case, for example, the "puddle trench" had to be carried down 160 feet, so as to render a ravine filled with drift water-tight, and this ravine, with the big boulders at the bottom left in the bed of the stream, by which it was hollowed, was intercepted twice in the course of the works.

The drift of the district under consideration is treated by Mr. De Rance in three divisions, the lower and upper boulder clays, separated from each other by the middle glacial marine sands and gravels. There appears to be no important physical difference between these clays, and their relative age can only be ascertained by their relation to the sands above mentioned. This, however, is not an infallible guide, because there are lenticular strata of sand and gravel intercalated here and there in the boulder clays. According to Mr. De Rance they are absent from the base of the lower boulder clay, a position which Mr. Binney has shown them to occupy in other districts in Lancashire and Cheshire. Our author notices also the glacial striæ, *roches moutonnées*, and the moraines on the higher grounds overlooking his area, and points out very justly (p. 46) that "the till" and "lower moraine drift" of other districts may have been formed at the same time that the lower boulder clay was being accumulated. It may also be pointed out that the local glaciation of North Wales and of the Pennine chain, and of the hills of Cumberland may have been produced while the upper boulder clay was being formed. Nevertheless, we cannot obtain an accurate idea of the relation of the various glacial phenomena to one another in point of time in different parts of Britain, until we can ascertain the sequence and extent of the changes of level, which has not as yet been made out. To my mind three great changes only have been proved to have taken place over a wide area: two periods when Britain stood at a higher level than it does now, with an intervening period, during which the region north of the lower Thames and Severn was submerged to a depth of 1,200 to 1,500 feet on the flanks of Snowdon. There were three corresponding climatal changes, the first period of elevation being marked by a very low temperature; the second, or that of depression, by temperate conditions; and the third or last period of elevation being also marked by severe climatal conditions. It is obvious that, during changes such as these, the sands, gravels, and clays, termed "glacial drift," would be so extremely complicated and so various in different places, that it is difficult, if not impossible, to ascertain the contemporaneity of the more minute sub-divisions of the glacial strata. While sands and shingle were being accumulated along the coast-line, melting icebergs were dropping their burdens to form boulder-clays in the adjacent sea, and on the land the moraines of the retreating glacier were being heaped together, or the advancing glacier was ploughing its way downwards. All these operations were going on simultaneously in different parts of the glacial area of Great Britain, and their results are rendered infinitely more complex by the oscillations of the level of the land, which may have been local, and by changes of climate as yet imperfectly understood. From these considerations it is evident that the clays and shingles and sands cannot be severally classified together, excepting in the strict *homotaxial* sense, and apart from all ideas of contemporaneity, and that the sequence of the minute divisions of the glacial period in Scotland, published by Dr. James Geikie in his able work, and based upon the relation of clays to sands and gravels, cannot in the nature of things apply to Lancashire, or any other areas beyond Scotland. Mr. De Rance has acted prudently in confining his attention to the drifts of the area treated in the memoir without dealing with the general question, which, to my mind, is not yet sufficiently known to be dealt with in any other fashion than that of the threefold classification of Ramsay, Jamieson, and Lyell. The refinements on this classification attempted by several authors are based upon phenomena which have not yet been proved to be other than local.

One of the more interesting sections of this memoir relates to the growth and accumulation of peat; the author's conclusion that the rate at which peat grows is very uncertain and dependant on local conditions, is confirmed by the recent researches of Dr. Angus Smith, and his observations regarding the manner in which forests have been destroyed by its growth are probably true. The presence, however, of large oaks at great elevations in Britain need not necessarily "point to warmer summers than at present," but may be accounted for by the fact of their having grown in a primeval forest, one under the shelter of another, thus attaining a height and reaching a size which they could not do on our bare hills exposed to the high winds. An example of this may be seen in the fine tall trees growing in the sheltered valley in which Furness Abbey stands, as compared with the stunted growth on the exposed hill-sides around. Nor can the Scotch firs on the peat of the south of England be taken to prove the inclement winters of the prehistoric period, since they now flourish also in the south of England at levels but little above the sea. For the same reason also the peat bogs cannot be looked upon as proving a lower temperature then than now. In Somersetshire the turf moss extending from Glastonbury to Highbridge is growing at the rate of from 4 to 6 feet in fifteen years, so that the places where the peat is cut are filled up in that time. These, however, are unimportant points in a valuable memoir which deals with the district in a very comprehensive manner and in a small space.

It should be remarked, in conclusion, that the price of 17*s.* for a small octavo of 139 pp. in paper wrappers is without precedent and unreasonable, and that the policy of absurdly high prices for Survey Memoirs, which, as it appears from the two last publications, is being pursued by the Stationery Office, is certain to restrict the sale, and thus render them comparatively useless. They cannot be expected to pay their cost any more than the Reports of Parliamentary Commissions; they ought to be issued at a mere nominal sum, and distributed with a liberality like that shown in similar cases by the American Government.

W. BOYD DAWKINS

SCIENTIFIC HORTICULTURE

The Parks and Gardens of Paris. By W. Robinson, F.L.S. Second Edition, Revised. (London: Macmillan and Co., 1878.)

THE Science of Horticulture are words often used and too often misused. That there is science in horticulture, or that it is capable of being based on scientific principles, cannot be denied. There is sufficient in the cultivation of plants and flowers, and in their proper disposition in the garden, to occupy a highly refined and cultivated intellect. A garden, according to its dimensions and capabilities, has always been, and is still, more or less a delight to its owners. Like everything else, the taste exercised in the science of gardening has in different ages shown itself in various ways. The hideous clipping of hedges and shrubs into the forms of animals, birds, &c., still occasionally to be seen in some old English gardens, are records of one of the worst periods of gardening in this country, and the modern system of carpet or ribbon bedding, aptly termed by Mr. Robinson the "coloured cotton handkerchief" style, is not one whit more defensible, but rather, we should say, even more reprehensible, considering what has been done and written of late with the view of elevating public taste in matters of science and art generally.

To no book can we point with so much satisfaction on the subject of laying out or grouping plants or trees in parks or gardens as to that which now lies before us. No writer on this, or on kindred subjects, has discoursed more pleasantly than has Mr. Robinson. That his theme has inspired his pen as that of a ready writer is self-evident; and that he is a true lover of plants for their own sake is also apparent from his frequent references to individual species. But he is something more than this, for it will be found from a perusal of his book that he possesses a thorough knowledge of his subject.

It must not be imagined from the title of the book that the parks and gardens of Paris are, in their entirety, held up for our admiration and imitation. On the contrary the author distinctly points out, and separates the good from the bad, the true from the false, retaining, so to speak, the wheat and consuming the chaff with the fire of a powerful criticism. Notwithstanding this, there can be no two opinions as to the general superiority of the French capital over that of our own in point of picturesque beauty. No visitor to Paris—and no one probably has ever visited that city without visiting also the Bois de Boulogne—can have failed to compare in his own mind the sylvan beauty of the Bois, and the ragged uncared-for appearance of our own London parks; and the contrast is even greater in the squares and gardens of the two capitals, and yet as Mr. Robinson points out, there are excellent sites and splendid opportunities in London to make it a city suitable for other purposes besides those of business and toil. To properly effect this of course architecture and horticulture must join hands. Nevertheless much depends upon a proper provision by the architect for the horticulturist and landscape gardener to exercise their skill. As an illustration of how this may be done, Mr. Robinson draws attention to the new avenue between the new Opera House and the Rue de Rivoli in Paris, and points out that they "have not only been made without cost to the town, but even with a balance on the right side, the vastly increased value of sites for business premises in these new and noble streets having more than repaid the cost of their formation and the removal of the old houses through which they were driven. Abroad, every little capital possessing enough interest to occupy one for two hours, is furbishing up its attractions, while we in London are neglecting advantages the like of which are not possessed by any other city in Europe. The river, the bridges, the suburbs, the surroundings are infinitely superior to Paris, but owing to stupid absence of plan many of the good points are lost, many of the best suburbs being unknown ground even to thousands of Londoners owing to the impossibility of reaching them without struggling through narrow and mean streets and roads."

The first chapter of Mr. Robinson's book opens with a consideration of the Bois de Boulogne, and from it we may learn much that is good both in the way of artistic grouping, planning, and in the selection of individual plants. Unlike what we too often see in public parks and gardens, the vegetation along the banks of the lakes in the Bois is properly diversified, "so that at one place we meet with conifers, at another rock shrubs, at another magnolias, and so on, without the eternal repetition of common things which one too often sees." The author next proceeds to point out the great advantage of permanent planting over that of plants which show only a fleeting annual display. In spring the early bloom and budding leaves are in themselves things of beauty, and are even more so when seen collectively or in company with each other. In summer they furnish an infinite variety of form and consequent depth of tone, while in autumn "the number and richness of the tints of the foliage afford a varied picture from week to week, and in winter the many graceful forms of the deciduous trees among the evergreen shrubs and pines offer as much to interest an observant eye as at any other season."

On the subject of sub-tropical gardening, which is well exemplified in the Park Monceau, the author writes in his pleasantest manner, as the following extract will show:—"We may be pleased by the wide spread of purple on a heath or mountain, but when we go near and examine it in detail we find that its most exquisite aspect is seen in places where the long moss cushions itself beside the Ling, and the fronds of the Polypody peer forth around little masses of heather. Everywhere we see nature judicious in the arrangement of her highest effects, setting them in clouds of verdant leafage, so that monotony is rarely produced—a state of things which it is highly desirable to attain, as far as possible, in the garden. We cannot attempt to reproduce this literally, nor would it be wise or convenient to do so; but assuredly herein will be found the chief source of true beauty and interest in our gardens; and the more we keep this fact before our eyes the nearer will be our approach to truth and success.

"We should compose from nature, as landscape artists do. We may have in our gardens—and without making wildernesses of them—all the shade, the relief, the grace, the beauty, and nearly all the irregularity of nature. This bold growth of 'fine-foliaged plants' has shown us that one of the greatest mistakes ever made in the garden was the adoption of a few varieties of plants for culture on a vast scale, to the exclusion of interest and variety, and too often of beauty or taste. We have seen how well the pointed, tapering leaves of the cannas carry the eye upwards; how refreshing it is to cool the eyes in the deep green of those thoroughly tropical castor-oil plants, with their gigantic leaves, how noble the Wigandia, with its fine texture and massive outline looks, after we have surveyed brilliant lines and richly painted leaves; how, too, the bold tropical palm leaves beautify the garden. In a word, the system has shown us the difference between the gardening that interests and delights all beholders, and not the horticulturist only, and that which is too often offensive to the eye of taste and pernicious to every true interest of what has been called 'the purest of human pleasures.'"

Notwithstanding the general interest kept up throughout Mr. Robinson's book, no part is of greater interest than the chapter on the cultivation of mushrooms in the caves under Paris, where, at a depth of from sixty to eighty feet below the surface of the ground, in old stone quarries, this edible fungus is grown systematically on a very large scale. These caves furnish not only the daily supply of the Paris markets, but to a large extent those of England and other countries also, preserved mushrooms to the extent of 14,000 boxes annually being exported to this country by one house alone. It is estimated that in and around Paris the daily production of mushrooms amounts to about twenty-five tons, of the value of about 1,000*l.*, or close upon 400,000*l.* per annum. One large grower near St. Denis is described as employing nineteen horses and fifty men. Mushrooms are very extensively used in France, not only in their fresh state, but preserved in various ways, either by tinning, bottling, preserving in butter or oil, or reducing to powder.

The book is exceedingly well printed and carefully got up.

OUR BOOK SHELF

Elements of Physiography. By Prof. D. T. Ansted, M.A., F.R.S. (Allen and Co.)

WE are glad to see a book on physiography from the pen of Prof. Ansted, whose name has so long been associated with the literature of physical geography. If the new subject, however, is to be treated as it is in the present work, we fail to see the advantage of any change. The distinctive parts of physiography are all got through in an introduction of some eighteen pages, which we fear are too much written up to the *ipsissima verba* of the syllabus issued by the Science and Art Department to be of much educational value.

And, in fact, it is on this point that we find most fault with the book. Take these two paragraphs concerning aqueous vapour, pp. 101, 102, and note the absence of the why in every case.

"Aqueous vapour is frequently rendered visible as mist, fog, or cloud. These are varieties of the same condition. Mist is formed when currents of air of different temperature, both containing invisible vapour, meet near the earth. In valleys such mixtures are very frequent, and in mountainous countries very striking. Fogs are formed in the same way in temperate climates at various seasons, and hang over shores and the mouths of certain streams.

"Clouds are produced when mixtures of currents take place at some distance above the ground, and the visible vapour is entirely detached from the earth. There are several varieties of clouds, some floating at a height of many miles, some hovering in large masses in mid-air, some drooping downwards and almost touching the earth. They are rarely alike long together. They assume, as we know, the most fantastic shapes, and are occasionally decorated with brilliant tints of colour! It is only the clouds that form in large masses and approach the earth that dissolve into heavy and long-continued rain, but all clouds are capable of yielding rain, and drops sometimes, though rarely, fall through air perfectly clear and cloudless."

A student had better not be taught at all than be taught in this manner, and, in fact, a student of average intelligence, after reading such a string of assertions without the least attempt at explanation—while everyone of them might have been made part of his nature for ever afterwards, by giving the simple reason in each case—if he does not doubt the competence of his teacher, will have no more to do with him.

La Spectroscopie. By A. Cazin. (Paris: Gauthièr-Villars.)

THE talented author of this work has passed away since the MS. was completed. This is by no means a systematic treatise, but it contains a large amount of information—some of it out-of-the-way information—and it will repay perusal. As much of M. Cazin's information on the celestial applications has been gathered from Secchi's works its complete accuracy is not to be relied on, but the explanation given of the different methods employed is very clear.

The part of the book which perhaps will be read with the greatest interest is that dealing with radiation and absorption spectra. In this part the author includes a notice of much of his own work, which is of great interest and importance. The *historique* of the question as to the existence of double or multiple spectra is interesting, and the author's leaning is against the view held by Ångström and Thalèn. He gives special observations of his own concerning nitrogen, and indeed was engaged on an extension of them at the time of his lamented death.

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.*]

Sun-spots and Rainfall

IF the sun-spot cycles were all of the same length the simplest way of comparing the yearly sun-spot areas with the yearly amounts of rainfall in order to see whether the two phenomena were more or less numerically related, would be to find the annual means of the greatest possible number of cycles, care being taken to place the years of maximum and minimum in the same two groups respectively, and the intervening years in due succession. But as the sun-spot cycles are not of the same length, we must in employing the method of arithmetical means, make some modifications calculated to suit the circumstances of the case.

1. We may, for example, take any cycle whatever may be its duration, and commencing with its first and ending with its last year, compare with the sun-spots the rainfall at any station, or the means of the rainfall at a number of stations for the same years.

2. If some of the cycles be of the same length, we may take these alone and compare them with the rainfall, still taking care that the years of maximum and minimum sun-spots shall be respectively in the same groups.

3. The average length of the sun-spot cycle being, as far as is yet known, about eleven years, we may take any number of cycles of different lengths and make two separate comparisons, in one of which the maximum years are to be placed in the same group, and in the other the minimum years in the same group, the number of the other groups preceding and following the epochal groups being determined by the fact that the mean interval from minimum to maximum is about 3·7 years, and from maximum to minimum about 7·4 means.

I have tried these and several other methods, with, I think, considerable success. The method just mentioned (3), which is the old one of arithmetical means somewhat modified to meet the conditions of the case, possesses several advantages, one of the most important of which is that it enables us to compare directly the rainfall with the sun-spots in the epochal years, and in at least two years before and two years after them, thus affording a fair comparison for nearly the whole cycle of eleven years—a most essential point, which, it would appear, has been overlooked by some writers on the subject.

Having in my last communication (NATURE, vol. xvii. p. 448) given an example of the first half of the above method, together with the results obtained by it for several localities, I intended on this occasion to submit only one or two examples of the second half. But finding that the method has been criticised by Mr. Buchan, and having now Prof. Wolf's latest edition of his relative sun-spot numbers, as well as the rainfall of Madras for 1877, it may be proper to give instances of the application of the whole process.

Let us begin with Wolf's relative numbers, which are so arranged in the following table that those for the years of maximum sun-spot, from 1811 to 1877, are all in the sixth line. This table has already been given by Mr. Buchan (NATURE, vol. xvii. p. 506), but as references will be made to it now, and also in future discussions of the rainfalls of various parts of the globe, it is desirable to reproduce it here.

TABLE I.—*Wolf's Sun-spot Numbers (Maximum Years in Sixth Line).*

Year.	1811–1823.	1824–1836.	1832–1844.	1843–1855.	1855–1867.	1865–1877.	Means.	Mean Cycle.	Variation.	Year of Cycle.
1	1·6	8·1	26·3	13·1	7·7	31·4	14·7	—	—	—
2	4·9	16·2	9·4	19·3	5·1	14·7	11·6	14·9	− 33·9	1
3	12·6	35·0	13·3	38·3	22·9	8·8	21·8	25·4	− 23·4	2
4	16·2	51·2	59·0	59·6	56·2	36·8	46·5	48·8	0·0	3
5	35·2	62·1	119·3	97·4	90·3	78·6	80·5	77·0	+ 28·2	4
6	**46·9**	**67·2**	**136·9**	**124·9**	**94·8**	**131·8**	**100·4**	**91·9**	**+ 43·1**	**5**
7	39·9	67·0	104·1	95·4	77·7	113·8	83·0	83·0	+ 34·2	6
8	29·7	59·4	83·4	69·8	61·0	99·7	65·7	65·6	+ 16·8	7
9	23·5	26·3	61·8	63·2	45·4	67·7	48·0	49·0	+ 0·2	8
10	16·2	9·4	38·5	52·7	45·2	43·1	34·2	34·6	− 14·2	9
11	6·1	13·3	23·0	38·5	31·4	18·9	21·9	24·6	− 24·2	10
12	3·9	59·0	13·1	21·0	14·7	11·3	20·5	22·5	− 26·3	11
13	2·6	119·3	19·3	7·7	8·8	7·0	27·5	—	—	—

It will be seen that each of six of the columns in the above table gives the sun-spot numbers for thirteen years, and that the first term of what is called the "mean cycle" is obtained from the expression, $\frac{a + 2b + c}{4}$, where a, b, c, are the means of the sun-spots for the first, second, and third years of the thirteen years, the remaining terms being obtained in a similar manner. The "variation" is simply the deviations from the mean value of the sun-spots for the "mean cycle."

The next table gives the sun-spot numbers, from 1816 to 1872, arranged so that the minimum years are in the eighth line:—

TABLE II.—*Wolf's Sun-spot Numbers (Minimum Years in Eighth Line).*

Year.	1816–1828.	1826–1838.	1836–1848.	1849–1861.	1860–1872.	Means	Mean Cycle.	Variation.	Year of Cycle.
1	46·9	35·0	119·3	95·4	94·8	78·3	—	—	—
2	39·9	51·2	136·9	69·8	77·7	75·1	73·1	+ 23·3	1
3	29·7	62·1	104·1	63·2	61·0	64·0	64·3	+ 14·5	2
4	23·5	67·2	83·4	52·7	45·4	54·4	54·6	+ 4·8	3
5	16·2	67·0	61·8	38·5	45·2	45·7	44·2	− 5·6	4
6	6·1	59·4	38·5	21·0	31·4	31·3	30·8	− 19·0	5
7	3·9	26·3	23·0	7·7	14·7	15·1	17·3	− 32·5	6
8	**2·6**	**9·4**	**13·1**	**5·1**	**8·8**	**7·8**	**12·7**	**− 37·1**	**7**
9	8·1	13·3	19·3	22·9	36·8	20·1	24·4	− 25·4	8
10	16·2	59·0	38·3	56·2	78·6	49·7	51·6	+ 1·8	9
11	35·0	119·3	59·6	90·3	131·8	87·2	80·7	+ 30·9	10
12	51·2	136·9	97·4	94·8	113·8	98·8	94·6	+ 44·8	11
13	62·1	104·1	124·9	77·7	99·7	93·7	—	—	—

We can now compare the rainfall of Madras with the values of the sun-spots in Tables I. and II., except for the years 1811 and 1812, for which there are no observations:—

TABLE III.—*Rainfall of Madras* (*Maximum Years in Sixth Line*).

Year.	1811–1823.	1824–1836.	1832–1844.	1843–1855.	1855–1867.	1865–1877.	Means.	Mean Cycle.	Variation.	Year of Cycle.
	in.	in.	in.	in.	in.	in.	in.	in.	in.	
1	—	33·7	18·4	*50·3	32·3	41·6	35·3	—	—	—
2	—	56·0	*37·1	65·4	*47·0	51·4	51·4	45·3	− 3·2	1
3	45·1	60·7	39·0	38·1	52·9	*24·4	43·4	48·3	− 0·2	2
4	32·4	88·4	41·5	79·8	48·5	41·4	55·3	51·3	+ 2·8	3
5	56·0	37·9	44·8	81·0	55·1	32·3	51·2	51·2	+ 2·7	4
6	**41·2**	**36·9**	**49·3**	**54·8**	**27·6**	**74·1**	**47·3**	**48·1**	**− 0·4**	**5**
7	63·6	32·4	52·3	39·8	37·2	56·3	46·9	48·7	+ 0·2	6
8	76·2	44·3	53·1	36·9	38·2	73·7	53·7	50·4	+ 1·9	7
9	36·3	18·4	58·6	64·3	54·6	51·8	47·3	51·5	+ 3·0	8
10	70·0	*37·1	58·3	72·7	47·2	62·9	58·0	50·7	+ 2·2	9
11	47·1	39·0	36·5	35·8	41·6	37·1	39·5	45·4	− 3·1	10
12	59·6	41·5	*50·3	43·2	51·4	21·5	44·6	42·1	− 6·4	11
13	*26·6	44·8	65·4	32·3	*24·4	45·0	39·7	—	—	—

The "mean cycle" in the above table has been formed (in the way already mentioned) with the view of reducing the effects of what are called "accidental" irregularities in the rainfall, and its mean value is 48·5 inches, while the mean of the thirteen "means" is 47·3 inches. Now it will be observed that, as was the case when only the years 1813 to 1867 were taken (NATURE, vol. xvii. p. 449), there is apparently a double oscillation of the Madras rainfall during the sun-spot cycle. It will be seen also—and it is important to bear this point in mind—that the mean rainfall of the seven maximum years not only does *not* exceed, but barely reaches, the mean for the whole cycle. There are apparently two maxima and two minima, and one of the minima seems to occur very soon after the sun-spot maximum. In fact, there was a great deficiency of rainfall at Madras in the maximum year 1860, and in the years 1830, 1861, and 1869, immediately following or preceding a maximum year, but whether there were famines in one or more of these years, I do not know. (The years of minimum sun-spots are marked with an asterisk.)

Coming now to the second half of the method, so far as Madras is concerned, we get the following results:—

TABLE IV.—*Rainfall of Madras* (*Minimum Years in Eighth Line*).

Year.	1816–1828.	1826–1838.	1836–1848.	1849–1861.	1860–1872.	Means	Mean Cycle.	Variation.	Year of Cycle.
	in.	in.	in.	in.	in.	in.	in.	in.	
1	*41·2	60·7	44·7	39·8	*27·6	42·8	—	—	—
2	63·6	88·4	*49·3	36·9	37·2	55·1	51·7	+ 2·7	1
3	76·2	37·9	52·3	64·3	38·2	53·8	52·3	+ 3·3	2
4	36·3	*36·9	53·1	72·7	54·6	50·7	51·0	+ 2·0	3
5	70·0	32·4	58·6	35·8	47·2	48·8	48·8	− 0·2	4
6	47·1	44·3	58·3	43·2	41·6	46·9	45·5	− 3·5	5
7	59·6	18·4	36·5	32·3	51·4	39·6	40·8	− 8·2	6
8	**26·6**	**37·1**	**50·3**	**47·0**	**24·4**	**37·1**	**40·0**	**− 9·0**	**7**
9	33·7	39·0	65·4	52·9	41·4	46·5	43·3	− 5·7	8
10	56·0	41·5	38·0	48·5	32·3	43·4	49·0	0·0	9
11	60·7	44·8	79·8	55·1	*74·1	62·9	57·4	+ 8·4	10
12	88·4	*49·3	81·0	*27·6	56·3	60·5	58·8	+ 9·8	11
13	37·9	52·3	*54·8	37·2	73·7	51·2	—	—	—

The above table shows that the minimum rainfall, both for the "means" and the "mean cycle," coincides with the minimum of sun-spots (see Table II.), and that, upon the whole, the spots and the rain decrease and increase together. But as the maximum years (with an asterisk) are not all in the same line, nor the minimum years in Table III., all in the same line, it is necessary to confine our attention to the results for about two years on either side of the epochal years in Tables III. and IV., and in doing so we find evidence of a double oscillation.

To ascertain whether there is some probability of such an oscillation in the rainfall of Madras, we must have recourse to the more efficient method of the harmonic analysis. I have not had leisure to do so in this particular case, but Mr. J. Allan Broun (NATURE, vol. xvi. p. 334), in a thorough examination of the rainfalls of Madras and Trevandrum for the years 1838-76, gives for the mean oscillations of the Madras rainfall during that period the following equation, where y is the mean yearly rainfall in inches:—

$$y = 5·4 \sin(\theta + 50°) + 4·6 \sin(2\theta + 252°);$$

and he remarks that these angles give the epochs of *minimum* rainfall both in the years of minimum and of maximum sun-spots, and that the single oscillation (of about five years) has held good in seven successive periods. Now this is nearly what we should expect from Tables III. and IV.

Leaving the Madras rainfall for the present, let us come to that of Edinburgh for the years 1824 to 1872. The following table gives a comparison of the Edinburgh rainfall with the sun-spots from 1824 to 1867:—

TABLE V.—*Rainfall of Edinburgh* (*Maximum Years in Sixth Line*).

Year.	1824–1836.	1832–1844.	1843–1855.	1855–1867.	Means	Mean Cycle.	Rain Var.	Spot Var.	Year of Cycle.
	in.	in.	in.	in.	in.	in.	in.		
1	24·8	23·2	*23·8	20·3	23·0	—	—	—	—
2	22·1	*20·9	20·9	*28·5	23·1	22·8	− 2·8	− 37·2	1
3	15·3	21·0	26·6	24·9	22·0	23·8	− 1·8	− 22·8	2
4	32·6	25·2	31·5	24·3	28·4	26·3	+ 0·7	+ 4·4	3
5	25·2	33·0	22·8	25·9	26·7	28·0	+ 2·4	+ 33·0	4
6	**30·0**	**26·8**	**30·6**	**33·4**	**30·2**	**28·9**	**+ 3·3**	**+ 43·8**	**5**
7	33·2	31·0	22·2	28·6	28·8	28·4	+ 2·8	+ 32·9	6
8	24·5	23·4	21·3	33·9	25·8	26·1	+ 0·5	+ 14·3	7
9	23·2	25·5	22·8	25·6	24·3	25·2	− 0·4	− 2·9	8
10	*20·9	26·2	31·5	28·1	26·7	24·6	− 1·0	− 16·6	9
11	21·0	16·9	[1]21·8	23·6	20·8	23·1	− 2·5	− 24·7	10
12	25·2	*23·8	20·9	27·2	24·3	23·9	− 1·7	− 24·0	11
13	33·0	20·9	20·3	*31·0	26·3	—	—	—	—

An inspection of the above table will show that there is a remarkable coincidence between the rainfall and sun-spot variations—much more remarkable than at Madras. The years of maximum and minimum rainfall and sun-spot for the mean cycles coincide, and, on the whole, there is a regular gradation from minimum to maximum and from maximum to the next minimum.

The next table (the second half of the process for Edinburgh) gives almost equally remarkable results.

TABLE VI.—*Rainfall of Edinburgh* (*Minimum Years in Eighth Line*).

Year.	1826–1838.	1836–1848.	1849–1861.	1860–1872.	Means	Mean Cycle.	Rain Var.	Spot Var.	Year of Cycle.
	in.	in.	in.	in.	in.	in.	in.		
1	15·3	33·0	22·2	*33·4	26·0	—	—	—	—
2	32·6	*26·8	21·3	28·6	27·3	27·2	+ 1·2	+ 24·7	1
3	25·2	31·0	22·8	33·9	28·2	27·8	+ 1·8	+ 15·9	2
4	*30·0	23·4	31·5	25·6	27·6	27·6	+ 1·6	+ 5·6	3
5	33·2	25·5	21·8	28·1	27·2	26·4	+ 0·4	− 5·4	4
6	24·5	26·2	20·9	23·6	23·8	24·1	− 1·9	− 20·4	5
7	23·2	16·9	20·3	27·2	21·9	23·4	− 2·6	− 36·3	6
8	**20·9**	**23·8**	**28·5**	**31·0**	**26·0**	**24·4**	**− 1·6**	**− 42·1**	**7**
9	21·0	20·9	24·9	28·6	23·8	24·6	− 1·4	− 28·6	8
10	25·2	26·6	24·3	22·2	24·6	25·2	− 0·8	+ 2·9	9
11	33·0	31·5	25·9	*22·1	28·1	26·8	+ 0·8	+ 35·3	10
12	*26·1	22·8	*33·4	23·2	26·4	28·2	+ 2·2	+ 48·9	11
13	31·0	*30·6	28·6	38·2	32·1	—	—	—	—

We find from the preceding table that the year of minimum rainfall was, on an average, the year immediately before the year of minimum sun-spot, and that the year of maximum sun-spot coincided with the year of maximum rainfall. Another coincidence is that the ratio of the rainfall to the sun-spots in the eleventh year of the "mean cycle" is nearly the same as the corresponding ratio in the first year of the cycle. Whether these relations are constant is another question; in a case of

[1] Interpolated.

this kind we can scarcely venture to go beyond actual experience.

It would be easy to multiply similar examples and results of the method which I ventured to submit in my former paper to NATURE—a method the main object of which was to refer, as nearly as possible (without using the more laborious method of the harmonic analysis), the rainfalls of remote localities to the epochs of maximum and minimum sun-spots. I will, however, for the present only give, further, the results that have been obtained for the Paris rainfall from 1824 to 1867, and from 1816 to 1870.

TABLE VII.—*Rainfall of Paris* (*Maximum Years in Sixth Line*).

Year.	1824–1836.	1832–1844.	1843–1855.	1855–1867.	Means	Mean Cycle.	Rain Var.	Spot Var.	Year of Cycle.
	m.	m.	m.	m.	m	m.	m.		
1	572	456	*542	344	478	—	—	—	—
2	469	*503	571	*565	527	502	– 11	– 37·2	1
3	410	421	581	492	476	493	– 20	– 22·8	2
4	501	438	564	466	492	501	– 12	+ 4·4	3
5	585	611	430	545	543	541	+ 28	+ 33·0	4
6	**560**	**547**	**575**	**655**	**584**	**563**	**+ 50**	**+ 43·8**	**5**
7	573	542	597	458	543	554	+ 41	+ 32·9	6
8	529	580	563	516	547	522	+ 9	+ 14·3	7
9	456	455	469	426	452	487	– 26	– 2·9	8
10	*503	527	597	366	498	472	– 41	– 16·6	9
11	421	342	454	542	440	484	– 29	– 24·7	10
12	438	*542	614	644	559	520	+ 7	– 24·0	11
13	611	571	344	*565	523	—	—	—	—

The above table shows that, whether we take the "means" or the "mean cycle," the rainfall was greatest in the years of maximum sun-spot; that it was least in the ninth year of the "mean cycle;" and that, on the whole, the rainfall and sun-spots, notwithstanding some discrepancies, increased and decreased together.

The next table, in which the arrangement is inverted, gives similar results for Paris.

TABLE VIII.—*Rainfall of Paris* (*Minimum Years in Eighth Line*).

Year.	1816–1828.	1826–1838.	1836–1848.	1849–1861.	1860–1872.	Means	Mean Cycle.	Rain Var.	Spot Var.	Year of Cycle.
	m.	m.	m.	m.	m.	m.	m.	m.		
1	*546	410	611	597	*655	564	—	—	—	—
2	565	501	*547	563	458	527	531	+ 20	+ 23·3	1
3	432	585	542	469	516	509	525	+ 14	+ 14·5	2
4	615	*560	580	597	426	556	516	+ 5	+ 4·8	3
5	378	573	455	454	366	445	501	– 10	– 5·6	4
6	584	529	527	614	542	559	501	– 10	– 19·0	5
7	424	456	342	344	644	442	492	– 19	– 32·5	6
8	**457**	**503**	**542**	**565**	**565**	**526**	**502**	**– 9**	**– 37·1**	**7**
9	572	421	571	492	512	514	510	– 1	– 25·4	8
10	469	438	581	466	477	486	499	– 2	+ 1·8	9
11	410	611	564	545	*418	510	510	– 1	+ 30·9	10
12	501	*547	430	*655	—	533	529	+ 17	+ 44·8	11
13	585	542	*575	458	—	540	—	—	—	—

We see that the minimum rainfall occurred, on an average, in the year immediately preceding the year of minimum sun-spots, as at Edinburgh, but that the variation was not so regular.

As formerly remarked, the rainfalls of Edinburgh and Paris—especially that of Edinburgh—are more favourable to the theory than the rainfall of Madras.

Mr. Buchan considers the method which has now been sketched a new one, and, "as such, deserving of a careful examination as to how far it is applicable to the data submitted for discussion." This examination consists almost wholly in showing that by placing the maximum years in the same line or group the minimum years are spread over six out of thirteen groups, and that by placing the minimum years in the same group the maximum are also spread over six groups. Hence he concludes that this double arrangement is inferior to a single one in which the maximum and minimum years together are "compactly" spread over six out of eleven groups. But it seems to me that he has in great measure lost sight of what should be a main object of comparisons of sun-spots and rainfall, namely, the closest possible reference of the rainfall to the *epochs* of maximum and minimum sun-spots, and that however compact the arrangement he recommends may be considered, it is fundamentally objectionable. By placing the maximum and minimum years respectively in the *same* groups there is certainly a much greater chance of finding any connection that may exist between the two phenomena than by spreading them over six groups out of eleven.

How far the method defended by Mr. Buchan is applicable to the data will appear from the following table of the Madras rainfall, in which Dr. Hunter's arrangement is adopted. The maxima and minima years are marked with an asterisk.

TABLE IX.—*Rainfall of Madras* (*Maximum and Minimum Years in Six Groups*).

Year.	1811–1821.	1822–1832.	1833–1843.	1844–1854.	1855–1865.	1866–1876.	Means	Rain Var.	Spot Var.
	in.	in.	in.	in.	in.	in.	in.		
1	—	59·6	*37·1	65·4	32·3	51·4	49·2	+ 0·9	– 37·6
2	—	*26·6	39·0	38·1	*47·0	*24·4	35·0	– 13·3	– 35·0
3	45·1	33·7	41·5	79·8	52·9	41·4	49·1	+ 0·8	– 14·2
4	32·4	56·0	44·8	81·0	48·5	32·3	49·2	+ 0·9	+ 16·6
5	56·0	60·7	49·3	*54·8	55·1	*74·1	58·3	+ 10·0	+ 45·0
6	*41·2	88·4	52·3	39·8	*27·6	56·3	50·9	+ 2·6	+ 37·0
7	63·6	37·9	53·1	36·9	37·2	73·7	50·4	+ 2·1	+ 24·5
8	76·2	*36·9	58·6	64·3	38·2	51·8	54·3	+ 6·0	+ 11·0
9	36·3	32·4	58·3	72·7	54·6	62·9	52·9	+ 4·6	– 2·4
10	70·0	44·3	36·5	35·8	47·2	37·1	45·1	– 3·2	– 15·4
11	47·1	18·4	*50·3	43·2	41·6	21·5	37·0	– 11·3	– 29·2

The mean rainfall for the cycle is 48·3 inches. Now the mean rainfall for the fifth group is 58·3 inches, and the mean value of the sun-spots for the same group 45·0, which is the *maximum*. It is thus made to appear that, on an average, the maximum rainfall of Madras coincides with the maximum of sun-spots. But this is contrary to fact. We know, as a matter of observation (see Table III.), that the mean rainfall of Madras in the maximum years was not above the average, and yet the arrangement recommended by Mr. Buchan makes it *ten* inches *above* the average.

Applying the same arrangement to the rainfall of Edinburgh, we get the following results.

TABLE X.—*Rainfall of Edinburgh* (*Maximum and Minimum Years in Six Groups*).

Year.	1822–1832.	1833–1843.	1844–1854.	1855–1865.	Mean Rain.	Mean Spots.	Rain Var.	Spot Var.
	in.	in.	in.	in.	in.		in.	
1	26·1	*20·9	20·9	20·3	22·1	8·1	– 3·6	– 43·2
2	*30·3	21·0	26·6	*28·5	26·6	14·8	+ 0·9	– 36·5
3	24·8	25·2	31·5	24·9	26·6	37·4	+ 0·9	– 13·9
4	22·1	33·0	22·8	24·3	25·6	72·3	– 0·1	+ 21·0
5	15·3	*26·1	*30·6	25·9	24·5	92·4	– 1·2	+ 41·1
6	32·6	31·0	22·2	*33·4	29·8	86·4	+ 4·1	+ 35·1
7	25·2	23·4	21·3	28·6	24·6	73·2	– 1·1	+ 21·9
8	*30·0	25·5	22·8	33·9	28·0	63·3	+ 2·3	+ 12·0
9	33·2	26·2	31·5	25·6	29·1	50·9	+ 3·4	– 0·4
10	24·5	16·9	21·8	28·1	22·8	41·5	– 2·9	– 9·8
11	23·2	*23·8	20·9	23·6	22·9	23·9	– 2·8	– 27·4

In the above table the sun-spot maximum occurs in the 5th group, and the rainfall is made to be 1·2 inch below the mean; that is, according to this arrangement the people of Edinburgh are supposed to get *less* than their average allowance of rain in the maximum year. But according to the observations published by the Scottish Meteorological Society, that again is contrary to fact, for (see Table V.) the rainfall of Edinburgh is not 1·2 inch *below* the mean for the maximum years, but, taking the thirteen "means" five inches *above* it, and, taking the "mean cycle" 3 inches above it.

Which of the two methods, then, is the more applicable to the data for discussion?

Of all the methods, that of the harmonic analysis is doubtless the best. It enables us to see whether there is any parallelism, and if there is a cycle, what is its probable length with respect to the sun-spot cycle, the range of variation, the times of maximum and minimum, with their intervals, &c. I have applied this method to yearly values of the rainfalls, and of the levels of rivers of various countries, and have come to the conclusion that, notwithstanding all apparent irregularities, there is an intimate connection between sun-spots and rainfall.

If the rainfall generally was above its mean in the years of maximum sun-spot, and below it in the years of minimum sun-spot, we should get for the mean yearly rainfall of a number of stations the equation $\frac{S - s}{s' - S} = \frac{R - r}{r' - R}$, where S is the mean value of the sun-spots for the period examined, s the mean value of the spots when below S, s' their mean value when above S, and R, r, r' the corresponding values for the rain for the years from which S, s, and s' were obtained. This formula applied to the public observations of different countries shows that with very few exceptions the rainfall for the periods examined were above the average. The results for the mean rainfall of fifty-four stations in Great Britain, and thirty-four in America from 1824 to 1867 are as follows:—

Great Britain ... ... $\begin{cases} \frac{S - s}{s' - S} = \frac{24{\cdot}9}{29{\cdot}8} = {\cdot}8356 \\ \frac{R - r}{r' - R} = \frac{+0{\cdot}75}{+0{\cdot}90} = {\cdot}8333 \end{cases}$

America ... ... $\begin{cases} \frac{S - s}{s' - S} = \frac{24{\cdot}9}{29{\cdot}8} = {\cdot}8356 \\ \frac{R - r}{r' - R} = \frac{+0{\cdot}94}{+1{\cdot}13} = {\cdot}8407 \end{cases}$

In other words, the rainfall of fifty-four stations in Britain from 1824 to 1867 was 0·75 inch below the mean when the sun-spots were below their mean and 0·90 inch above it when the spots were in excess, and the corresponding values for America were 0·94 and 1·13 inch.

C. MELDRUM

Sun-spots and Weather

IN NATURE, vol. xvii. p. 326, Dr. Balfour Stewart concludes an article with the following remark:—

"It is nearly, if not absolutely, impossible from observations already made, to tell whether the sun be hotter or colder as a whole when there are most spots on his surface. The sooner we get to know this the better for our problem."

The Bombay barometric observations appear to me to afford fairly conclusive evidence in favour of the sun being hottest about the time of maximum spotted area, and coldest when the spotted area is at its minimum.

It is well known that in Central Asia the annual variation of the barometric pressure is greater than in any other portion of the globe, and it is universally admitted that this variation is due to the great variation of temperature between summer and winter, the barometer being low when the temperature is high, and *vice versâ*. If, therefore, the absolute heat of the sun is subject to considerable variations, we ought to find the barometric pressure in Central Asia responding to those variations just as it does to the annual variations of temperature; in other words, the summer barometric minimum should be lowest in those years when the sun is hottest, and the winter maximum should be highest in those years when the sun is coldest.

Similar results should be obtainable from the barometric records of any station where the annual variation of pressure is considerable and of the same character as in Central Asia. Bombay is such a station, and one where cyclonic disturbances are less frequent and violent than at most other Indian coast stations. I give below the mean barometric pressure at Bombay for the summer and winter half-years from 1847 to 1877:—

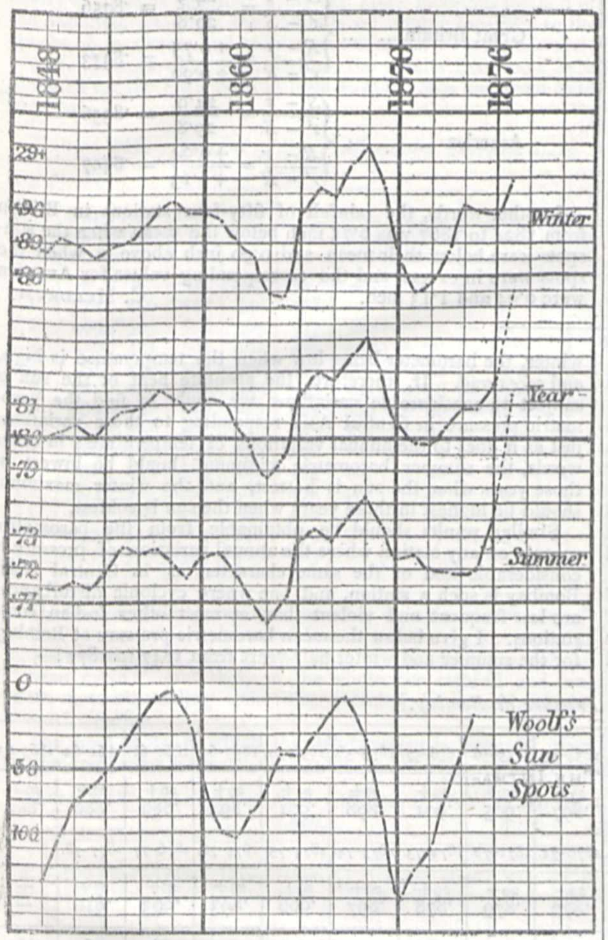
Mean Barometric Pressure at Bombay.

October to March.	1847–48.	1848–49.	1849–50.	1850–51.	1851–52.	1852–53.	1853–54.	1854–55.	1855–56.	1856–57.	1857–58.	1858–59.	1859–60.	1860–61.	1861–62.	1862–63.
	29+ ·884	29+ ·888	29+ ·894	29+ ·886	29+ ·888	29+ ·891	29+ ·891	29+ ·897	29+ ·905	29+ ·901	29+ ·898	29+ ·901	29+ ·894	29+ ·886	29+ ·886	29+ ·862
	1863–64.	1864–65.	1865–66.	1866–67.	1867–68.	1868–69.	1869–70.	1870–71.	1871–72.	1872–73.	1873–74.	1874–75.	1875–76.	1876–77.	1877–78.	—
	29+ ·885	29+ ·912	29+ ·902	29+ ·906	29+ ·925	29+ ·913	29+ ·903	29+ ·872	29+ ·879	29+ ·878	29+ ·897	29+ ·906	29+ ·893	29+ ·903	29+ ·916	—
April to September.	1847.	1848.	1849.	1850.	1851.	1852.	1853.	1854.	1855.	1856.	1857.	1858.	1859.	1860.	1861.	1862.
	29+ ·707	29+ ·722	29+ ·703	29+ ·730	29+ ·704	29+ ·719	29+ ·737	29+ ·712	29+ ·743	29+ ·712	29+ ·718	29+ ·723	29+ ·729	29+ ·722	29+ ·707	29+ ·705
	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	1876.	1877.	—
	29+ ·698	29+ ·751	29+ ·720	29+ ·738	29+ ·722	29+ ·760	29+ ·726	29+ ·721	29+ ·731	29+ ·713	29+ ·725	29+ ·713	29+ ·722	29+ ·723	29+ ·773	—

The winter means correspond in time to the beginning of each year, the summer means to the middle of each year. Taking the mean of each pair of winter means, we obtain a new set of numbers which correspond to the middle of each year, and which give a somewhat smoother curve than the original numbers, and performing a similar operation twice upon the summer means, we obtain a similarly smoothed set of numbers also corresponding to the middle of each year. These two sets of smoothed numbers, and their means, are given below, and graphically represented by the accompanying curves, along with the inverted sun-spot curve.

—	1848.	1849.	1850.	1851.	1852.	1853.	1854.	1855.	1856.	1857.	1858.	1859.	1860.	1861.	1862.
Winter	29+ ·886	29+ ·891	29+ ·890	29+ ·887	29+ ·889	29+ ·891	29+ ·894	29+ ·901	29+ ·903	29+ ·899	29+ ·899	29+ ·897	29+ ·890	29+ ·886	29+ ·874
Summer	·713	·714	·716	·714	·719	·726	·725	·727	·721	·717	·723	·725	·719	·710	·703
Year	·799	·802	·803	·800	·804	·808	·809	·814	·812	·808	·811	·811	·804	·798	·788
—	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	1876.	1877.
Winter	29+ ·873	29+ ·898	29+ ·907	29+ ·904	29+ ·915	29+ ·919	29+ ·908	29+ ·887	29+ ·875	29+ ·878	29+ ·887	29+ ·901	29+ ·899	29+ ·898	29+ ·909
Summer	·712	·729	·732	·729	·735	·742	·733	·724	·724	·720	·719	·718	·719	·735	[·773]
Year	·792	·813	·819	·816	·825	·830	·820	·805	·799	·799	·803	·809	·809	·816	[·845]

It will be seen that there is a remarkable degree of resemblance in the progression of these phenomena from year to year, but that the barometric curve "lags behind" the sun-spot curve, particularly in the years of maximum sun-spots.



The winter curve is more regular than the summer one, probably because the weather generally in India is more settled in the winter than in the summer, but on the whole the two curves support each other in showing a low pressure about the time of sun-spot maximum, and a high pressure at the time of sun-spot minimum. We may therefore conclude that the sun is hottest about the time when the spots are at a maximum, and coldest when they are at a minimum.

The range of the variation of the year by mean pressure from the minimum of 1862 to the maximum of 1868, is ·042 of an inch, and the mean range of the barometer from January to July is ·291, from which it appears that the variations of pressure produced by absolute variations of the sun's heat are, in comparison with the usual seasonal changes, by no means insignificant.

These results appear to harmonise well with the decennial variations of the rainfall in India, and to throw light upon the inverse variation (compared with the sun-spots) of the winter rainfall of Northern India. Mr. Archibald has attempted to explain this latter phenomenon on the assumption that the sun is coldest when it is most spotted, but the inverse winter variation of the rainfall of Northern India, as well as the direct variation at Madras, Bombay, Trevandrum, and elsewhere, appear to me to follow more naturally from the contrary view; for if the winter rainfall in Northern India is really due to the cold of winter we should expect it to be greatest when the sun is coldest, just as the summer rainfall is expected to be greatest when the sun is hottest. FRED. CHAMBERS

Bombay, August 23

The Norwegian Arctic Expedition

THE *Vöringen*, Capt. Wille, returned to Christiania on the 10th, from Spitzbergen, after a most successful cruise. No less than 375 stations have been thus explored by sounding, dredging, and trawling during the last three summers; and the *Morgenbladet* reminds us that only 354 of such stations were recorded in the notice of the *Challenger* expedition. The number of stations in the *Porcupine* cruises of 1869 and 1870 was 148.

Prof. G. O. Sars informs me that in every department of zoology a vast amount of material was procured in his last cruise, and that especially the Mollusca are abundantly represented, not only by magnificent specimens of rare Arctic species, such as *Fusus kroyeri* and *F.* (*Neptunea*) *deformis*, but also by several interesting new forms. He adds that the conchological collection from this cruise is indeed much richer than both of those made in his former two cruises. Herr Friele will work out all the Mollusca.

I cannot omit expressing my admiration of the recent work of Prof. G. O. Sars, entitled "Mollusca Regionis Arcticæ Norvegiæ." His descriptions are excellent, and his figures (all by his own hand, and autographed) are inimitable. The work contains 466 pages and 52 plates, besides a chart.

Ware Priory, Herts, September 23 J. GWYN JEFFREYS

Albinism in Birds

WHEN I was forming the Government Central Museum at Madras, an albino crow was brought to me, which was stuffed and placed in the museum. It was mentioned to me at the same time that there is a colony of albino crows at a part of the Malabar coast, but I have never been able to verify the statement. That district is daily becoming more frequented by Europeans, and some of them may soon be in a position to ascertain how far the report was correct. EDWARD BALFOUR

2, Oxford Square, September 19

As Mr. Page says, in NATURE (vol. xviii. p. 540), he has only heard of one white swallow, it may be interesting to him and your readers to learn that in the Newcastle Museum there is a specimen, also white specimens of the rook, pheasant, curlew, sparrow, and starling; in the same collection will also be found a "pale rose" coloured specimen of the bullfinch.

Newcastle-upon-Tyne, September 20 WM. LYALL

"Hearing of Insects"

I AM able to confirm the accounts given by Mr. Simson in your last number as to the probability of the hearing of insects. When travelling on the River Magdalena, New Granada, in 1861, the mode of which is by a long boat, arched over with bamboo, on which the sailors (bogas) passing from one extremity to the other, propel it with long poles, hugging the river bank, accompanied with wild cries and execrations, I observed on several occasions that these cries suddenly ceased, a dead silence following, and on inquiring the cause they pointed to nests high up in the trees, whispering the word vispa (wasp). As the bogas pursue their avocations in a state of semi-nudity, they have the greatest dread of these insects, fearing to speak aloud, as their only alternative if attacked by them is to plunge into the stream, where alligators abound. The wasp is long, slender, and black in colour.

117, Cromwell Road, S.W., Sept. 21 W. L. DUDLEY

The Meteor Shower of Andromedes I.

MR. GREG's meteor radiant (No. 103 of his 1876 catalogue) at R.A. 7°, Dec. 33° N., for July 21 to August, really consists of two well-defined showers near α Andromedæ. The meteors are quite distinct, and, moreover, there is a difference of 10° in declination. My observations since 1873 indicate two positions as below :—

I. July 6 to August 16, 6° + 37°	60 very swift streak-leaving meteors.
II. July 6 to August 11, 3° + 27°	23 slow, bright, trained meteors.

Schiaparelli and Zezioli, Greg and Herschel, and Tupman, found the former some years ago, and Denza gives the latter at 2° + 29° August 8–13. Mr. Greg averages these with several others (including one by Schmidt at 7° + 30°, and another by Heis at 11° + 30°, both for August) and finds a centre at 7° + 33° for the whole. There is no doubt, however, that there are *two* conspicuous contemporary radiants of entirely different meteors. This year, on August 10, in bright moonlight, I traced five meteors from 6° + 37°, and the epoch and place fall near Comet II. 1780, August 14, 3½° + 38½°, but the comet was only visible for three days after its discovery by Montaigne and Olbers on November 28, 1780, and hence the orbit is not likely to have been exactly determined. At the nodal passage the comet's orbit lies far within the orbit of the earth, so that an encounter of the earth with the comet-particles is only possible on the thesis of Weiss and Schiaparelli that "some part of the cometary materials repelled from its proper orbit by the sun in the form of the tail or other luminous appendage emitted by the comet near its perihelion passage extends to such a distance in its orbital plane as to intersect the orbit of the earth" (see B.A. *Report*, 1873, pp. 401–2).

Ashleydown, Bristol, September 16 W. F. DENNING

The Zoological Record

IN the third number of vol. iii. of the *Niederländisches Archiv für Zoologie* (Leiden: E. T. Brill), I published in German a "Catalogue Raisonné" of zoological works and papers that appeared in the Netherlands during 1875 and 1876. You noticed the appearance of this paper in your "Notes" (NATURE, vol. xvi. p. 112).

The principal reason of my publishing this bibliography was my wish to make known in other countries what is done in the Netherlands in the zoological department. For the same purpose, about the end of May, 1877, I sent a copy of my paper to the *Zoological Record* and addressed it "Solely to the Editor of the *Zoological Record*, care of Mr. Van Voorst, 1, Paternoster Row, London."

Afterwards studying vol. xii. and xiii. of the said "Record," I found that about twenty of the papers recorded in my catalogue were not mentioned in these volumes. Of course this might have been occasioned by the unimportance of these twenty unlucky papers; but conscientiously comparing their value with that of the other sixty of my bibliography, and as far as possible in general with the papers mentioned in the *Record* I got the conviction that this could not be the reason.

I feel a great deal of admiration, and at the same time of gratitude for the immense amount of work done by the contributors of the *Record*, and I quite agree with you (NATURE, vol. xviii. p. 485) that it would be to the everlasting disgrace of zoologists (not only of your tongue, but of all tongues) if its existence should be prematurely brought to a close. But only when I find in the *Record* as much completeness as possible, the use of it will spare me the endless trouble of looking for every detail over the totality of zoological literature.

Now I don't believe that in the case mentioned here (to secure this completeness) much care has been taken.

September 19 P. P. C. HOEK

Earth Pillars

SHOULD you deem the following of sufficient interest, will you kindly insert it in NATURE?

A few days since I saw an interesting example of minute earth-pillars on the shore of the Hecht Sea, above Kiefersfelden, Inn Thal. In a cove to the north the beach for many yards formed a perfect forest of little pillars, whose height ranged from a quarter to three-quarters of an inch. On the top of most lay a small stone, a fragment of wood or shell; but some, which had lost their coverings, were wearing away. The shell fragments (from a Unio, I fancy) seemed to form the most complete protection, and these often fitted the pillars like helmets; in fact, it required no great stretch of the imagination to fancy the whole a marching army, and the jutting wood fragments spears.

South Tyrol is by no means the only place in this country where earth-pillars occur, though the Bozen pillars are probably the finest. Amongst others in North Tyrol there is a very interesting example of large earth-pillars on the Brenner railway, between Innsbruck and Patsch, on the right—going south. JAMES H. MIDGLEY

Brixlegg am Inn, Tyrol, September 17, 1878

Indian Building Timber

IN NATURE, vol. xviii. p. 317, it is stated "much or most of the wood used in Peking in building houses, temples, and palaces is said to come from Corea;" it is further remarked editorially, "we think, however, our contemporary is in error in stating, without qualification, that 'the great wooden masts which support the noble temples and gatehouses of the Imperial City of Peking (all enormous, beautiful, and enduring spars) come from Corea.'"

Having had some experience in the timber and timber-trees of Burma I am inclined to the opinion that this valuable timber "nan-mu" therein referred to will prove to be identical with the wood used for the same purposes generally over Burma. The wood is called in Lower Burma "Pyenkadoo," it has a wide distribution under a variety of names, according to the different provincial dialects of the districts it is found in. Its great length of bole without branches, the different sizes at which it can be obtained renders it from its great durability, readiness to polish, and its variegated and coloured grain (brown mahogany colour) most suitable for the supports or pillars of "kyoings," or temples. It belongs to the natural order Leguminoseæ, specific name *Inga xylocarpa*.

Besides this there are several other woods highly esteemed by the Burmese for durability, and these chiefly are found amongst the Cassias and Dalbergias.

Inga xylocarpa has great toughness—a piece of three feet long by one inch square I find stood a breaking weight of 1,153 pounds; its specific gravity is nearly double that of teak and it does not float.

The objection to the introduction of the different ornamental and useful timbers of Burma is their toughness, hardness to work, and hence increased labour and wear of tools.

Whitby R. BENSON

[With reference to the question of the identity of the wood of the "nan-muh" tree with that of *Inga xylocarpa* we may point out that from material received at Kew the former has been referred to a Lauraceous tree, probably *Phœbe pallida*. From comparison of the two woods microscopically they present something in common, the annual rings, however, are much more apparent in the "nan-muh" than in the "Pyenkadoo." This latter is of a dark reddish brown, extremely heavy, as described by Col. Benson, while the nan-muh is of a dull umber colour and much lighter in weight.—ED.]

OUR ASTRONOMICAL COLUMN

THE INTRA-MERCURIAL PLANET.—The particulars of Mr. Lewis Swift's observations during the totality of the recent eclipse, given in his letter which appeared in NATURE last week, are satisfactory so far as they afford independent testimony to the existence of an unknown body in the vicinity of the star θ Cancri, or in the locality where Prof. Watson, a few minutes previously, had observed an object which he considers to have been an intra-Mercurial planet. In other respects Mr. Swift's letter is indefinite and contradictory in itself. He tells us that he observed two red stars "with large, round, and equally bright discs," estimating the distance between them at about 7′ or 8′; and, one of the objects being identified with θ Cancri, he intimates that the proximity of the other to this star enabled him to estimate its position with great exactness, especially in declination. But in a subsequent paragraph, where the place of the star is adopted from the Astronomer-Royal, the unknown object is fixed to a position which makes its distance from θ Cancri 30′, or four times as great as mentioned previously. The place of the supposed planet, according to Prof. Watson, was, as stated last week, in right ascension 8h. 27m. 24s., and declination 18° 16′ N.; and as the apparent place of the star at the time was in right ascension 8h. 24m. 39·9s., and declination 18° 30′ 19″, the distance between the two was 42′, on an angle at the star, of 110°. With regard to Mr. Swift's concluding observation as to the position of the presumed planet in its orbit, it is evident that, to present a round or nearly round disc, it must have been situate, as Prof. Watson infers, in the superior part of the orbit, and being to the west of the sun, would be approaching superior conjunction.

Prof. Watson states that the magnitude of the object in question was 4 to 4½, and that of the second unknown star, which he alone appears to have observed, was 3½, and adds, "they were probably really brighter, because the illumination of the sky was not considered in the estimates." Before he entered upon the reduction of his observations, he had thought the second object might be ζ Cancri (though surprised to find it so bright) because he did not see that star—a well-known double star, components $5\frac{1}{2}$ and $6\frac{1}{2}$,—a gust of wind which occurred just before the end of totality having possibly disturbed the telescope. Yet, the circumstance of his not having remarked ζ Cancri would be accounted for by his statement that he did not sweep further than the second object, which his reading places in right ascension 8h. 9m. 24s., declination 18° 3′, and which he believes to be correct—the sun coming out immediately afterwards, and of course putting an instantaneous termination to his observations.

VARIABLE STARS.—The following are the Greenwich times of geocentric minima of Algol and S Cancri in the last quarter of the present year, which fall between dusk and 13h. M.T.

ALGOL.

		h. m.				h. m.				h. m.
Oct.	7	12 27	...	Nov.	2	7 46	...	Dec.	12	11 11
,,	10	9 16	...	,,	19	12 39	...	,,	15	8 0
,,	13	6 5	...	,,	22	9 28	...	,,	18	4 49
,,	30	10 57	...	,,	15	6 17	...			

S CANCRI.

		h. m.				h. m.				h. m.
Oct.	2	11 46	...	Nov.	9	10 12	...	Dec.	17	8 38
,,	21	10 59	...	,,	28	9 35				

The maximum of Mira Ceti occurs on October 11, and the minimum of χ Cygni on October 26, according to Prof. Schönfeld.

THE METEOR OF DECEMBER 24, 1873.—Mr. Cleveland Abbe, Director of the Observatory of Cincinnati, sends us a discussion of the observations of a remarkable meteor seen on Christmas Eve, 1873, to which the attention of the Washington Philosophical Society had been particularly directed soon after its occurrence, and, on the suggestion of the late secretary of the Smithsonian Institution, a committee formed with the view of collecting and discussing observations. Though about fifty accounts of the appearance of the meteor were thus brought together, they have only sufficed to give a general idea of its track and altitude. Nearly all the observers describe it as equalling the full moon in brightness, with conical form moving base forward, but not followed as in so many cases, by any regular train; colour bright yellow, sparks or flames of red and blue proceeding from it. Mr. Abbe finds that it "entered the earth's atmosphere at some point vertically above the northern part of the State of Delaware, so that its apparent altitude, as seen at Danbury, Conn., was 30°, and at Washington, D.C., about 45°," whence he concludes its real altitude above the earth's surface to have been about ninety miles. Its after-course was downward to beyond Fairfax county, and at its nearest approach to the earth it was distant twenty miles. Its entire visible path of about 120 miles was described in from three to five seconds. An explosion occurred at a distance of about thirty miles from Washington, where the meteor was observed, amongst others, by Profs. Newcomb, Hilgard, and Holden.

MR. THOMAS GRUBB, F.R.S.

WE regret to announce the death of Mr. Thomas Grubb, F.R.S., and we cannot do so without referring to his marked talents and the important service he has rendered to science.

Mr. Grubb was originally intended for a mercantile career, but his natural tastes proved too strong to allow him to continue in the life he had begun. He became a mechanical engineer, and soon his workshops became known not only for excellent workmanship but still more for marked originality of conception in design. His *chef-d'œuvre* in both these respects is probably the machinery for engraving, printing, and numbering the notes of the Bank of Ireland.

Whilst Mr. Grubb's workshops were rising in reputation, he was quietly and steadily developing marked powers as a practical optician. During the construction of the great 6-feet equatorial at Parsonstown, the late Lord Rosse frequently acknowledges his obligations to Mr. Grubb for valuable suggestions. The system of levers on which Lord Rosse supported his specula, to avoid flexure, was of Mr. Grubb's invention. Of his chief scientific works we may mention:—

1. The equipment of nearly forty British magnetic stations under the direction of Provost Loyd.

2. A 15-inch reflecting telescope equatorially mounted at the Armagh Observatory.

3. The equatoreals of the Markree and Dunsink Observatories—refractors of 13 and 12 inches aperture respectively. And lastly, the great Melbourne telescope of 4 feet aperture, equatorially mounted, and driven by clockwork. After this last achievement Mr. Grubb retired from business life, succeeded by his son, Mr. Howard Grubb.

Such works as the 15-inch refractors in the observatories of Lord Lindsay and Dr. Huggins, and the great refractor of 27 inches aperture now in course of construction for Vienna show that the son is no unworthy representative of the father.

Mr. Thomas Grubb was elected a Fellow of the Royal Society of London in 1864, in recognition of his successful completion of the great Melbourne telescope, and of his other successful labours in the cause of science. He was also a member of the Royal Irish Academy. He died on the 19th instant at his residence in Dublin, and leaves to mourn his loss a large circle of friends whom his kindly genial manners and ever interesting conversation had drawn round him.

MR. THOMAS BELT, F.G.S.

THE scientific world will hear with regret the recent death of the well-known naturalist and geologist, Mr. Thomas Belt, F.G.S., which has just been telegraphed from Colorado. It is believed to have been caused by mountain fever. Elected a Fellow of the Geological Society in 1866, the geological world owes to him the division of the Lingula flags into Maentwrog, Ffestiniog, and Dolgelly flags, proposed in 1867. In 1874 appeared his well-known and deservedly popular "Naturalist in Nicaragua," in which he showed how his professional avocations as an engineer had lent keenness to his observing faculties, and how an acute reasoner can utilise his observations. The work conveyed much information on protective mimicry, plant-fertilisation, sexual selection, and the other collateral issues of the theory of evolution. It contained the first sketch of those views on glacial geology which were the most prominent subject of the author's study for the rest of his life. These views were given in considerable detail in the *Geological Magazine* for April, 1874, and were well expounded by Mr. Henry Woodward, F.R.S., in his presidential address of that year to the Geologists' Association. Mr. Belt skilfully answered his opponents in NATURE, vol. x., his controversial speaking and writing being always marked by a candour and temper which, if it did not carry conviction, could not fail to elicit admiration from perfect strangers and mere spectators. In November, 1875, he read a paper to the Geological Society "On the Drift of Devon and Cornwall" (*Quart. Jour. Geol. Soc.*, vol. xxxii.), and another "On the Steppes of Southern Russia" (*Quart. Jour. Geol. Soc.*, vol. xxxiii.), in June, 1877. He also contributed various papers to the *Quarterly Journal of Science*, amongst others one "On the Loess of the Rhine and the Danube," in January, 1877, and one "On the Glacial Period in the Southern Hemisphere," in July, 1877.

ON THE NATURE OF VIBRATORY MOTIONS[1]

On the Nature of Sound

SOUND is the sensation peculiar to the ear. This sensation is caused by rapidly-succeeding to-and-fro motions of the air, which touches the outside surface of the drum-skin of the ear. These to-and-fro motions may be given to the air by a distant body, like a string of a violin. The string moves to and fro, that is, it *vibrates*. These vibrations of the string act on the bridge of the violin, which rests on the belly or sounding-board of the instrument. The surface of the sounding-board is thus set trembling, and these tremors, or vibrations, spread through the air in all directions around the instrument, somewhat in the manner that water-waves spread around the place where a stone has been dropped into a quiet pond. These tremors of the air, however, are not sound, but the cause of sound. Sound, as we have said, is a *sensation;* but, as the cause of this sensation is always vibration, we call those vibrations which give this sensation *sonorous vibrations*. Thus, if we examine attentively the vibrating string of the violin, we shall see that it looks like a shadowy spindle, showing that the string swings quickly to and fro; but, on closing the ears, the sensation of sound disappears, and there remains to us only the sight of the quick to-and-fro motion which, the moment before, caused the sound.

Behind the drum-skin of the ear is a jointed chain of three little bones. The one, H of Fig. 1, attached to the

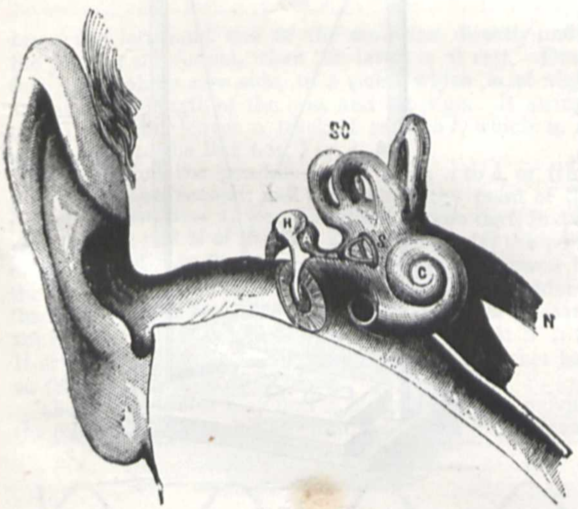


FIG. 1.

drum-skin, is called the *hammer;* the next, A, is called the *anvil;* the third, S, has the exact form of a stirrup, and is called the *stirrup-bone*. This last bone of the chain is attached to an oval membrane, which is a little larger than the foot of the stirrup. This oval membrane closes a hole opening into the cavity forming the *inner ear;* a cavity tunnelled out of the hardest bone of the head, and having a very complex form. The oval hole just spoken of opens into a globular portion of the cavity known as the vestibule, and from this lead three semicircular canals, SC, and also a cavity, C, of such a marked resemblance to a snail's shell that it is called *cochlea*, the Latin word for that object. The cavity of the inner ear is filled with a liquid, in which spread out the delicate fibres of the auditory nerve.

Let us consider how this wonderful little instrument acts when sonorous vibrations reach it. Imagine the violin-string vibrating 500 times in one second. The sounding-board also makes 500 vibrations in a second. The air touching the violin is set trembling with 500 tremors a second, and these tremors speed with a velocity of 1,100 feet in a second in all directions through the surrounding air. They soon reach the drum-skin of the ear. The latter, being elastic, moves in and out with the air which touches it. Then this membrane, in its turn, pushes and pulls the little ear-bones 500 times in a second. The last bone, the little stirrup, finally receives the vibrations sent from the violin-string, and sends them into the fluid of the inner ear, where they shake the fibres of the auditory nerve 500 times in a second. These tremors of the nerve—how we know not—so affect the brain that we have the sensation which we call sound. The description we have just given is not that of a picture created by the imagination, but is an account of what really exists, and of what can actually be seen by the aid of the proper instruments.

A body may vibrate more or less frequently in a second; it may swing over a greater or less space; and it may have several minute tremors while it makes its main swing. These differences in vibrations make sounds higher or lower in pitch, loud or soft, simple or compound. It is easy to say all this, but really to understand it, one must make experiments and discover these facts for himself.

On the Nature of Vibratory Motions

The character of a sound depends on the nature of the vibrations which cause it, therefore our first experiments will be with vibrations which are so slow that we can study the nature of these peculiar motions. These experiments will be followed by others on vibrations of the same kind, only differing in this—that they are so rapid and frequent that they cause sounds. A correct knowledge of the nature of these motions lies at the foundation of a clear understanding of the nature of sound. We hope that the student will make these experiments with care, and keenly observe them.

Experiment 1.—At the toy-shops you can buy for a few pence a wooden ball having a piece of elastic rubber fastened to it. Take out the elastic and lay it aside, as we shall need it in another experiment. Get a piece of fine brass wire, about 2 feet (61 centimetres) long, and fasten it to the ball. The weight of the ball should pull the wire straight, and, if it does not, a finer wire must be used. Hold the end of the wire in the left hand, and with the right hand draw the ball to one side. Let it go, and it will swing backward and forward like the pendulum of a clock. This kind of movement we call a *pendulous or transverse vibration*.

Experiment 2.—Cut out a narrow triangle of paper, 4 inches (10 centimetres) long, and paste it to the bottom of the ball. Twist the wire which supports the ball by turning the latter half round, and watch the paper pointer as it swings first one way and then the other. Here we have another kind of vibration, a motion caused by the twisting and untwisting of the wire. Such a motion is called a *torsional vibration*.

Experiment 3.—Take off the wire and the paper, and put the elastic on the ball. Hold the end of the elastic in one hand, and with the other pull the ball gently downward, then let it go. It vibrates up and down in the direction of the length of the elastic. Hence we call this kind of motion a *longitudinal vibration*.

These experiments show us the three kinds of vibrations, transverse, torsional, and longitudinal. They differ in direction, but all have the same manner of moving; for the different kinds of vibration, transverse, longitudinal, and torsional, go through motions with the same changes in velocity as take place in the swings of an ordinary pendulum. These vibrations all start from a position of momentary rest. The motion begins slowly, and gets faster and faster till the body gains the position it naturally has when it is at rest—at this point it has its

[1] From a forthcoming work on "Sound: a Series of Simple, Entertaining, and Inexpensive Experiments in the Phenomena of Sound, for the Use of Students of every Age." By Alfred Marshall Mayer, Professor of Physics in the Stevens Institute of Technology. Communicated by the author.

greatest velocity. Passing this point, it goes slower and slower till it again comes momentarily to rest, and then begins its backward motion, and repeats again the same changes in velocity.

It is now necessary that the student should gain clear ideas of the nature of this pendulous motion. It is the cause of sound. It exists throughout all the air in which a sound may be perceived, and, by the changes in the number, extent of swing, and combinations of these pendular motions, all the changes of pitch, of intensity, and of quality of sound are produced. Therefore the knowledge which we now desire to give the reader lies at the very foundation of a correct understanding of the subject of this book.

An experiment is the key to this knowledge. It is the experiment with

The Conical Pendulum

An ordinary pendulum changes its speed during its swings right and left exactly as a ball *appears* to change its speed when this ball revolves with a uniform speed in a circle, and we look at it along a line of sight which is in the plane of the circle.

Experiment 4.—Let one take the ball and wire to the farther end of the room, and by a slight circular motion of the end of the wire he must cause the ball to revolve in a circle. Soon the ball gets into a uniform speed around the circle, and then it forms what is called a conical pendulum; a kind of pendulum sometimes used in clocks. Now stoop down till your eye is on a level with the ball. This you will know by the ball appearing

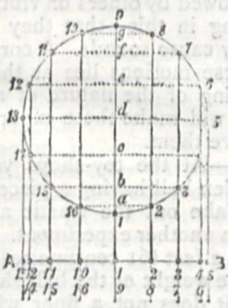


FIG. 2.

to move from side to side *in a straight line.* Study this motion carefully. It reproduces exactly the motion of an ordinary pendulum of the same length as that of the conical pendulum. From this it follows that the greatest speed reached during the swing of an ordinary pendulum just equals the uniform speed of the conical pendulum. That the apparent motion you are observing is really that of an ordinary pendulum you will soon prove for yourself to your entire satisfaction; and here let me say that one principle or fundamental fact seen in an experiment and patiently reflected on is worth a chapter of verbal descriptions of the same experiment.

Suppose that the ball goes round the circle of Fig. 2 in two seconds; then, as the circumference is divided into sixteen equal parts, the ball moves from 1 to 2, or from 2 to 3, or from 3 to 4, and so on in one-eighth of a second. But to the observer who looks at this motion in the direction of the plane of the paper the ball *appears* to go from 1 to 2, from 2 to 3, from 3 to 4, &c., on a line A B, while it really goes from 1 to 2, from 2 to 3, from 3 to 4, &c., in the circle. The ball when at 1 is passing directly across the line of sight, and, therefore, appears with its greatest velocity; but when it is in the circle at 5 it is going away from the observer, and when at 13 it is coming toward him, and, therefore, although the ball is really moving with its regular speed when at 5 and 13, yet it appears when at these points momentarily at rest. From a comparison of the similarly numbered positions of the ball in the circle and on the line A B, it is evident that the ball appears to go from A to B and from B back to A in the time it takes to go from 13 round the whole circle to 13 again. That is the ball appears to vibrate from A to B in the time of one second, in which time it really has gone just half round the circle. A comparison of the unequal lengths 13 to 12, 12 to 11, 11 to 10, &c., on the line A B, over which the ball goes in equal times, gives the student a clear idea of the varying velocity of a swinging pendulum.

Fig. 3 represents an upright frame of wood standing on a platform, and supporting a weight that hangs by a cord. A A is a flat board about 2 feet (61 centimetres) long and 14 inches (35·5 centimetres) wide. B B are two uprights so high that the distance from the under side of the cross-beam C to the platform A A is exactly $41\frac{1}{10}$ inches (1 metre and 45 millimetres). The cross-beam C is 18 inches (45·7 centimetres) long. At D is a wooden post standing upright on the platform. Get a lead disk, or bob, $3\frac{3}{16}$ inches (8 centimetres) in diameter, and $\frac{5}{8}$ inch (16 millimetres) thick. In the centre of this is a hole 1 inch (25 millimetres) in diameter. This disk may easily be cast in sand from a wooden pattern. At the tinner's we may have made a little tin cone $1\frac{3}{16}$ inch (30

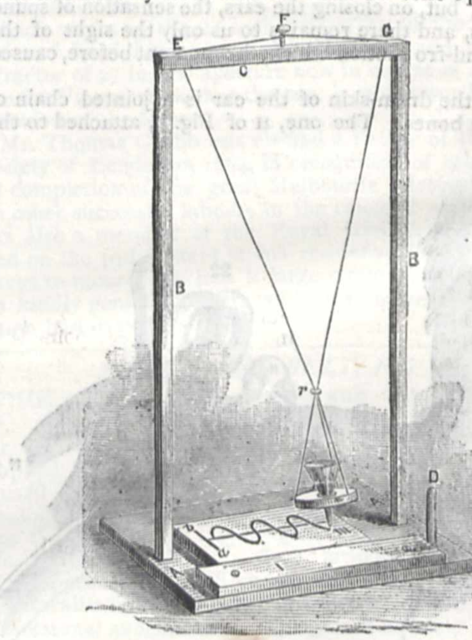


FIG. 3.

millimetres) wide at top, and $2\frac{1}{4}$ inches (57 millimetres) deep, and drawn to a fine point. Carefully file off the point till a hole is made in the tip of the cone of about $\frac{1}{16}$ inch in diameter. Place the tin cone in the hole in the lead disk, and keep it in place by stuffing wax around it. A glass funnel, as shown in the figure, may be used instead of the tin cone. With an awl drill three small holes through the upper edge of the bob at equal distances from each other. To mount the pendulum, we need about 9 feet (271·5 centimetres) of fine strong cord, like trout-line. Take three more pieces of this cord, each 10 inches (25·4 centimetres) long, and draw one through each of the holes in the lead-bob and knot it there, and then draw them together and knot them evenly together above the bob, as shown in the figure. On the cross-bar, at the top of the frame, is a wooden peg shaped like the keys used in a violin. This is inserted in a hole in the bar—at F in the figure. Having done this, fasten one end of the piece of trout-line to the three cords of the bob, and pass the other end upward through the hole marked E; then pass it through the hole in the key F; turn the key round several times; then pass the cord through the hole at G, to the bob, and fasten it there to the cords. Then get a small bit of copper wire and bend it once round the two cords just above the knot, as at *r* in the figure. This wire ring, and the upright post at the side of the platform, we do not need at present, but they will be used in future experiments with this pendulum.

Tack on the platform A A a strip of wood I. This serves as a guide, along which we can slide the small board *m*, on which is tacked a piece of paper.

Experiment 5.—Fill the funnel with sand, and, while the pendulum is stationary, steadily slide the board under it. The running sand will be laid along L M, Fig. 4, in a straight line. If the board was slid under the sand during exactly two seconds of time, then the length of this line may stand for two seconds, and one-half of it may stand for one second, and so on. Thus, we see how time may be recorded in the length of a line.

Brush off the heaps of sand at the ends of the line, and

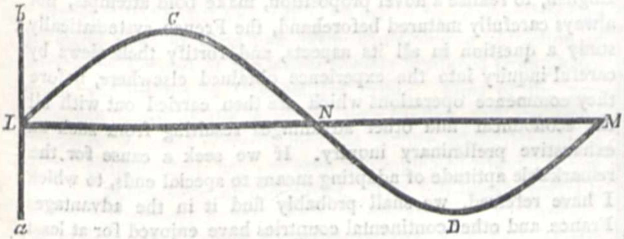


FIG. 4.

bring the left-hand end of the sand-line directly under the point of the funnel, when the latter is at rest. Draw the lead bob to one side, to a point which is at right angles to the length of the line, and let it go. It swings to and fro, and leaves a track of sand, *a b*, which is at right angles to the line L M, Fig. 4.

Suppose that the pendulum goes from *a* to *b*, or from *b* to *a*, in one second, and that, while the point of the funnel is just over L, we slide the board so that, in two seconds, the end M of the line L M comes under the point of the funnel. In this case the sand will be strewed by the pendulum to and fro, while the paper moves under it through the distance L M. The result is that the sand appears on the paper in a beautiful curve L C N D M. Half of this curve is on one side of L M, the other half on the opposite side of this line.

The experimenter may find it difficult to begin moving the paper at the very instant that the mouth of the funnel

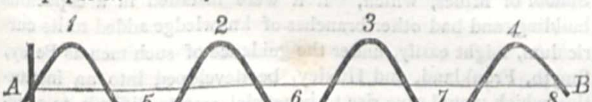


FIG. 5.

is over L; but, after several trials, he will succeed in doing this. Also, he need not keep the two sand-lines, L M and *a b*, on paper during these trials; he may as well use their traces, made by drawing a sharply-pointed pencil through them on to the paper.

By having a longer board, or by sliding the board slowly under the pendulum, a trace with many waves in it may be formed, as in Fig. 5.

As the sand-pendulum swung just like an ordinary pendulum when it made the wavy lines of Figs. 4 and 5, it follows that these lines must be peculiar to the motion of a pendulum, and may serve to distinguish it. If so, this curve must have some sort of connection with the motion of the conical pendulum, described in Experiment 4. This is so, and this connection will be found out by an attentive study of Fig. 6.

In this figure we again see a wavy curve, under the same circular figure which we used in explaining how the motion of an ordinary pendulum may be obtained from the motion of a conical pendulum. This wavy curve is made directly from measures on the circular figure, and certainly bears a striking resemblance to the wavy trace made by the sand-pendulum in Experiment 5. You will soon see that to prove that these two curves are precisely the same is to prove that the apparent motion of the conical pendulum is exactly like the motion of the ordinary pendulum.

The wavy line of Fig. 6 is thus formed:—The dots on A B, as already explained, show the apparent places of the ball on this line, when the ball really is at the points correspondingly numbered on the circumference of the circle. Without proof, we stated that this apparent motion on the line A B was exactly like the motion of a pendulum. This we must now prove. The line L M is equal to the circumference of the circle stretched out. It is made thus:—We take in a pair of dividers the distance 1 to 2, or 2 to 3, &c., from the circle, and step this distance off sixteen times on the line L M; hence L M equals the length of the circumference of the circle. *In time* this length stands for two seconds, for the ball in Experiment 4 took two seconds to go round the circle. This same length, you will also observe, was made in the same time as the sand-line L M was made in Experiment 5. In Fig. 6 the length L M, of two seconds, is divided into sixteen parts; hence each of them equals one-eighth of a second, just as the same lengths in the circle equal eighths of a second. Thus the line L M of Fig. 6, as far as a record of time is concerned, is exactly like the sand-line L M of Experiment 5, and the line A B of Fig. 6, in which the ball appeared to move, is like the line *a b* of Fig. 4, along which the sand-pendulum swung.

Now take the lengths from 1 to 2, 1 to 3, 1 to 4, 1 to 5, and so on, from the line A B of Fig. 6, and place these lengths at right angles to the line L M at the points 1, 2, 3, 4, 5, and so on; by doing so, we actually take the distances at which the ball appeared from 1 (its place of greatest velocity), and transfer them to L M; therefore, these distances correspond to the distances from L M, Fig. 4, to which the sand-pendulum had swung at the end of the times marked on L M of Fig. 6.

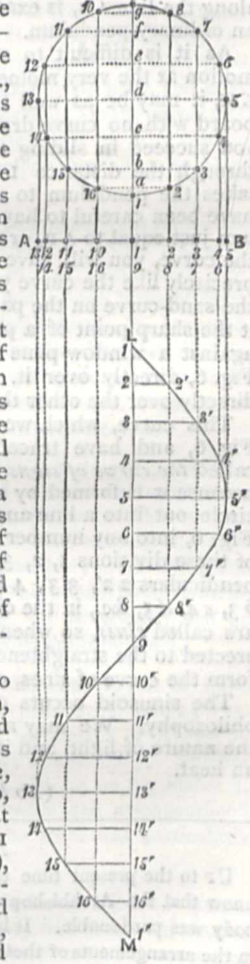


FIG. 6.

Join the ends of all these lines, 2 2′, 3 3′, 4 4′, &c., by drawing a curve through them, and we have the wavy line of Fig. 6.

This curve evidently corresponds to the curve L C N D M of Fig. 4 made by the sand-pendulum; and it must be evident that, if this curve of Fig. 6 is *exactly* like the curve traced by the sand-pendulum in Experiment 7, it follows that the *apparent* motion of the conical pendulum, as seen in the plane in which it revolves, is exactly like the *real* motion of an ordinary pendulum.

Experiment 6.—To test this, we make on a piece of paper one of the wavy curves exactly as we made the one in Fig. 6, and we tack this paper on the board L M of the sand-pendulum, being careful that when the board is slid under the stationary pendulum the point of the funnel goes precisely over the centre line L M (Fig. 9) of the curve.

Now draw the point of the funnel aside to a distance from the line L M equal to one-half of A B, or, what is the same, from 5 to 5′ of Fig. 9. Pour sand in the funnel, and let the bob go. At the moment the point of the funnel is over L, slide the board along so that when the point of the funnel comes the third time to the line L M, it is at the end M of this line. This you may not succeed in doing at first, but after several trials you will succeed, and then you will have an answer from the pendulum as to the kind of motion it has, for you will see the sand from the swinging pendulum strewed precisely over the curve you placed under it. Thus you have conclusively proved that the apparent motion of the conical pendulum, along the line A B, is exactly like the swinging motion of an ordinary pendulum.

As it is difficult to start the board with a uniform motion at the very moment the pendulum is over the line L M, it may be as well to tack a piece of paper on the board with no curve drawn on it, and then practise till you succeed in sliding the board under the pendulum, through the distance L M, in exactly the time that it takes the pendulum to make two swings. Now, if you have been careful to have had the swing of your pendulum just equal to A B, or from 5 to 5′ on the drawing of the curve, you will have made a curve in sand which is precisely like the curve you have drawn; for, if you trace the sand-curve on the paper by carefully drawing through it the sharp point of a pencil, and then place this trace against a window-pane with the drawing of the curve, Fig. 6, directly over it, you will see that one curve lies directly over the other throughout all their lengths.

This curve, which we have made from the circle in Fig. 6, and have traced in sand by the pendulum, is called *the curve of signs*, or the *sinusoid*. It is so called because it is formed by stretching the circumference of a circle out into a line and then dividing this line, L M of Fig. 6, into any number of equal parts. From the points of these divisions 1, 2, 3, 4, 5, &c., of L M, we erect perpendiculars 2 2′, 3 3′, 4 4′, 5 5′, &c., equal to the lines *a* 2, *b* 3, *c* 4, *d* 5, &c., in the circle. These lines in the circle are called *sines*, so when we join the ends of these lines, erected to the straightened circumference by a curve, we form the curve of sines, or the sinusoid.

The sinusoid occurs often during the study of natural philosophy. We may meet with it again in our book on the nature of light, and it certainly will occur in our book on heat.

(*To be continued.*)

NOTES

UP to the present time the ignorance of those who did not know that the Archbishop of Canterbury was a degree-giving body was pardonable. It is so no longer. A serious alteration in the arrangements of these diplomas is now announced. Archbishop Tait, while he intends to dispense doctorates as before at his will and pleasure, has determined that his degree of M.A. is from December next to be a matter of examination. The standard is to be that of "honour examinations in the Universities." There is to be due choice of subjects, among which, however, Greek and Latin are not to be compulsory, though English literature is. To qualify for examination, formal testimonials required for University matriculation, with the addition of a certificate from the Bishop of the diocese whence the candidates come, are required. As the *Daily News* puts it, "the Archbishop has evidently determined to make himself into a university with all the paraphernalia which the modern conception of such a body requires." Both the London Examining Board (commonly called the London University) and Owens College are to be congratulated on the publicity now given to this singular system of granting degrees. The London Examining Body is not a teaching body, neither is the Archbishop, but the Archbishop is a university, therefore the London University system is perfect, and all methods of education whatever may be disregarded so that a standard of instruction is reached. Owens College as a teaching centre which has won its way to general esteem and confidence, may now bide its time, for this last grotesque thing calling itself a university will either make the power of granting degrees, and degrees themselves ridiculous, or direct attention to the whole subject.

ALTHOUGH the Paris meeting of the Iron and Steel Institute has not called for any lengthy notice at our hands, there are passages in Dr. Siemens' admirable address to which we cannot too strongly draw attention, and which we are anxious to place on record in our columns. He remarked that "Whilst the English, to realise a novel proposition, make bold attempts, not always carefully matured beforehand, the French systematically study a question in all its aspects, and fortify their views by careful inquiry into the experience obtained elsewhere, before they commence operations which are then carried out with all the economical and other advantages resulting from such an exhaustive preliminary inquiry. If we seek a cause for the remarkable aptitude of adapting means to special ends, to which I have referred, we shall probably find it in the advantages France and other continental countries have enjoyed for at least a generation of a more extended technical education than we could boast of, and of the personal influence which has been exercised by a line of scientific writers and experimentalists, of whom I shall only mention here such honoured names as those of Réaumur, Ebelmen, Régnault, Pouillet, Péclet, Thomas, and Le Châtelier, as belonging to the past, and of Deville, Grüner, Lan, Laurens, Jordan, Frémy, and Dumas, who are fortunately still among us. It is chiefly to such men as these that France owes her admirable system of education, which enables her to place her metallurgical establishments under the guidance of men who are scientifically qualified for the discharge of their respective duties, and for the attainment of practical results which may well excite our admiration." The organisation of the École Centrale, the creation of M. Dumas, recommends itself, as it may well do, to Dr. Siemens, and he points out that the only establishment in Great Britain comparable with the École Centrale as regards metallurgy is our School of Mines, which, "if it were installed in a capacious building, and had other branches of knowledge added to its curriculum, might easily, under the guidance of such men as Percy, Smyth, Frankland, and Huxley, be developed into an institution which would give rise to beneficial results difficult to overestimate." Had Dr. Siemens been speaking in England he would doubtless have added that this was the distinct recommendation made by the Duke of Devonshire's Commission after a long inquiry. The Government has not yet acted upon this recommendation, and the result is that students of the School of Mines have to get their mathematics when and how they can; they form no part of the curriculum. Many may think that such schools in France are too heavily weighted with mathematics, but to omit the subject altogether is to court Scylla with a vengeance. Why should not each student of the School of Mines receive, as at the École Centrale, a three years' course of general scientific education, including the higher branches of mathematics, as well as physical science, pure and applied chemistry, geology, mechanics, metallurgy, and mineralogy.

MR. H. FORBES, F.L.S., is about to leave this country to investigate the fauna and flora of Celebes, Borneo, and adjacent islands. He proposes to devote five or six years to the work.

VESUVIUS is now giving some very definite signs of an eruption.

MR. JOHN PENN, F.R.S., the eminent marine engineer, died on Monday last, at Lee, in his seventy-third year. Mr. Penn's various patents for marine engines were considered so good that no fewer than 740 British war vessels were fitted with his machinery. Among them were the *Warrior*, the *Black Prince*, the *Achilles*, the *Hercules*, and the *Sultan*. Messrs. Penn also supplied the engines for nearly all the largest war ships for the Italian, Spanish, Brazilian, German, Danish, and Peruvian Governments, and those for the yachts of the Queen, the Emperor of Russia, the Khedive, the Sultan, and the Emperor of Austria. Mr. Penn was elected a member of the Institution of Civil Engineers in 1828, and a Fellow of the Royal Society in 1859. He was also a past president of the Society of Mechanical Engineers, and had received many marks of distinction from various foreign governments.

MESSRS. BARRAUD AND JERRARD are to be congratulated on the latest issue of their fine plate containing a collection of portraits of Fellows of the Royal Society produced from photographs and printed in "permanent print." Most of the portraits and many of the *poses* are very lifelike, and the introduction of so many portraits into one picture, while retaining a certain artistic effect, has not been accomplished without a considerable overcoming of difficulties. The full-length portraits of the Astronomer-Royal, Sir J. Lubbock, Dr. Richardson, Dr. Siemens, Dr. Lister, Dr. Spottiswoode, and Prof. Martin Duncan, in the fore-front, are all excellent. A convenient key accompanies the plate.

THE Seth Thomas Clock Company have recently introduced a time-keeper with a novel and most useful addition. This consists of a perpetual calendar, the day, date, and month being shown on a second dial. We have personally tested the action of this part and can state that the mechanism takes ordinary leap years perfectly into account. Indeed, the novelty is a marvel of ingenuity. We hope later to be able to give more details.

THE Trustees of the Australian Museum seem not only to have an unhappy knack of getting into hot water, but a strong feeling that it is good to remain in it. Without entering into the quarrel between them and the late curator, any one will regret the course of action thus referred to in the last report "to his Excellency, the Governor-in-Chief":—"The Trustees regret to state that, notwithstanding their strenuous endeavours to bring all disputes with their late Curator to a satisfactory conclusion, and to relieve the institution of the custody of the whole of the property which they could admit to belong to him, that gentleman has thought fit to bring an action to recover certain medals awarded to him as Curator of the Museum in respect of property of the Museum exhibited on various occasions, at the expense of the Museum, and certain specimens and articles of clothing and furniture alleged to have been detained by the Trustees. The Crown Solicitor was authorised by the Minister of Justice and Public Instruction to defend the action, which came on for trial in the Supreme Court on November 6, and resulted in a verdict against the Trustees of 50*l*. damages for the temporary detention of the plaintiff's property, which had been returned to him before the commencement of the action, and the further sum of 850*l*. in addition to the sum of 25*l*. which the Trustees had been advised to pay into court as amply sufficient to cover the value of the articles to which the plaintiff could show even a colour of title. Notwithstanding the astonishment of the Trustees at this most unexpected result, they yielded to the advice offered by their counsel and by the court, *and offered by way of compromise to give up the medals and other property claimed, and to pay the further sum of* 175*l*. *in addition to the* 25*l*. *paid into court*, but this offer was rejected by the plaintiff, whereupon a *rule nisi* for a new trial was granted by the court."

THE daughter of Laplace has offered an excellent picture of her father to be copied. The family of Arago have likewise offered a picture of the illustrious astronomer. A large and excellent picture of Leverrier was in the hands of M. Bischofsheim, who wrote a letter to Admiral Mouchez, wishing him to take possession of it, at his earliest convenience, on behalf of the Observatory.

ON October 4–6 the annual meeting of ornithologists will take place at Berlin. The following papers will be read:—On the birds of the Danube forty years ago and at the present time, by E. von Homeyer (Stolp), president of the society; On the recent researches in the osteology and myology of birds with regard to classification, by Prof. Blasius (Brunswick); On an ornithological excursion to Hungary and Croatia, by Dr. Brehm (Berlin); On the latest collection of birds from Eastern Africa, by Prof. Cabanis (Berlin); On the birds of the Caucasus, by Dr. Radde (Tiflis); On the latest acquisitions of the Zoological Museum of Berlin, with special reference to the nests and eggs of African birds, by Dr. Reichenow (Berlin); On the importance of splanchnology with regard to systematics, by Herr Gadow (Algiers); On the progress of ornithology since 1875, by Herr Schalow (Berlin).

A LARGE and very brilliant meteor was observed at numerous places in Central Germany on September 6. Near Hanau it appeared in the south-east about 9.10 P.M., and looked like a large comet, with a solid nucleus and a long train of light stretching across the sky to the north-west. It appeared suddenly like a flash of lightning, and, when the nucleus had disappeared, the line of light yet remained, and little stars could be noticed in it by the naked eye. The total duration of the phenomenon was about thirty seconds.

ON September 15, at 6 to 7 P.M., a large meteor was observed at Montpellier. On the same day and at about the same time, one was witnessed at Tenez, and Constantine in Algeria, and a number of places at great distances from each other. It is not yet known whether it was the same body or if the earth met in its course a meteor swarm. The bolides were notable for their brilliancy and their duration. It is reported that at Constantine the phenomena were accompanied by noise.

A SHORT but violent shock of earthquake was felt at Buir, near Düren (Rhenish Prussia), on September 2, at 9.15 P.M. Indeed it is stated that since the great earthquake of August 26 shocks have been felt in that district almost daily. Another shock is reported from Remagen, on the Rhine, on September 3, at 1 A.M., and a third one from Wiesbaden and neighbourhood on September 14, at 11.35 P.M. The latter was particularly violent.

ACCORDING to the last official calculation the total receipts of the Exhibition from entrance-money will reach 13,000,000 francs up to the end of October. There are other sources of revenue and a subvention from the City of Paris. It is supposed that the deficit will not exceed 400,000*l*., which will be more than covered from various sources.

QUITE recently a most remarkable new cave has been discovered in the United States near Glasgow Junction, Kentucky. It has been investigated to a distance of no less than twenty-three miles in one direction and sixteen in another. Most of its passages are very broad, and it is stated that a carriage with a pair of horses has room to drive some eleven miles into the interior. It contains several very deep rivers, one of which has been traced to a distance of fourteen miles; further progress was then arrested, the cave narrowing too much to allow a boat to pass. The cave is described to be "most wonderful," by an American contemporary, and is said to surpass in grandeur all other caves hitherto known, the Mammoth Cave not excepted. Some human remains, resembling Egyptian mummies, were also found in the cave. They were contained in stone sarcophagi of rough structure. The floor of the cave is extremely uneven, full of fissures and irregularities, so that it seems as if violent volcanic phenomena had taken place here. The new cave has been baptised with the more or less appropriate name Grand Crystal Cave. Our readers are doubtless aware that the Mammoth Cave is also situated in the vicinity of Glasgow Junction, Kentucky.

AMONG the resolutions passed by the International Congress on Weights, Measures, and Coins, at Paris, was the following:—The Congress learns with pleasure the progress of the metric system; it deplores that England, Russia, and the United States have not yet entered into the same path; and it is of opinion that the Governments of those countries should be solicited to give effect as early as possible to an act of progress so eminently useful to science, commerce, and international relations." The British and American members had a separate meeting, and resolved to petition their respective Governments to appoint a mixed commission to consider the adoption of the metric system by both countries, and to make all necessary recommendations for the proper legislation to secure the desired end.

THE conchological collection formed by the late Dr. Marie, of New Caledonia, has been purchased by Mr. Bryce-Wright.

AT the Royal Foundry of Munich a colossal monument cast in bronze was exhibited from September 8 to 11. It is intended for St. Louis, U.S., and represents Alexander von Humboldt; the design is by Herr von Müller, jun. The figure is some 3½ metres in height, and represents Humboldt in the freshness of manhood, leaning against a stump of a tree, upon which he has deposited his mantle; in the right hand he holds a map. The statue will be placed upon a stone pedestal, which is in course of construction at St. Louis, and which will be adorned by three relief medallions by the same artist, the one on the front showing the features of the founder of the monument, Mr. Henry Shaw, of St. Louis.

THE following are the more important addresses delivered and papers read at the sectional meetings of the German Naturalists' Association at Cassell:—Mathematical and Astronomical Section: On some new solutions of the problem of the division of the ball, by Prof. E. Hess (Marburg); On the old calculating machine of Leibnitz, by Prof. Listing (Göttingen); On the solar parallax, by the same. Prof. Listing gives the value $\pi = 8''\cdot8786$ as the most correct one at present.—Physical and Meteorological Section: On some new magnetic phenomena, by Dr. Fromme (Göttingen); On the influence of interior friction upon resonance, by Dr. Auerbach (Breslau); On a new method of employing the induction of the earth for the determination of the magnetic inclination, by Dr. Schering (Göttingen); On a new apparatus for the distillation of mercury, by Dr. Weber (Kiel); On a case of pleochroism, by Prof. Staedel (Tübingen); On a new method and apparatus to determine low tensions of vapours, by Dr. Moser (Berlin); On some applications of total reflexion, by Prof. Kohlrausch (Würzburg); On the theory of the exchange of air through porous walls, by Prof. Recknagel (Kaiserslautern); Remarks on galvanometry, by Prof. Töpler (Dresden); On the point of gravitation in curves, planes, and solid bodies, by Dr. Feussner (Marburg); Record of observations of solar spots and protuberances, by Prof. Spörer (Potsdam); On the behaviour of different heat colours in the reflection of polarised rays from metals, by Prof. Knoblauch (Halle); On the mathematical theory of friction electricity, by Dr. Schering (Göttingen); On an improvement in the hair hygrometer, by Dr. Nippoldt (Frankfort-on-the-Main). Chemical Section: On the chemical constitution of the turnip molasses, by Prof. Gunning (Amsterdam); On some new platina compounds, viz., platinofulminates, by Prof. von Meyer (Leipzig); On pyromeconic acid, by the same; On a new apparatus for the combustion of organic substances containing halogens, by Dr. Kopfer (Bonn); On the conversion of starch into sugar, by Dr. Salomon (Brunswick); On a physically isomeric modification of dinitrochlorobenzol, by Prof. Daubenheimer (Giessen); On the conditions of life of the lower organisms, by Prof. Gunning (Amsterdam); On a new method in the preparation of bromine, by Dr. Frank (Stassfurt); On some new phenomena concerning gases, by Prof. Mitscherlich. Geological and Palæontological Section: On the heat of the earth, by Herr E. Dunker (Halle); On some animal footmarks in the coloured sandstone of Carlshafen, by Dr. Hornstein; On the geological condition of the Büdingen forest, by Dr. Bücking; On an explanation of earthquakes, by Herr von Dücker; On the tridymit of Friedrichroda (Thuringia), by Dr. Luedecke; On basalt and its decay, by Dr. Franke; On the geological condition of the Kyffhäuser mountain, by Dr. Moesta. Geographical Section: On the botanical aspect of the Caucasus, by Dr. Raddé; On an exploring expedition to San José de Cucuta (South America), by Dr. K. Müller (Halle); On mountain and valley winds and their effect upon the vegetation of volcanic mountains, by Prof. Rein (Marburg); Ethnological researches in the Island of Cyprus, by Herr Schmölder (Frankfort); On the progress of the commercial relations between Europe and Western Siberia, by Dr. M. Lindemann (Bremen). Numerous papers and treatises were read at the zoological, botanical, anatomical, and physiological section-meetings, but by far the greater part were of minor interest, most of them having special reference to medicine only. The medical sections were as numerous as last year at Munich, and the attendance was, if anything, greater than on any previous occasion.

THE Aeronautical Society has issued its twelfth annual report. To it, in accordance with the expressed intention to reprint any matter of interest which might be otherwise unattainable, is annexed a reprint of a pamphlet printed in the year 1810 by Thos. Walker, of Hull, its title being "A Treatise on the Art of Flying by Mechanical Means." Mr. Walker was a portrait painter. An American would say that the treatise was a little "mixed."

THE physicists of the French Central Bureau of Meteorology are engaged in establishing a nomenclature to diminish the number of letters used in signalling.

THE American Academy of Arts and Sciences is probably one of the most active and efficient scientific societies anywhere; its *Proceedings* will bear comparison with those of any society in the old country. The part before us, including the period from November, 1877, to May, 1878, contains a number of papers of great value, the titles of which we shall give meantime, in the hope of being able to notice some of them in detail shortly. The Moon's Zodiacal Light; Undulations Observed in the Tail of Coggia's Comet, 1874; Sudden Extinction of the Light of a Solar Protuberance; On Saturn's Rings, by L. Trouvelot; Supplementary Note on the Theory of the Horizontal Photoheliograph, by Prof. William Harkness, U.S. Navy; Researches on the Substituted Benzyl Compounds, by C. Loring Jackson; Remarks on the Brain, illustrated by the Description of the Brain of a Distinguished Man, by Thomas Dwight, M.D.; Theory of Absorption-Bands in the Spectrum, and its Bearing in Photography and Chemistry, by Dr. Robert Amory; Surfaces of the Second Order, as treated by Quaternions, by Abbott Lawrence Lowell; On the Synonymy of some Species of Uredineæ, by W. G. Farlow; Metasomatic Development of the Copper-bearing Rocks of Lake Superior, by Raphael Pumpelly; Investigations in Quaternions, by Washington Irving Stringham; On a New Method for the Separation and Subsequent Treatment of Precipitates in Chemical Analysis, by F. A. Gooch; On Peirce's Criterion, by Benjamin Peirce; Note on the Measurement of Short Lengths, by Leonard Waldo; Contributions to the Botany of North America, by Asa Gray; Spherical Conics, by Gerrit Smith Sykes; On the Influence of Internal Friction upon the Correction of the Length of the Second's Pendulum for the Flexibility of the Support, by C. S. Peirce; Colour Perception, by G. Stanley Hall; On the Intensity of Terrestrial Magnetism at Cambridge, by Henry Goldmark. Among the foreign honorary members we find the names of J. C. Adams, Airy, Cayley, Sylvester, Clerk-Maxwell, Balfour Stewart, Stokes, Sir Wm. Thomson, Darwin, Joule, W. H. Miller, A. C. Ramsay, Sabini, Bentham, Hooker, Owen, Max Müller, Rawlinson, Gladstone, Tennyson.

THE receipts of the Giffard Captive Balloon on the first sixty days have been more than 500,000 francs. The sum spent on the construction of the balloon has been realised. It is supposed that the receipts for the month of October will be sufficient to cover the working expenses, so that M. Giffard will be rewarded for his enterprise by the possession of the balloon, machinery, and gas-producing apparatus.

A MILLION tickets have been sold at one franc each for the Paris Exhibition Lottery. Two-thirds of the sum are to be spent in purchasing prizes, the other third being destined to assist the Government in paying the travelling expenses of the working men visiting the Exhibition.

THE additions to the Zoological Society's Gardens during the past week include two Ostriches (*Struthio camelus*) from Africa, presented by the Hon. H. C. Vivian, H.B.M. Consul-General; two Secretary Vultures (*Serpentarius reptilivorus*) from South Africa, presented by C. Rivers Wilson, C.B.; an Oriental Eagle Owl (*Bubo orientalis*) from Karenee, Siam, presented by Mr. Charles Fowler; two Prairie Marmots (*Cynomys ludovicianus*) from North America, two Smaller Rattlesnakes (*Cerotalus miliarius*) from Canada, presented by Mr. Wilfred G. Marshall; two Egyptian Gazelles (*Gazella dorcas*) from Egypt, presented by Mr. Thomas Moss; three Reddish Finches (*Spermophila nigro-aurantia*), one Bluish Finch (*Spermophila cærulescens*), one Half-white Finch (*Spermophila hypoleuca*), one Tropical Seed Finch (*Oryzoborus torridus*) from South America, presented by Mr. R. C. Batterbee; three Rufous Tinamous (*Rhynchotus rufescens*) from Brazil, presented by Mr. J. A. Iliffe; two Lesser Black-backed Gulls (*Larus fuscus*), British Isles, presented by Mr. A. H. Cocks, F.Z.S.

THE FIGURE AND SIZE OF THE EARTH[1]

II.

IN addition to the measurement by Picard above-mentioned, two other arcs were measured in a north and south direction; La Hire measured northward towards Dunkirk, and Cassini southwards towards Perpignan. The result, published by Cassini in the year 1718, was as follows:—The southern arc gave 57098 toises (Picard's was 57060), and the northern 56960. This result was quite opposed to Newton's theory; it indeed favoured an elongated figure for the earth. There now began among the learned of the time a controversy which was carried on with much bitterness, between the supporters of Newton and Huyghens on the one hand, and of Cassini on the other. Cassini published the results of his measurements in his work, "De la Grandeur et de la Figure de la Terre" (Paris, 1722), and in consequence of the high reputation which he, as Director of the Observatory and member of the Academy, possessed over all France, nearly all the French *savants* took his side. But the arguments adduced by him were not such as could convince the great number of Newton's followers in all other nations. The French results were all the more objected to, that the measured arcs were much too small to allow one to base thereon a conclusion as to the form of the earth. In order to bring to an end the controversy carried on with so much violence on both sides, the French Government sent out in the year 1735 an expedition, consisting of the astronomers Bouguer, de la Condamine, and Godin, to Peru to measure the length of an equatorial degree. A second expedition, consisting of the academicians Maupertuis, Clairaut, and Lemonnier, was sent to Lapland; and while the former found the length of a degree at the equator to be 56753 toises, the latter, in connection with Celsius, found the result in latitude 66° 20′ to be 57437 toises. These results, obtained from the most careful observations and the most accurate calculations, gave the palm to the Newtonian theory, and the amount of flattening as ascertained so nearly agreed with Newton's calculation as to give the greatest confidence in his works.

It should, however, be mentioned that the Lapland measurement was much behind the Peruvian in correctness. Indeed it was soon seen that it was much more inaccurate than Picard's, and therefore the flattening of the earth was based only on Picard's and the Peruvian measurements. But the admirable execution of the Peruvian measurements was of importance also in another respect; in it, two base-measurements had been made, the southern base being considered a base of verification. This importance, which appeared at a later period, consisted in the fact that the unit of measure used in laying down the base-line, the "Toise of Peru," after it had with the greatest care been brought to Paris uninjured, was instated as the French normal measure, and this standard, at a temperature of 13° R., was appointed as the legal measure of length for France.

At a later period Freiherr von Zach reduced the length[1] of an equatorial degree to the sea-level and found it to be 56732 toises. He made use for this purpose of a second measurement at the equator, carried out by the Spaniards (assisted by Godin) between Cuenza and Mira, which embraced a length of 3° 26′ 52″ and gave 56768 toises as the length of a degree. Since the careful measurement of a degree in Peru—which put a final end to the opposition to the Newtonian theory of the figure of the earth as opposed to the view of Cassini, and proved to all the world as an undoubted fact, that the inhabitants of the earth did not live upon a perfectly spherical planet, but on one flattened at the poles; since then has also grown the universal desire for accurate knowledge of the dimensions of the earth, as from the amount of its deviation from the spherical form, we expect to form important conclusions as to the origin and development of our planet. Meantime the great progress made in methods of measurement and in instruments, combined with the beautiful results of the constantly-developing mathematical sciences, now promised the best results for new undertakings in reference to measurements of the earth.

Although the degree measurements carried out up to the middle of last century left no further doubt as to the spheroidal form of the earth, yet, as already said, the Peruvian measurement was the only one that had been carried out with the greatest possible accuracy. But on combining this arc with the French, Lapland, and other known measurements, only differing from it in point of accuracy, results differing much from each other were obtained. If we represent the polar and equatorial semi-diameters by the letters a and b, the quotient $\frac{a - b}{a}$ represents the amount of flattening. We thus obtain, by combining the Peruvian and the Lapland measurements, the value $\frac{1}{215}$, the Peruvian and French $\frac{1}{304}$, and lastly, the French and Lapland $\frac{1}{145}$. To understand in what way, by means of the data for the length of a degree, and the combining of the data for two or more different parts of the earth's surface, it would lead us to the amount of the flattening, requires considerable knowledge of the higher mathematical methods, and we must not, therefore, enter here on this part of the subject.

The great differences between the three values referred to, showed how untrustworthy were the measurements hitherto obtained, and this led to endeavours in many quarters to come nearer to the truth by better measurements. Lacaille availed himself in 1750 of his stay at the Cape of Good Hope to carry out the measurement of a degree, and found for S. lat. 33° 18′ 30″,

[1] Continued from p. 558.

[1] This probably refers to the reduction by the Baron de Zach of the observations made in the Peruvian operations.—*Mon. Corresp.*, xxvi., p. 52.

the length to be 57037 toises; and if this measurement was not carried out with the greatest care, since Lacaille could only devote two months to it, yet it was so far of no small importance, that it was the first which had been effected in the southern hemisphere. In the years 1751–53, Boscovich and Le Maire carried out a triangulation in the States of the Church, in 40° N. lat., and found the length of the degree to be 56973 toises. A degree-measurement made in the plains of Turin in 1768, between Andrate and Mondovi, gave for 44° 44′, N. lat., a degree length of 57024 toises. Mention should be made also of a series of extended measurements in Austria; and we may remark that most of the operations above referred to, as well as some of the following, were undertaken at Boscovich's instigation, while the Austrian operations were initiated and carried out by the Jesuit Liesganig. He found the length of a degree for N. lat. 48° 43′ to be 57086 toises, and for 45° 57′, 56881 toises. It may be seen from a comparison of these two values that, notwithstanding the small difference of latitude, they indicate a flattening at the poles, and if the calculation based upon these values alone give an anomalous result, it must be ascribed to the much too small difference of latitude. Indeed, errors of measurement were subsequently found in them. Finally we have to mention as belonging to this period the measurements after the old method which were carried out by direct measurement of a long meridian distance by means of a surveyor's chain. The one was in America, on the plains of Pennsylvania, by Mason and Dixon [1] over a distance of 1⅛ degree, and it gave, in N. lat 39° 11′ 56″, 56888 toises for the degree-length. The second measurement of this kind was in Bengal in 1790, by Burrow and Dalby, and it resulted in giving 56725 toises as the length of degree in N. lat. 23° 18′.

During the first forty years of the latter half of the eighteenth century a great number of geodetic operations were undertaken in various parts of the earth, and it was sought by various combinations of these measurements to ascertain the amount of polar flattening; but it was soon found that, with the exception of the Peruvian undertaking, they were too full of errors to yield a satisfactory result. The scientific men of that time soon became convinced of this drawback, and efforts were made by various academies not only to discover improved methods of measurement, but also by offering prizes to induce mechanics to perfect instruments, more especially the chronometer, so indispensable to astronomical observations. Both courses were followed with good results, and by English mechanics especially astronomical and geodetic instruments of measurement were brought to a high degree of perfection.

Strange though it may seem, France, with her revolutionary troubles coming fast upon her, was the first to commence the subsequent highly accurate geodetic operations. The multiplicity of units of measure had at this time reached its *ne plus ultra*. Not only each little territory, each separate province, but often each town had its own peculiar measure of length; and the case was nearly as bad with regard to weight, endless difficulties and disputes being the result. It was first resolved in 1790, in the French National Assembly, to come to an understanding with England on the length of the seconds pendulum, but after a year the French *savants* declared that, seeing that the seconds pendulum would be of different lengths at different parts of the earth, it would be more advantageous to adopt a given measure of the earth itself as unity, and that as such the ten-millionth part of the earth's quadrant should be taken. But, to settle this point definitely, it was necessary to measure a long arc of meridian with the greatest possible accuracy, and accordingly, March 30, 1791, it was decided to measure the meridian arc between Dunkirk and Barcelona, from which the length of the quadrant and its ten-millionth part, the metre, could be inferred. After the length of the seconds pendulum in France had been accurately observed, measuring operations were at once commenced, and thus began the great geodetic operation in France, afterwards carried on to the Balearic Isles, and in our own time but little surpassed. Notwithstanding revolutionary storms the operations were carried on and with unvarying accuracy. This measurement, effected by the method of triangulation, consisted of 120 triangles, connecting the two points Dunkirk and Montjouy, near Barcelona. The length of the arc between the two points was found to be 551584 toises.

There were also three intermediate points determined astronomically, and in order that the amount of the earth's oblateness might be inferred from this measurement alone, on Mechain's representation it was carried to the Balearic Isles, and thus the middle point of the measured arc coincided approximately with the middle point of the earth's quadrant. This extension was carried out in the years 1806-8 by Biot and Arago. The entire measured arc had now an amplitude of 12° 22′ 13·44″, the length being 705,188·8 toises, and the final result for the length of a meridian degree at 45° N. lat. was 57047 toises. It is characteristic of that time that in order to obtain the length of the metre, the conclusion of this measurement, which was undertaken for this purpose, was not waited for; but a preliminary metre measure was obtained from the results of the Peruvian, the Lapland, and the old French measurements, equal to 443·443 lines of the toise of Peru. As the results of the first-measured distance, Dunkirk to Barcelona, were known in 1797, the length was changed to 443·296 lines, and two platinum rods of that length (at a temperature of 0° C.) were prepared as standard measure, one of which was deposited in the Archives of the Republic, and the other in the Paris Observatory; two copies of these in steel served as the normal measure. On December 10, 1799, the Metre was instated as the legal measure in France, while in England the length of the seconds pendulum in the latitude of London remained as the unit of measure.[1] But the original object of the great French degree measurement, to obtain a natural measure of length, was not attained, and it is erroneous to imagine that the metre is in reality exactly the ten-millionth part of the earth's quadrant; for the length of the metre was, in subsequent degree measurements, ascertained more accurately and differently. But what was then attained was more accurate information as to the extent of the earth's oblateness.

Simultaneously with these French operations was the measurement of a degree in England, which was carried out with extreme accuracy in the year 1784, with the view of a general triangulation of the country; the measurements were made by Gen. Roy, with an accuracy not previously attained. While the angles were observed with theodolites constructed with the greatest accuracy, Roy effected the measurement of a base line with long glass tubes. Again, in the years 1800–2, was a similar geodetic operation undertaken, for the purpose of measuring a degree; the result was that for latitude 51° 20′ 54″ the length of the degree was found to be 57180 toises, and for 52° 50′ 29″·8, 57017 toises The great ellipticity of the earth resulting from these numbers gave rise to the idea that the measurements were inaccurate; but it was considered later on that mountain-masses must have exercised a disturbing influence on the plummet, and that the error must be due to this cause. In the years 1801–3 a new degree measurement under the polar circle was carried out by Svanberg and Ofverbom, the results of which, determined with great care, proved the inaccuracy of the earlier Lapland measurements by Maupertuis. For lat. 66° 20′ 12″ the length of the degree was found to be 57209 toises. The operations of Major Lambton in the East Indies, not hitherto surpassed in extent, were begun in 1802, and, as the final result, the length of the degree was found in four different places between 8° and 18° N. lat.

After so great a series of degree measurements obtained at so many different parts of the earth, it was now endeavoured, on the strictest mathematical principles, to submit them all to calculation and test their accuracy. The result was that the great French, the second north polar measurement, and the last measurement carried out in England, were shown to be of such a degree of accuracy as was needed to permit of a certain determination of the figure of the earth. All other measurements had to be cast aside as inadequate; in most of them the sources of error were pointed out, and the degree of accuracy noted, but as factors in the working-out of the final results, they could not stand. In the majority of the measurements of the latter kind the sources of error belonged mainly to two classes. The one was the rude, unsatisfactory construction of the geodetic instruments; the other concerned the astronomical part of the operation, and consisted not only in the want of accuracy in the instruments, but chiefly—and this reproach touched the greater part of the *savants* concerned—in the ignorance of the use of astronomical instruments, for of the majority it is certain that before they began their geodetic measurements, they never had an astronomical instrument in their hands.

[1] The measurement by Mason and Dixon was made with deal rods.

[1] The seconds pendulum never was the unit of length in England. But in the Act of 1824 legalising the "standard yard," reference is made to the seconds pendulum, and the length of the latter (vibrating in London) is given in inches of the standard yard, with the intention that should the standard yard be destroyed, it might through the seconds pendulum be restored.

At this period, when it was resolved to overhaul the accumulated material, subjecting to further mathematical treatment what was valuable, and leaving unregarded what was faulty, then it was resolved to work out the problem theoretically in so clear and accurate a manner as to be worthy of the high standpoint of mathematical and natural science. Thus it appeared, above all, necessary to get rid of the inequalities of the earth's surface, to reduce all measurements to an ideal form of surface, the most suitable being that (according to Gauss's definition) which the still water of the ocean would assume if it covered the whole surface of the earth. It is also defined as a surface which is at every point at right angles to the direction of a free-falling body. But in order that this ideal surface might be observed in reality, researches on a large scale would have to be undertaken on the tides, in order to obtain a mean water-level. At that time, also, theory fell upon a new and suitable method of ascertaining the amount of the earth's oblation, in the theoretical perfection of the long-known phenomena of precession and nutation. And while both the theory and the practical methods of measurement were being carried to a high degree of perfection, in spite of the political storms in nearly all European nations, new preparations were made to find a worthy solution of this problem by means of the newest and best acquisitions of science. Especially now was there a people who not only emulated the noble efforts of other nations, but whose *savants*, the first of their time, were able soon to place themselves, through their thoughtful and ingenious researches, supported by a liberal people, at the head of the efforts made by nearly all civilised nations to obtain a knowledge of the truth—this was the German.

In what follows we shall explain these acquisitions as to a knowledge of our earth which have been made in our century, and in great part by our people.

The operations which have been undertaken during the present century for the purpose of obtaining an accurate idea of the figure of the earth and its dimensions, have by no means been confined, as nearly all the early operations were, to the carrying out of degree measurements; but even in the earlier periods a method already mentioned was brought prominently forward, which would not only show the form of the surface, but from which it was also expected that conclusions could be drawn as to the internal physical condition of the crust of the earth, and the manner in which the mass under the surface is distributed—we refer to pendulum measurements. We have already seen how Richer found a difference of lengths of the pendulum in Paris and Cayenne, and after Bouguer in Peru and Lacaille at the Cape had made similar observations, an idea was obtained of the law of variation of the lengths of the pendulum at different latitudes. It was soon seen from this that the differences in length of the pendulum at the extreme points, the pole and the equator, would only be very small, and that the very nicest observations would be necessary to allow conclusions to be drawn as to the form of the earth. The Spaniards were the next who, in two ships of war, carried out measurements in very different parts of the earth, but which unfortunately proved not to be of the requisite accuracy. Shortly afterwards, a new triangulation was undertaken in France, and while Laplace sought by it as far as possible to obtain data as to the oblateness of the earth, several other *savants*, especially Biot and Arago, carried on pendulum measurements along the meridian of the great degree-measurement (Dunkirk). In connection with this new triangulation, extended and exceedingly accurate measurements of longitude were carried out. In England efforts were now made to utilise triangulation for both methods of measuring degrees, and now, especially in the southern hemisphere, pendulum observations were accomplished on a scale and with an accuracy such as had not previously been known. These observations established the fact that the southern hemisphere had no essentially different condition from the northern hemisphere. There was used for this purpose a very delicate pendulum apparatus, the "Reversible Pendulum," the inventor of which was Bohnenberger, a German. From 1822 to 1824 such observations were carried out at many coast stations as far north as the Arctic Ocean, embracing an extent of 93° of latitude.

Accurate methods of observation of this kind, as also very exact and ingeniously-constructed pendulum apparatus, were now invented and brought into use mainly by German astronomers; Bessell especially has done lasting service in this respect, his method, perfected with the greatest ingenuity, being still fruitful in results.

The principle of this method, viz., from various pendulum measurements to obtain the figure of the earth—cannot be here explained, on account of the mathematical principles involved, and we can only give some of the results obtained from the above-mentioned measurements. The first Spanish measurements gave the oblateness of the earth as $\frac{1}{300}$; the French, $\frac{1}{315}$; the English results varied between $\frac{1}{230}$ and $\frac{1}{308}$. The value obtained from the earliest mentioned astronomical observations (precesssion and nutation) was $\frac{1}{299}$. The difference of the results obtained by means of pendulum measurements could not be ascribed to erroneous observations, but rather to the unequal density of the earth, as was shown quite clearly by later measurements. It was sought, especially in Germany, to discover the amount of this disturbing influence, and to obtain observations free from these disturbances. Already, in the year 1806, a German published the thus improved results of the measurements, and obtained from the various methods of observation the following nearly accordant results:—Newton's theory gave $\frac{1}{304}$; precession and nutation, $\frac{1}{304}$; the theory of the moon's motion, $\frac{1}{304}$; pendulum measurements, $\frac{1}{321}$; and

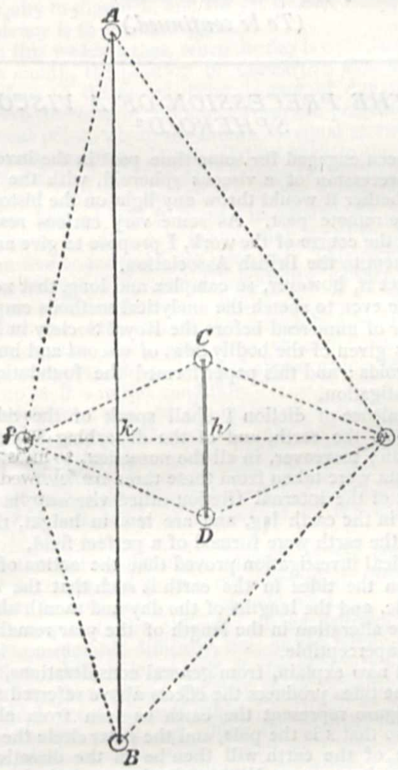


Fig. 2.

degree-measurements, $\frac{1}{304}$. Laplace and Sabine deduced, according to the newer mathematical methods, the most probable value of the oblateness of the earth, from all these results, and found, the former $\frac{1}{306}$, the latter $\frac{1}{289}$. German *savants* also repeated this calculation, and obtained, certainly not exactly the same, though very similar results; but more accurate results could only be based on more delicate measurements.

These more accurate measurements were soon carried out, mainly in Germany and Russia. Gauss, in 1821–24, measured the distance between Göttingen and Altona, and obtained for latitude, 52° 2′ 17″, 57126 toises as the length of the degree. For this purpose he had adopted greatly improved methods of observation. Schumacher made a new measurement in Denmark, and found for 54° 8′ 13″·5 the degree-length to be 57092 toises.

It may, perhaps, be interesting to show here what the improvement was which had been introduced at that time into these operations in Germany; it was the method of enlargement of the base-line, whereby the very difficult labour of base-line measurement was considerably reduced and more accurate measurements thereby became possible. For example, let the length A B be the distance to be measured, but with only a small plain surface on which a base-line can be measured (Fig. 2). The small base-line C D is then measured with the greatest possible accuracy, and the angle formed at C and D in the directions *e* and *f* are obtained. Thus the two triangles C D *e* and C D *f* are completely given; for in each is a side (C D) and the two adjacent angles known. But thus also are their heights *e h* and *f h* given, and these added give the side *e f* in the great triangles. From *e* and *f* the two triangles formed towards A and B are measured, and this gives completely the two triangles A *f e* and B *f e*, as also their heights A *k* and B *k*, which added give the distance sought.[1] It will be seen at once that this method offers great advantages, especially if it be possible to obtain with the greatest accuracy the small base-line. This latter condition Bessel fulfilled at first to an astonishing degree, as he, by the introduction of a base-apparatus, attained the greatest accuracy. Bessel and Baeyer accomplished a degree-measurement between Memel and Trunz in 1831–36. They obtained for the mean latitude of the measured arc (54° 58′ 25″·5) a degree-length of 57142 toises. An operation was carried out by Maclear between 1836 and 1848 at the Cape of Good Hope, by which for south latitude 35° 43′ 20″ a degree-length of 56933 toises was obtained.

(*To be continued.*)

ON THE PRECESSION OF A VISCOUS SPHEROID[2]

I HAVE been engaged for some time past in the investigation of the precession of a viscous spheroid, with the intention of seeing whether it would throw any light on the history of the earth in the remote past. As some very curious results have appeared in the course of the work, I propose to give an account of part of them to the British Association.

The subject is, however, so complex and long, that no attempt will be made even to sketch the analytical methods employed.

In a paper of mine read before the Royal Society in May last, a theory was given of the bodily tides of viscous and imperfectly elastic spheroids; and this paper formed the foundation of the present investigation.

For convenience of diction I shall speak of the tidally disturbed body as the earth, and of the disturbing bodies as the moon and sun; moreover, in all the numerical applications, the necessary data were taken from these three bodies.

The effect of the internal friction called viscosity, is that the bodily tides in the earth lag, and are less in height, than they would be if the earth were formed of a perfect fluid.

An analytical investigation proved that the action of the sun and moon on the tides in the earth is such that the obliquity to the ecliptic, and the lengths of the day and month all become variable; the alteration in the length of the year remains, however, quite imperceptible.

But I will now explain, from general considerations, how the lagging of the tides produces the effects above referred to.

Let the figure represent the earth as seen from above the south pole, so that S is the pole, and the outer circle the equator. The rotation of the earth will then be in the direction of the curved arrow close to S. Within the larger circle is a smaller concentric one, one-half of which is drawn with a full line, and the other half with a dotted line. The full line semicircle is part of a small circle in S. latitude and the dotted one part of another small circle in the same latitude, but to the north of the equator. Generally, dotted lines indicate parts which are behind the plane of the paper.

It will make the explanation somewhat simpler, if we suppose the tides to be raised by a moon and antimoon diametrically opposite to one another; this, as is well known, is a justifiable modification of the true state of the case.

Then let M and M′ be the projections of the moon and antimoon on to the terrestrial sphere.

If the substance of the earth were a perfect fluid, or were perfectly elastic, the apices of the tidal spheroid would be at M and M′. If, however, there be internal friction, the tides will lag, and we may suppose the apices of the spheroid to be at T and T′. In order to make the subject more intelligible, the tidal protuberances are then supposed to be replaced by two equal heavy particles T and T′, which are instantaneously rigidly connected with the earth. This same idea was, I believe, made use of by Delaunay, in considering the ocean tidal friction.

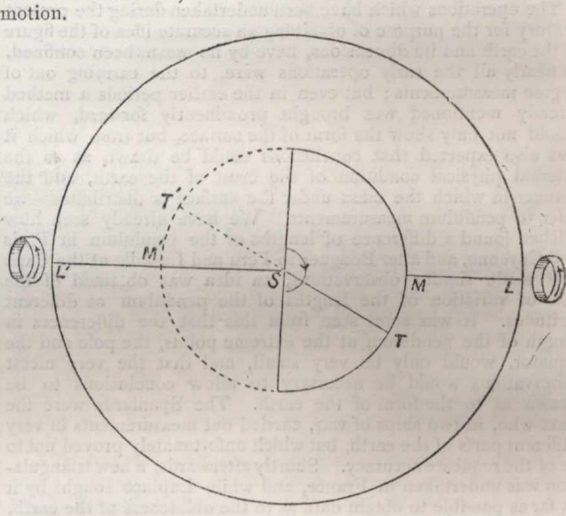
Then the attraction of the moon on T is greater than on T′; and that of the antimoon on T′ greater than on T. Hence, besides equal and opposite forces acting at the earth's centre, directly towards M and M′, there are small forces (varying as the square of the tide generating force) acting in the directions T M and T′ M′.

We will consider the effect on the obliquity first. These two forces, T M, T′M′, clearly cause a couple about the axis L L′ in the equator, which lies in the same meridian as the moon. The couple is indicated by the curved arrows at L and L′. Now, if the effects of this couple be compounded with the existing rotation of the earth, according to the principle of the gyroscope, it is clear that the south pole S tends to approach M and the north pole to approach M′. Hence supposing the moon to move in the ecliptic, the inclination of the earth's axis to the ecliptic diminishes; in other words, the obliquity of the ecliptic increases.

Next with regard to tidal friction; the forces T M and T′M′ produce a couple about the earth's axis, S, which tends to retard the earth's rotation.

Lastly, since action and reaction are equal and opposite, and since the moon and antimoon produce the forces T M, T′ M′ on the earth, therefore the earth must cause forces on the moon and antimoon in the directions M T and M′ T′. These forces are in the same direction as the moon's orbital motion; hence the moon's linear velocity is augmented. The consequence of this is that her distance from the earth is increased, and with that increase comes an increase of periodic time round the earth.

The consequences of the lagging of the earth-tides, therefore, are an increase of the obliquity to the ecliptic, a retardation of the earth's rotation, and a retardation of the moon's mean motion.



In this general explanation it is assumed that the lagging tides are exactly the same as though the earth were perfectly fluid, and as though the tide-*raising* moon were more advanced in her orbit than the true moon, whilst the moon which attracts the tidal protuberances was the true moon. That is to say, it is assumed that the tides raised are exactly the same as though the earth were a perfect fluid, save that the time of high tide is late, and that the tides are reduced in height.

Now although this serves in a general way to explain the phenomena which result from the supposition of the earth's viscosity, yet it is by no means an accurate representation of the state of the case.

In fact the internal friction sifts out the whole tide-wave into its harmonic constituents, and allows the different constituents to be very differently affected as regards height and phase.

Thus the lagging tide-wave is not exactly such as the general explanation supposes, and the nearer does the spheroid approach to absolute rigidity the greater does the discrepancy become.

The general explanation is a very fair representation for moderate viscosities, but for large ones it is so far from correct that the tendency for the obliquity to vary may become nil, and for yet larger ones the obliquity may tend to decrease.

[1] It is here assumed that *f e* is at right angles to C D, and A B at right angles to *f e*. There is no necessity for this condition, and it could never actually occur.

[2] A paper read at the Dublin Meeting of the British Association, by G. H. Darwin, M.A., Fellow of Trinity College, Cambridge.

A complete analysis of this state of things for various obliquities and viscosities shows that there is a great variety of positions of dynamical equilibrium, some of which are stable and some unstable.

Although there is all this variety with respect to the change of the obliquity, yet the tidal friction always tends one way, namely, to stop the earth's rotation.

It has already been remarked in the general explanation that the effect on the moon is a force tangential to her orbit accelerating her linear motion, and thus indirectly retarding her angular motion. But it appears that for a very great degree of stiffness and for large inclinations of the earth's axis to the ecliptic, this force on the moon may be actually reversed; so that the retardation of the moon's motion may actually be replaced by an acceleration.

To a terrestrial observer, however, unconscious of the slackening of the earth's diurnal rotation, it would be indifferent whether the moon were undergoing true retardation or true acceleration, for in every case there would result an apparent acceleration of the moon's mean motion.

It is obvious from what has been said that we have the means of connecting the heights and lagging of the bodily tides in the earth with an apparent secular acceleration of the moon's mean motion. I have applied these ideas to the supposition that the moon has an apparent secular acceleration of 4″ per century, and I find that if the earth were a homogeneous viscous spheroid, then the moon must be undergoing a secular *retardation* of 3″·6 per century, while the earth (considered as a clock) must be losing 14 seconds in the same time. Under these circumstances the effective rigidity of the earth must be so great that the bodily diurnal and semi-diurnal tides would be quite insensible; the *bodily* fortnightly tide would, however, be so considerable that the *oceanic* fortnightly tide would be reduced to one-seventh of its theoretical amount on a rigid nucleus, and the time of high water would be accelerated by three days.

The supposition that the earth is a nearly perfectly elastic body leads to very different results, which, however, I must now pass over.

From this and various other considerations, I arrive at the conclusion that the earth has a very great effective rigidity, and that the apparent acceleration of the moon's motion affords no datum for determining the amount of tidal friction on the earth.

Sir William Thomson has made some interesting remarks about the probable age of the earth in connection with tidal friction, and he derived his estimate of the rate at which the diurnal rotation is slackening principally from the secular acceleration of the moon. He fully admitted that his data did not admit of precise results, but if I am correct in the present conclusion, it certainly appears that his argument must lose part of its force.

The investigation of the secular changes, which such a system would undergo, is surrounded by great mathematical difficulties, but I think that I have succeeded in surmounting them by methods partly analytical and partly arithmetical.

In a communication of the present kind it would be out of place to consider the methods employed, and I will therefore only speak of some of the results.

There are two standards by which we may judge of the viscosity in the present problem—first the ordinary one, in which it is asserted that it requires so many pounds of tangential stress to the square inch to shear an inch cube through so much in such and such a time; and secondly, when the viscosity is judged of by the amount by which the behaviour of the spheroid departs from that of a perfectly fluid one; a numerical value for this sort of measure is afforded by the angle by which the crest of the tidal spheroid precedes the moon, when the obliquity to the ecliptic is zero.

Now it appears that if the earth possessed a viscosity which was not at all great as estimated by the tidal standard, yet the materials of the earth, when considered in comparison with the substances which we know, would be found to be a substance of very great stiffness—stiffer than lead, and perhaps nearly as stiff as iron. I see, therefore, no adequate reason why some part of the changes, which will be considered presently, should not have taken place during geological history.

The problem was solved numerically for a degree of viscosity, which would make the changes proceed with nearly a maximum rapidity. Estimated by the tidal standard, this is neither a very great nor a very small viscosity, for the crest of the semi-diurnal tide precedes the moon by 17° 30′.

I found, then, that if the changes in the system are tracked back for fifty-six million years, we find the day reduced to six hours fifty minutes, the obliquity to the ecliptic 9° less than at present, and the moon's period round the earth reduced to one day fourteen hours.

This very short period for the moon indicates of course that her distance from the earth is small. As the moon goes on approaching the earth the problem becomes much more complex, and, for periods more remote than fifty-six million years ago, I abandoned the attempt to obtain a scale of times. The solution up to this point shows that the times requisite for these causes to produce such startling effects are well within the time which physicists have admitted to have elapsed since the earth existed.

From this point in the solution the parallel changes of the obliquity, day and month, were traced without reference to time.

It appears, then (still looking backwards in time), that the obliquity will only continue to diminish a little more beyond the point already reached; for, when the sidereal month has become equal to twice the day, there is no longer any tendency for the obliquity to diminish, and for yet smaller values of the month the tendency is to increase again.

From this we learn that, when the day is equal to or greater than half the month, the position of the earth's axis at right angles to the plane of the moon's orbit is one of dynamical stability. The whole decrease of obliquity from the present value back to the critical point, where the month is equal to twice the day, is 10°. From this point in the solution back to the initial state to which the earth and moon are tending, the obliquity to the plane of the lunar orbit was neglected. I then found that the limiting condition, beyond which it was impossible to go, was one in which the earth and moon are rotating, fixed together as a rigid body, in five hours and forty minutes. This condition was also found to be one of dynamical instability, so that, if the month had been a little shorter than the day, the moon must have fallen into the earth, but if the month had been a little longer than the day the moon must have receded from the earth, and have gone through the series of changes, which were traced backwards up to this initial condition.

This periodic time of the moon of five hours forty minutes corresponds to an interval of only 6,000 miles between the moon's centre and the earth's surface. Moreover, if the earth had been treated as heterogeneous instead of homogeneous, this interval between the primeval earth and moon would have been yet further diminished, as also would be the common periodic time.

The conclusion, therefore, appears to me almost irresistible that if the moon and earth were ever molten viscous bodies, then they once formed parts of a common mass.

With respect to the obliquity of the ecliptic, the question is one of considerable difficulty, but, on the whole, I incline to the view that, while a large part of the obliquity may be probably referred to these causes, yet that there remains an outstanding part which is not so explicable.

Besides the results, of which the outlines have been given, I have obtained some others which, as I believe, will aid in the formation of a modified edition of the nebular hypothesis—such as some of the changes to which an annular satellite would be subjected.

One of the collateral results, which appeared in considering the secular changes of such a system as the earth, moon, and sun, was that a large amount of heat would have been generated in the interior of the earth by means of friction. If, then, it is permissible to suppose that any considerable part of these changes had taken place during geological history, Sir William Thomson's problem of the secular cooling of the earth would require some modification.

The magnitude of the undertaking has not allowed me time as yet to apply these ideas to the questions of the eccentricity and inclination of the orbit of the satellite, nor to the cases of other planets besides the earth.

I think, however, that I see in Asaph Hall's wonderful discovery of the Martian satellites, a confirmation of this theory. Their extreme minuteness has, I think, preserved them as a standing memorial of the primitive period of rotation of that planet. The Uranian system, on the other hand, appears, at least at first sight, a stumbling-block.

It is easy to discern in the planetary system many *veræ causæ* which tend to change its configuration, but it is in general very hard to give any quantitative estimate of their effects.

It will have been seen that in the investigation of which I have given an imperfect account, free scope has been given to speculation, but that speculation has been governed and directed in every case by appeal to the numerical results of a dynamical problem, and I therefore submit that it stands on a different footing from the numerous general speculations to which the nebular hypothesis has given rise.

NATURAL SCIENCE IN HUNGARY IN THE LAST TEN YEARS

FEW of the readers of NATURE are aware that Hungary has of late years become the scene of active efforts in science, and especially the natural sciences.

The following sketch of an article, written by Mr. Coloman Szily, member of the Academy of Sciences, and Professor of Physics at the Polytechnic of Budapest, and published in the *Budapesti Szemli* (*Budapest Review*), may therefore not be altogether without interest.

The first active sign of native scientific life in this direction in Hungary was the founding of the Academy of Sciences in 1830. Up to that time there were single men of science, but no organised scientific life. But the chief object aimed at by the Academy was the cultivation of the national language, and the excessive zeal with which it pursued this aim did much harm to the cause of the natural sciences here. An erroneous attempt to substitute purely Hungarian words for the mathematical and other scientific expressions universally accepted elsewhere, threw great obstacles in the way of the progress of the natural sciences in our country.

This and other errors soon brought a reaction.

The "General Assembly of Physicians and Naturalists" was soon started amid general enthusiasm. The meetings of the assembly were held yearly in different cities from 1841 up to 1848, and then renewed in 1863, after a cessation of fifteen years, caused by political events. A yearly report was issued, containing the various papers read at the meeting, as well as an account of physical characteristics of the district in which it was held.

Far more brilliant was the success reached in the cultivation and promulgation of the natural sciences by the "Termíszettudományi Társulat" (Society of Natural Sciences), which was started at the same time. By 1848 the number of the members rose to more than 400. Its first yearly report was then issued, and a contract made for the starting of a scientific magazine, entitled "Magyar Iris." This powerful start, which was made independently of the Academy, and which proved of ever-increasing importance, could not remain without effect. In 1844 a proposition that the two classes of mathematicians and naturalists might hold their meetings and carry on their financial and other operations separately from the rest was partially accepted. Some years later (1861) it only required a single lecture (of Prof. Joseph Izabo's) to bring the whole Academy to pronounce a resolution against the attempt to Magyarise the nomenclature.

In the meantime the events of 1848-49 were followed by a long period of despotism, which tended to paralyze all attempts at association. The most distinguished men of science were forced off the field of action, the Academy could hold no meetings, the Society of Natural Sciences was on the brink of dissolution, its members were scattered, its collections had to be given away for lack of funds to pay the rent of the accommodation needed for them, and it was barely able within ten years to publish two of its yearly reports.

Between 1850 and 1860 the nation began to breathe more freely. Its very first efforts were turned towards the advancement of science. A very fine building was raised for the Academy, and its capital considerably increased, by means of private subscriptions. It thus became able to do much more than it did previously, both for the improvement of our native language and for the cultivation of the various branches of science. In 1860 it appointed a committee of mathematicians and naturalists, whose duty it was to explore the whole country and give an account of its natural and technical features. Ever since the year 1868 Government has devoted the yearly sum of 5,000 florins to the furtherance of the labours of this committee. But this sum frequently proves insignificant. Fourteen volumes of the publications of the committee, entitled, "Scientific Treatises Relating to Home Topics" (Termiszettudományi közleménysk, vonatkozólag a hazai viszonyokra), edited by its secretary, Prof. Joseph Szabo, have already appeared. At the same time with this the Academy started a second series of periodicals for publishing mathematical and scientific treatises, not confined to topics within the limits of our country. They appeared yearly from 1860 to 1867, six volumes in all.

After the renewal of constitutional life in 1867 our naturalists were also filled with a strikingly-increased zeal for labour. The Academy has up to the present day issued thirty-two volumes in all. Many articles, treating of the original researches of our naturalists, have appeared in foreign periodicals. The meetings of the department of natural sciences in the Academy have of late borne witness to a truly diligent and scientific spirit, there scarcely being one in which less than six or eight treatises have been presented upon topics of original research.

In 1868, though no preliminary agreement had taken place between the two institutions, the department of Naturalists of the Academy, having arrived at the conviction that the popularising of the natural sciences was not their calling, abandoned the attempt, and decided that they should henceforth direct their efforts solely to the cultivation of the sciences and the making of scientific researches in our country, while the Society of Natural Sciences took upon itself the spreading and popularising of them. To this end the Society started a monthly periodical; the number of members of the Society rising in the very first year of its existence from 600 to 1,600, the second year to 2,200, until at present it borders upon 4,800. A short time later the Society began to arrange lectures connected with experiments for the benefit of the public. These lectures have now been kept up for eight years, and the large lecture-hall in which they have been held has always been crowded with hearers. As long as it was possible these lectures were published in the *Journal of Natural Sciences*; now, however, they appear in the form of a new series of publications under the title, "Collection of Popular Treatises upon Topics pertaining to the Natural Sciences." In 1872 the Society again started a new undertaking, namely, the translating into Hungarian and issuing of foreign works of a popular kind upon the natural sciences. The result of this undertaking, which has enjoyed the support of 1,500 subscribers, as well as a yearly aid from the Academy, has up to the present time been the issuing of twelve volumes, such as "Geologie der Gegenwart," by von Cotta; Darwin's "Origin of Species," Helmholtz's "Populäre Vorlesungen," Huxley's "Lessons in Physiology," Lubbock's "Prehistoric Times," Proctor's "Other Worlds than Ours," and Tyndall's "Heat as Motion." An Anthology has also been compiled, containing a treatise from every scientific author who has contributed to the popularising of the natural sciences, from the time of Arago and Humboldt downwards, and a volume containing the complete works of the late Julius Greguss. These books always find a large number of purchasers.

But there is another branch of activity which is of more importance, perhaps, than all those, namely, its efforts for the encouragement of original research. From 1870 the legislature of our country, in appreciation of the labours of the Society, has voted a yearly sum at first of 5,000, and afterwards of 4,000 florins, for the promotion of such researches as stand most nearly connected with the interest of our country, and the publication of an account of the same. In this series of publications the following have appeared up to the present time:—"The Rise and Fall of Tide in Fiume Bay," by Emile Stahlberger; "The Ice Grotto of Dobsina," by Dr. Joseph Alexander Krenner; "Sketch of the Ligaridas of Hungary," by Dr. Géza Horváth; "The Spiders of Hungary: Vol. I. General Part," by Otto Hermann; "The Iron Ores and Iron Products of Hungary, with Special Reference to the Principal Chemical and Physical Qualities of the Iron," by Anton Kerpsly.

In searching out and making known the physical characteristics of our country, the Society of Geologists (Magyar földtani társulat), founded in 1851, can also boast considerable merits. The Society also publishes a monthly periodical under the title of *Geological Review*.

In 1872 there was also a Geographical Society founded, which did not aim so much at the advancement of geographical researches as at keeping the public informed of any progress made on this field by means of a two-monthly review.

This active interest in the natural sciences is not confined only to our capital, but has taken root throughout the country. To prove this we have but to note the interest manifested in the labours of the Society of Natural Sciences in all grades of society everywhere, and the fact that societies under various titles, but all aiming at the cultivation of the natural sciences, were formed in many of our larger towns as early as between 1850-60, and more still of late years.

There is another circumstance worthy of notice, which affords perhaps a clearer illustration of the general interest in the natural sciences, and diligence on the field of the same, than even the rise and progress of the various societies already mentioned. It is the constant increase in the number of scientific periodicals, of which the following are already in circulation, besides the publications of the academy and the three societies of which we have spoken:—

(1) *Nature* (*Természet*), which appears every two weeks, and is now in the tenth year of its existence; (2) *Polytechnical Journal* (*Müesseteni lapok*), a monthly periodical for mathematical and technical treatises; (3) *Reports of the Meetings of the Society of Physicians and Naturalists in Klausenburg*, started in 1876; (4) *Botanical Papers*, a monthly, started in 1877; (5) *Leaves of Natural History*, a quarterly periodical, started in 1877, and edited by the Hungary National Museum. At the end of each volume is a review of the contents, written either in French or German, in order to be understood in foreign countries, and containing either a complete translation or an abstract of all the more important articles in the volume; (6) *Magazine of Natural Sciences*, a two-monthly periodical, started in 1877, and edited in Temesvár by the Association of Naturalists of South Hungary.

To recapitulate briefly what has been said, there are in Budapest, besides the department of natural sciences in the Academy, three societies, the object of which is exclusively the cultivation of the natural sciences, and one of which is of dimensions that, considering the total number of inhabitants, are scarcely equalled anywhere in Europe. In other parts of our country we have in all six societies of naturalists, and there are in circulation nine scientific periodicals, not one of which enjoys any aid from the State, all being supported exclusively by the readers.

These societies, however, are not the sources of science, but merely, so to speak, its conducting pipes. The sources of it spring from the collections and laboratories of the universities. The progress made here of late years has also been considerable. Our university, which was greatly neglected up to the year 1850, has of late taken such a start forward that its condition of ten years ago is not to be compared to its present state. The number of professors' chairs pertaining to the natural sciences (including those of the University and the Polytechnic of Budapest, and the University of Klausenburg) now amounts to three times what it was previously. The greater part of these chairs are occupied by young professors, who have been educated at foreign universities, under the instruction of the most distinguished men of science. Each one has a respectable sum of money at his disposal for cabinet and laboratory purposes. Separate buildings have been raised and equipped as institutes of natural sciences, such as the Chemical and Physiological Institute, connected with the University, which stands under the direction of Prof. Charles Than, and already enjoys a wide-spread renown in Europe. The buildings intended for a clinic are nearly completed, and soon the Institute for Physics, the Mineralogical and Zoological Institute, and the buildings of the Polytechnic will be raised in their turn. The number of the students of philosophy increases with striking rapidity, and some of the most distinguished of these are yearly sent out to foreign universities at state expense, with a view to their afterwards accepting appointments in their own country.

All these things clearly show that Hungary has within the last ten years made striking progress in the field of natural sciences, so that the distance which separated her in this respect from her western neighbours, has grown palpably less. Would that providence should permit her to continue the work thus begun! J. M. A.

PUBLISHERS' ANNOUNCEMENTS

MESSRS. WILLIAM BLACKWOOD AND SONS announce "The Transvaal of To-day: War, Witchchraft, Sport, and Spoils in South Africa," by Capt. Alfred Aylward, late Commandant, Transvaal Republic. Capt. Aylward commanded the Leydenberg volunteers on the Boers' frontier until the Republic was annexed by the British, and from the prominent part he played in Transvaal politics, as well as from his knowledge of the country and his experience of Kaffir warfare, his book may be expected to throw some light upon questions that are now attracting public attention.

MESSRS. KEGAN PAUL AND CO. announce that they will add to their International Scientific Series in October, a "History of the Growth of the Steam Engine," by R. H Thurston, Professor of Mechanical Engineering in the Stevens Institute of Technology, Brooklyn. The volume will give a history of the discoveries, inventions, and many ingenious experiments that gradually led to the success of the steam engine in the last century, and will be illustrated with fifteen portraits and 148 engravings on wood. This work will be followed speedily by Prof. Huxley's volume on "The Crayfish: an Introduction to the Study of Zoology." Prof. Alexander. Bain's "Education as a Science;" and Dr. H. Charlton Bastian's treatise on "The Brain as an Organ of Mind." Two translations from the French will also be included in the series before Christmas, namely, "The Human Race," by Prof. A. de Quatrefages, and "The Brain and its Functions," by Dr. J. Luys. The same publishers announce "The Geology of Ireland," by G. Henry Kinahan, M.R.I.A., &c., of Her Majesty's Geological Survey; with numerous illustrations and a geological map of Ireland; to be ready in October. "Etna; a History of the Mountain and its Eruptions," with maps and illustrations, by G. F. Rodwell, F.R.A.S., F.C.S. "Flowers and their Unbidden Guests," by Dr. A. Kerner, Professor of Botany in the University of Innsbruck; translation edited by W. Ogle, M.A., M.B.; with illustrations. "Mind in the Lower Animals in Health and Disease," by W. Lunder Lindsay, M.D., F.R.S.E., F.L.S., Hon. Member of the New Zealand Institute; Vol. I. Mind in Health; Vol. II. Mind in Disease. "History of the Evolution of Man," by Prof. Ernst Haeckel, Author of "The History of Creation;" two vols., with numerous illustrations. "Gaur: its Ruins and Inscriptions," by the Late John Henry Ravenshaw, B.C.S.; edited by his Widow; with forty photographic illustrations, and fourteen facsimiles of inscriptions. These three last books will be ready for publication in November.

MESSRS. REEVE AND CO. have just published the fourth edition of Bentham's "Handbook of the British Flora." The principal alteration in this edition has been the giving the first place to the Latin names of the genera and species. The attempt made in previous editions to establish an English scientific nomenclature, in imitation of the French and German ones introduced into several standard Continental floras, has, we regret to say, proved a failure.

MESSRS. MACMILLAN AND CO. will publish, in the course of the season, the following new books and new editions:—"Sport and Work on the Nepaul Frontier, or Twelve Years' Sporting Reminiscences of an Indigo Planter," by "Maori," with Map and Illustrations; "Coal, its History and its Uses," by Profs. Green, Miall, Thorpe, Rücker, and Marshall, of the Yorkshire College, Leeds, with numerous Illustrations, 8vo; "Chemistry: a Treatise on," by Profs. Roscoe and Schorlemmer, of the Owens College, Manchester, vol. ii., "Metals," part I, 8vo; "Gegenbaur's Comparative Anatomy," a Translation revised with Preface by Prof. E. Ray Lankester, F.R.S., with numerous Illustrations, medium 8vo; "A System of Medicine," Edited by J. Russell Reynolds, M.D., F.R.S., vol. v., completing the work; "Modern Realism Examined," by the late Prof. Herbert, Edited by Prof. James M. Hodgson; "Science Lectures at South Kensington," vol. ii., crown 8vo; "The Theory of Sound," by Lord Rayleigh, M.A., F.R.S., vol. iii., 8vo; "Heat," an Elementary Treatise, by P. G. Tait, Professor of Natural Philosophy at Edinburgh, with numerous illustrations, crown 8vo; "Sound" (NATURE series): a series of simple, entertaining, and inexpensive experiments in the phenomena of sound, for the use of students of every age, by Alfred Marshall Mayer, Professor of Physics in the Stevens Institute of Technology, &c., &c.; "Ismailia" by Sir Samuel W. Baker, Pasha, a narrative of the expedition to Central Africa for the suppression of the slave trade, organised by Ismail, Khedive of Egypt, with maps, portraits, and numerous illustrations by Zwecker and Durand, new and cheaper edition, one vol., crown 8vo; "A Ramble Round the World," by M. le Baron de Hübner, formerly Ambassador and Minister, translated by Lady Herbert, new and cheaper edition with numerous illustrations, crown 8vo.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

It has been determined to give a course of lectures on Agricultural Chemistry at the High School, Kelso, and the first of the series, at which Sir Geo. Douglas presided, was delivered on the 19th inst. by Mr. Wm. Ackroyd, from the Science Schools, South Kensington.

Prof. Marignac, after having filled for a period of thirty-eight years the Chair of Chemistry at the University of Geneva, with the greatest distinction, has recently resigned. He has been replaced in the chair by Prof. Graebe, from the University of Königsberg.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale de Belgique, No. 6, 1878.—In this number M. Spring communicates some interesting preliminary results obtained from enormous pressure exerted on the fine powder of some solid bodies (nitrates of sodium and potassium, sawdust, chalk, &c.). Homogeneous blocks were thus produced, harder and more resistant than if they had been obtained by fusion. Two of them were found translucid, and did not present the least vestige of the particles which were united to form them. A force of 40,000 atmospheres was employed, but of this—the friction of the apparatus being enormous—only about half was disposable pressure.—Some experiments with regard to the action of atropine and physostigmine on the heart of the frog, and in connection with the physiology of the vagus nerve, are described by MM. Putzeys and Swaen.—From a careful examination of some Chilopodan Myriapoda, especially several large Scolopendra from Java, M. Jules M'Leod has succeeded in detecting the true venomous glands in the substance of the forcipular foot-jaws. The structure of these organs is detailed.—M. De Heen has sought to determine the viscosity of liquids from the retardation of a small spheroidal glass runner allowed to descend in a tube of 3 m. height, filled with different liquids. He deduces numerical values, which he considers to represent the *coefficients of fluidity* of the liquid. Objection is taken, however, to his formula by MM. Valerius and Montigny, who report on the paper.

The *Zeitschrift der oesterreichischen Gesellschaft für Meteorologie* (vol. xiii., Nos. 19 and 20) contains the following more important papers :—On the conditions of temperature in the United States, by Dr. A. Woeikoff.—Appeal to meteorologists to make observations of clouds, by Dr. H. Hildebrandsson.—On a new balance thermograph, by Dr. A. Sprung.—On the estimation of ozone, by Albert Lewy.—On the origin of atmospheric ozone, by Dr. Leuder.—On the climate of Queensland, by Herr Hann.—On the local winds of the Hiberian peninsula, by Herr Hellmann.

Kosmos, August.—The inhabitants of the planets, by C. du Prel.—Harvey on generation: a study, by W. Preyer.—Insects as unconscious florists, part 2, by Hermann Müller; with wood-cuts illustrating potentilla, ranunculus, lychnis, daphne, crocus, &c.—On the struggle of languages in the Valais, by Alexander Maurer.

Zeitschrift für wissenschaftliche Zoologie, vol. xxxi. part 1.—On the Siphonophora (Hydrozoa) of deep water, with descriptions of new species of Rhizophyda and Bathyphysa, by Th. Studer; three plates.—Contribution to the morphology of Oxytrichidæ, by V. Sterki; one plate.—On *Trichaster elegans*, by H. Ludwig.—On Loxosema, by O. Schmidt.—On the Tomopteridæ, by F. Vejdovsky; two plates.—Contribution on the Caprellæ, by A. Gamroth; three plates.

Morphologisches Jahrbuch, vol. iv. part 2.—Contributions on the formation, fertilisation, and segmentation of the animal ovum, by Oscar Hertwig, part 3, continuing and confirming his previous researches; three plates, with figures of ova of Nausithoë, Physophora, Helix, Unio, Tellina, Sagitta, and many others.—On fossil vertebræ: the Cestracionts, by C. Hasse; fifty-five pages, three plates; one containing coloured diagrams of vertebræ to illustrate a scheme of the evolution of fishes.—On *Gorgonia verrucosa*, by G. von Koch.—On the retrogression of eyes in Arachnida, by A. Stecker; one plate, illustrating Chernes and Chelifer.—On the osteology of the gorilla, by C. Aeby; with five wood-cuts.

SOCIETIES AND ACADEMIES

London

Entomological Society, September 4.—Fred. Smith, vice-president, in the chair.—Mr. David Price of Horsham, Sussex, was elected as a Member, and Capt. Thos. Broun, of Auckland, New Zealand, as a Subscriber.—Mr. Rutherford exhibited two specimens of an orthopterous insect *Palophus centaurus*, West, from Old Calabar.—Mr. F. Smith exhibited a specimen of the fruit of the so-called "locust-tree" (*Hymenea conbaril*), from British Guiana, forwarded to Dr. Sharp from Mr. Harper. The fruit on being opened had been found to contain three living specimens of a weevil (*Cryptorhynchus stigma*, Linn.), a cocoon containing the chrysalis of a moth, together with the remains of one or more such cocoons, and lastly, a small parasitic hymenopteron (an Ichneumon allied to *Chelonus*). Mr. Smith also exhibited a specimen of *Melolontha vulgaris*, which had lately been found alive under turf at the bottom of a box in which the larva had been placed last April, thus making it appear probable that the insect assumes its perfect state underground a long time before making its actual appearance.—Mr. Champion exhibited a series of *Spercheus emarginatus*, taken at West Ham, Essex.—Mr. Jno. Spiller exhibited some so-called "jumping-seeds," received from Mexico, and contributed remarks thereon.—The secretary exhibited a photograph of a fossil butterfly, *Prodryas persephone*, Scudd., received from Mr. Scudder. The insect represented was in an excellent state of preservation, and had been found in the tertiary formation of Colorado.—Mr. Smith stated that having recently had occasion to refer to the Linnean collection in the apartments of the Linnean Society, he regretted to find it had been allowed to fall into a state of complete neglect.—Mr. Swinton communicated a paper on the vocal and instrumental music of insects.—Mr. Waterhouse read a paper entitled notice of a small collection of coleoptera from Jamaica, with descriptions of new species from the West Indies.

Philadelphia

Academy of Natural Sciences, April 30.—On the bridging convolutions in primates, by A. J. Parker.

May 14.—Elements of the sidereal system, by Jacob Ennis.—Descriptions of new species of North American bees, by E. T. Cresson.

May 28.—Transition forms in crinoids, and description of five new species, by C. Wachsmuth and F. Springer.

June 4.—The law governing sex, by T. Meehan.

July 9.—On pelagic amphipods, by T. H. Streets.

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