THURSDAY, NOVEMBER 20, 1873

THE ARCTIC EXPEDITION OF 1874

THE prospect of the Government being convinced of the propriety of despatching an Arctic Expedition, really seems to be brightening. We expressed some apprehension, when the Royal Geographical Society addressed the late Chancellor of the Exchequer on the subject last year, that sufficient pains were not taken to have all branches of Science represented in the Deputation, and that, consequently, the importance of the results of Arctic Research had not been completely explained. There is no cause for any such doubt on the present occasion. The matter has been most carefully and maturely considered by a joint committee appointed by the Councils of the Royal and the Royal Geographical Societies, and consisting of representatives of various departments of Science as well as of the most eminent Arctic authorities.

A memorandum has been drawn up, and submitted to the Council of the Royal Society, in which the scientific results to be obtained from the examination of the unknown area round the North Pole are set forth; the different sections having been prepared by men who are in the first rank as authorities in their particular departments of study—namely, geography, hydrography, geodesy, physics, meteorology, geology, botany, zoology, and anthropology. The memorandum also includes a carefully prepared statement, drawn up by distinguished Arctic authorities of the practical aspects of the question, the composition of such an expedition, the precautions that should be taken, and the best route.

The Royal Society is a body which, from its high position and from its strong sense of responsibility, never takes action without very careful and mature previous consideration. When this body once adopts a course on any question, the public can always feel satisfied that it has first received the closest attention, in all its bearings, from men of the highest attainments. The memorandum of the Committee has been before the Council, and we are able to announce that the value of the scientific results to be derived from Arctic exploration has been recognised, and that the Royal Society is prepared to represent to the Government the desirability of undertaking the discovery of the unknown region.

With the object of inducing the Government to undertake a North Polar Expedition, the Council of the Royal Society has appointed a deputation to represent their views, consisting of Dr. Hooker, the President-elect, Prof. Huxley, Prof. Allman, Mr. Prestwich, Mr. Busk, Mr. Sclater, and General Strachey.

The British Association has also appointed a Committee with the same object.

The Royal Geographical Society will be represented by its President, Sir Bartle Frere, Sir Henry Rawlinson, the veteran Arctic explorer, Sir George Back, and Admirals Collinson, Ommanney, and Sherard Osborn.

The Dundee Chamber of Commerce is also deeply impressed with the practical importance of discovery in the unknown area, and has drawn up a memorial to be presented to the Prime Minister, through the member, Sir John Ogilvy. Dundee is not only the principal whaling port of Great Britain, but is also the centre of a great and thriving industry, namely, the manufacture of jute, the growth of which employs millions of ryots in Bengal. Now, in the process of preparing the jute fibre, the use of animal oil is essential, so that the business of chasing whales and narwhals in the Arctic seas is of the utmost importance to the cultivators of the Gangetic delta. One industry supports the other, and India, as well as Great Britain, has an interest in Arctic discovery. The Chamber of Commerce, considering the vast interests at stake, holds it to be most important that the unknown polar region should be explored, in order that a more complete knowledge may be acquired of the haunts, migrations, numbers, and habits of the various oil-yielding animals. The Chamber also feels the advantages derived from Arctic expeditions by the best among the experienced mates and harpooners who obtain employment, and indirectly by the whole seafaring population of the west coast of Scotland. Nor are the bold seamen and enterprising manufacturers of the northern ports, any more than the naval officers and men of science, indifferent to the old renown of their country, and to the immense advantages which are derived from voyages of discovery.

The events of the last year have strengthened the arguments in favour of an Arctic Expedition. We believe that the despatch of a naval officer to Baffin's Bay last spring was due to the forethought of Admiral Sherard Osborn. The choice was undoubtedly a fortunate one, for Captain Markham entered heart and soul into the spirit of the service on which he was employed. He studied the new system of ice navigation, and of handling powerful steamers in the ice with minute attention. He had the rescued crew of the *Polaris* on board for several months, and learned from Dr. Bessels and Mr. Chester all the particulars of their extraordinarily successful voyage. Nothing escaped him, and on his return he submitted a full and most valuable report. Thus the fact that a ship can pass up Smith Sound to 82° 16′ N. without check of any description, unknown before, is now established, as well as the constant movement and drift of the ice in the strait leading to the unknown region. The revolution in ice navigation, caused by the use of powerful steamers, is also more fully understood and appreciated through the report of Captain Markham.

The deputation which is about to seek an interview with Mr. Gladstone and Mr. Goschen, is thus strengthened with fresh arguments and with a more exact and complete statement of the objects of Arctic research. It will represent interests which cannot be neglected, and bodies whose individual opinions must needs carry great weight. There will be the Royal Society, the recognised adviser of the Government on all matters relating to Science; the Royal Geographical Society, the British Association, and the Dundee Chamber of Commerce representing the interests of a great industry and of the sea-faring population of Scotland. The navy will also be fully represented, and the leading Arctic authorities will be present, acting in perfect unanimity as regards the route to be taken and the work to be done.

We believe that such a deputation must have considerable influence on the decision of the Government, and that there is every prospect of sanction being given to the fitting out of a naval Arctic expedition in 1874. Mr. Goschen is, we have reason to think, now conversant with the subject, and, as the Minister whose duty it is to advance and foster the interests of the British navy, it is imposible that he can fail to see the advantages of Arctic service. He is supported, at the Admiralty, by Sir Alexander Milne, who has ever been friendly to such enterprises, and sensible of the excellent school for nnval men afforded by voyages of discovery; and by Admiral Richards, the hydrographer, whose sound judgment and great Arctic experience render his advice most valuable.

The Prime Minister, with whom the decision will rest, is a statesman who well knows the general, as well as the scientific uses of Arctic enterprise. He formed one of that Ministry which despatched the last scientific expedition to the Arctic Regions; and, as a member of the Select Committee of the House of Commons on Sir John Ross's case, he signed a report expressing his approval of Arctic voyages in the strongest terms—"A public service is rendered to a maritime country, especially in times of peace, by deeds of daring, enterprise, and patient endurance of hardship, which excite the public sympathy and enlist the general feeling in favour of maritime adventure." Such were, and we trust still are, the views of Mr. Gladstone with reference to the general uses of Arctic voyages of discovery. When to these general impressions are added a knowledge of the important scientific and practical results to be attained, the assurance that there is no undue risk, that the cost will be comparatively slight, and the good both to the navy and to mercantile interests incalculable, we cannot bring ourselves to believe that the decision of Mr. Gladstone will not be favourable to a renewal of Arctic research.

LOCAL SCIENTIFIC SOCIETIES *

II.

ALTOGETHER, so far as we have been able to ascertain,† the number of existing local societies ‡ which have for their main, or only as a part of their object the culture of Science, that were established in the years between 1781 and 1830, are only 22. We shall see that the increase since 1830 has been enormous, though the large majority of those established during the last forty-three years are of a much more simple kind, so far as organisation is concerned, than those established during the former period, have to a great extent a different object in view or rather accomplish the intellectual improvement of the members after a different fashion, and are, we think, thoroughly characteristic of the scientifically inquisitive and increasingly intelligent period during which they have been established. Not many "Literary and Philosophical Societies" have been established during the latter period, most of them being professedly devoted to study and research in Science, especially in natural history, in all or one of its branches, and a large majority of them being Field Clubs, as those associations are called, the whole or part of whose programme is to investigate the natural history (including botany, zoology, and geology) of particular districts, in combination sometimes with their archæology. Indeed the last forty years might well be designated the era of field clubs.

We have already mentioned the Northumberland, Durham, and Newcastle Natural History Society, established in 1829, which, although it has done some excellent field-club work, was not professedly established for this purpose. There can be no doubt that the first genuine field-club was the Berwickshire Naturalists' Club, founded September 21, 1831, though Sir Walter Elliot traces the true origin of field-clubs to an association of students, formed in 1823 at the University of Edinburgh, under the name of the Plinian Society, for the advancement of the "study of natural history, antiquities, and the physical sciences in general." They met weekly in the evening during the session, from November to July, for reading papers and discussions; and also, as the season advanced, made occasional excursions into the neighbouring country. The chief promoters of the scheme were three brothers named Baird, from Berwickshire; but John, the eldest, must be considered the founder. He drew up an elaborate code of laws in eighteen chapters, and, as the first president, made a statement of the proposed plan and objects of the society at their inaugural meeting on the 14th January 1823. Among the original members occur the names of James Hardie, J. Grant Malcolmson (both Indian geologists), and Dr. John Coldstream; and, at a later period, those of Charles Darwin * (of Shrewsbury, 1826), John Hutton Balfour (1827), and Hugh Falconer (1828), with others who have since become distinguished in the scientific and literary world. The latest notice of the society is the session of 1829-30, up to which time the Bairds, although they had left the University, appear as occasional contributors.

No doubt this Edinburgh Association had considerable influence in originating the Berwickshire Club, for two of the Bairds became parish ministers in Berwickshire, and it was they, along with their brother, the late Dr. William Baird, of the British Museum, Dr. Johnstone, Dr. Embleton, and four or five others, who met at Coldingham on the date above given, and drew up the plan of the Berwickshire Naturalists' Club, "a term," Sir W. Elliot remarks, "now first extended to a scientific body." Its object was declared to be the "investigation of the natural history of Berwickshire and its vicinage;" in reality its field extends over the whole of Berwickshire, Roxburghshire, and the north-east part of Northumberland, to the limits of the Tyneside Club's district. The rules of the club, as all rules should be, are short, providing that the club should hold no property, require no admission fee, and should meet five times in the year at a place and hour to be communicated to each member by the secretary. Thus the Berwickshire Club is a field-club pure and simple, having, unlike many other similar clubs, no winter meetings for the reading of papers, whatever papers are read being read after dinner on the days when excursions are made. At the first anniversary it numbered 27 members, and in 1870, when Sir Walter Elliot gave his address, there were 249 members on the roll, including a few ladies, and "two corresponding members, the last description having been

* Continued from vol. viii. p. 524.

† We regret to say that none of the Edinburgh Societies have seen meet to forward us information.

‡ We do not include in this article the great London Societies, as the Royal, the Linnean, the Astronomical, &c.

* The first paper contributed by him, entitled "On the Ova of the *Flustra*," in which he announces that he has discovered organs of motion, and, secondly, that the small black body hitherto mistaken for the young of *Fucus loreus* is in reality the ovum of *Pontobdella muricata*, exhibits his early habits of minute investigation.

added in 1868 to admit intelligent working-men," though why this invidious distinction should be maintained in a body solely devoted to scientific research, we fail to see; surely Science at least is a common ground on which all classes can meet without a shadow of bitter class-feeling to mar the geniality of intercourse. The more that the higher tastes and recreations are common to all classes, the less room will there be for misunderstanding and bitterness. If a working-man can pay the subscription—and the field-club subscription is usually small, and working-men's wages are now unusually high—by all means let them be received on a common footing with the other members. Many of our best field-clubs are composed almost entirely of working-men, and every encouragement should be given to this class to join such clubs, for, morally and intellectually, we think they will reap more benefit from such associations than any other class.

The Berwickshire Club continues to be one of the most efficient and productive in the country, the fruits of its excursions being contained in six goodly volumes, containing many valuable papers on the natural history and archæology of its large district, and extensive and carefully compiled lists of the existing and extinct fauna and flora. As the Berwickshire Club is the model after which, to some extent, all succeeding field-clubs have been formed, we shall here give from Sir Walter Elliot's address, its simple and inexpensive method of conducting its field-days:—"Arrangements are made with the railway companies for the issue of tickets on favourable terms. The members assemble at breakfast at 9.30, after which the programme of the day is explained, and any objects of interest procured since the last meeting are exhibited and described. At 11 the party proceeds on foot or by conveyance to the points indicated, breaking into sections for botanical, geological, or antiquarian research, and either meeting again at some convenient spot, or returning independently to dinner at 4 o'clock. The members present rarely exceed from 30 to 50, often fewer. Of course the hive contains a considerable proportion of drones who rarely appear, ladies never. The distances are so great, the excursions so thoroughly directed to investigation, that few but those intent on work attend. After a frugal repast, the staple of which is a fine salmon invariably sent from Berwick, papers are read and discussed, and the members disperse according to the exigencies of their trains. The whole expenses of the day vary from four to five shillings per head."

In the decade between 1830 and 1840, other sixteen local societies were formed, many of which, though not professedly field-clubs, have done, through individual members, good field-club work, as is testified by their publications, and have otherwise done much to promote the cause of Science in the neighbourhood. It was during this period that the Cornwall Polytechnic Society (already mentioned), the Penzance Natural History and Antiquarian Society, the Royal Institution of South Wales, the Ludlow Natural History Society, and the West Riding Geological and Polytechnic Society, were formed, each of which, in its own particular fashion, does good service to Science, and helps to keep the lamp of culture burning in its neighbourhood.

No other regular field-club was instituted until nearly fifteen years after the foundation of the Berwickshire Club, when a sort of offshoot of that Society was formed in 1846 in Newcastle-on-Tyne, under the title of the Tyneside Naturalists' Field Club, which, "guided by the experience of the parent club, at once assumed a perfect organisation." The constitution was, however, somewhat amplified, a proviso being put in the rules that should assuredly have a place in the rules of every similar society in the kingdom. Its last rule, we think, worthy of all commendation and universal imitation; it is as follows:—

"That the Club shall endeavour to discourage the practice of removing rare plants from the localities of which they are characteristic, and of risking the extermination of rare birds and other animals by wanton persecution; that the members be requested to use their influence with landowners and others, for the protection of the characteristic birds of the country, and to dispel the prejudices which are leading to their destruction; and that consequently the rarer botanical specimens collected at the Field Meetings be chiefly such as can be gathered without disturbing the roots of the plants; and that notes on the habits of birds be accumulated instead of specimens, by which our closet collections would be enriched only at the expense of nature's great museum out of doors. That in like manner the club shall endeavour to cultivate a fuller knowledge of the local antiquities, historical, popular, and idiomatic, and to promote a taste for carefully preserving the monuments of the past from wanton injury."

We have more than once recently in noticing the proceedings of some societies, and it has been animadverted on in other quarters, referred to the pernicious practice of encouraging, by the offer of prizes for rare specimens, especially of plants, the extermination of the rare flora peculiar to certain districts. One of the prime duties of every local club should be the preservation of such rare specimens, the fact of whose existence is often of great value from a scientific point of view, and the destruction of which, by transference to a herbarium, can serve no good purpose whatever. The Tyneside Club is divided into six sections, each charged with a special department for investigation:—1, Mammalia and Ornithology; 2, Amphibia, Ichthyology, Radiata; 3, Mollusca, Crustacea, Zoophytes; 4, Entomology; 5, Botany; 6, Geology. This club holds meetings during the winter in Newcastle. Up to 1864, it had published six volumes of very valuable Transactions. In that year an arrangement was come to whereby the members (numbering 429), became associates of the Northumberland, Durham, and Newcastle Natural History Society, already referred to. Thenceforth, as we have already said, the proceedings of the two bodies have been published conjointly under the title of "Natural History Transactions of Northumberland and Durham," of which three volumes have been published. "The work of the Club," Sir Walter Elliott says, "has been most conspicuous in zoology. It has the merit of publishing its lists and catalogues in a separate form for sale, so as to make them accessible to all inquirers."

We cannot mention in detail the foundation of the swarm of field-clubs which have come into existence in the various parts of the country since 1846; we can only allude very briefly to two of the most important, the Cotteswold and the Woolhope, the former an offshoot of the Berwickshire Club. The originators of the Cotteswold Field Club, which, like the Tyneside Club, was started in 1846, were Sir Thomas Tancred (who had been a member of the Berwickshire Club), Mr. T. B. Lloyd Baker (the well-known originator of the "Reformatory System"), Dr. Daubeny, of Oxford, Hugh Strickland, and some others, who met "at the Black Bull Inn, in Birdlip, a village on the summit of the Cotteswold range overlooking the vales of Gloucester and Worcester, about six miles south of Cheltenham, and seven south-west of Gloucester." There the club was inaugurated, Mr. Baker being elected the first president. "The labours of the club have been most conspicuous in geological investigation, for which the district offers such a rich field. Many of the members have, by their recorded observations, attained to high distinction. In the words of the president, 'It will suffice to mention the names of Daubeny, Strickland, Woodward, Maskelyne, Wright, Moore, Buckman, Jones, Lycett, Brodie, Symonds, Maw, and Etheridge, all members of the club, to recall at once names of writers well known in the scientific annals of the county, and of whom some have by their works obtained a more than European reputation.'"

The Woolhope Club, in Bedfordshire, whose publications are also well known as among the most valuable of those of provincial societies, was formed in 1851, and derived its name from the mass of Silurian rocks described by Sir Roderick Murchison as the "Woolhope Valley of Elevation." This club and the Cotteswold have occasional joint field days, and their example is followed by several other societies, and might, we think, with advantage be followed much more extensively than it is.

The Worcestershire Naturalists' Club originated in the same year as the Cotteswold, followed the year after by the Huddersfield Naturalists' Society, and in 1849 by the Yorkshire Naturalists' Club. Besides the four field-clubs mentioned, other six societies originated in this decade, most of them distinctly scientific, including the Torquay Natural History Society, the Bristol Microscopic Society, and the Isle of Wight Philosophical and Scientific Society.

In the decade between 1850 and 1860, twenty-two local scientific societies were founded, of which sixteen are field-clubs, including such well-known names as the Woolhope, just mentioned, the London Geologists' Association, the Liverpool Naturalists' Field Society, the Bath Natural History and Antiquarian Field Club, and the Malvern Field Club.

(*To be continued.*)

HARTWIG'S "SEA AND ITS WONDERS"

The Sea and its living Wonders. A popular account of the Marvels of the Deep, and of the progress of Maritime Discovery from the earliest ages to the present time. By Dr. G. Hartwig. Fourth edition, enlarged and improved, with numerous woodcuts and eight chromoxylographic plates. (London: Longmans, 1873.)

NO other evidence is needed beyond the publication of the fourth edition of this work to prove the demand there is in Great Britain for this kind of literature, The reading public want to know what about the sea, and all that is in it; and, in their eagerness to know, they buy even such books as this. When will scientific men turn their attention towards teaching the public as far as it can be taught, in a correct, yet popular manner, the rudiments of biological science? When they do the time for such books as the one we must now notice will have passed away, and the resources of the great publishing firm who issue it will be engaged on more truly solid and important work. As an indication of what we mean, let us contrast the popular works of Hartwig or Figuier with Quatrefages' "Souvenirs d'une Naturaliste," or Gosse's "Devonshire Rambles;" or let the reader imagine what a delightful work the one before us would have been if written by, say Huxley, Allman, Günther, or Wyville Thomson. But to return to this volume, which consists of three parts; (1) the Physical Geography of the Sea; (2) the Inhabitants of the Sea; (3) the Progress of Maritime Discovery. The latter part commences with the maritime discoveries of the Phœnicians, and ends with a reference of sixteen lines in length to the numerous scientific voyages of circumnavigation of the present century.

Before proceeding to very briefly notice Parts I. and II., we have to object most strongly to the woodcuts not being drawn to any scale; thus, on page 101 the Rorqual is figured as rather smaller than the Herring, while, on the same page, and just above these figures, will be found a Whale Louse, and a Lepas represented as bigger than either. Surely figures like these must terribly mislead the ordinary reader, who, though he may possibly have some notion of the size of a herring, cannot be supposed to be aware of the dimensions of the whale's parasites. Many of the woodcuts are very good, but several of them are bad, and the majority of them are not seen in this volume for the first time; this we would not so much object to if the woodcuts were selected to illustrate the text, and not, as is too often the case in this work, the text written so as to make some forced allusion to the woodcuts.

Though the Dugong is illustrated by copying the woodcut from Tennent's work on Ceylon, yet scarcely a word is to be found about it in the chapter on the Cetacea. The Tailor birds' nest is figured on page 143, but no allusion whatever is made to it in the text. The great Auk is figured, and in the accompanying explanation is said to congregate in vast flocks on the rocky islets and headlands of the Northern Coasts. Surely a little careful supervision would have prevented such mistakes as these occurring. But leaving the subject of the woodcuts, we come to consider the letterpress; and here, too, not only a more careful supervision, but some more acquaintance with the subject would have been desirable. Why, among the Fishes, should the Anchovy have five lines devoted to it, when not one word is to be found about that equally important little fish, the Sardine? and surely half a page would not have been too much to devote to that interesting living wonder of the sea, the Whitebait. It would be an easy, but withal a useless task to point out other errors of omission and commission among the other classes.

Among the Corals and Sponges the author had enough to guide him, for he has borrowed wholesale the really beautiful woodcuts illustrating Prof. Greene's Manuals; if he had borrowed equally largely from their text, he would have made this the most trustworthy portion of his book.

No notice is taken of such important new forms as Rhizocrinus, or Brissinga, nor do we find mention under the Sponges of such strikingly beautiful ones as belong to the genera Euplectella, Holtenia, &c., though, indeed, some allusion is made to these in the chapter on the geographical distribution of marine life. But perhaps we have said enough to show that while the subject of this work is a good one, it might easily have been treated by a writer more familiar with it in a better, a more original, and a more comprehensive manner. E. P. W.

OUR BOOK SHELF

The Theory of Evolution of Living Things. By Rev. G. Henslow. (Macmillan and Co.)

SCIENTIFIC men cannot but feel how false is the stimulus given to that form of literature of which the above-named work is an example. If considerable pecuniary reward is offered for the production of treatises in favour of any theory, or of the mutual compatibility of any two or more different doctrines, the work will undoubtedly be produced, however inaccurate the theory, or however dissimilar the doctrines. That mistaken enthusiasm which led to the production of the Bridgwater Treatises and the establishment of the Actonian Prize, has resulted in the publication during the last year of two Actonian prize essays, the former of which, by Mr. B. T. Lowne, we noticed on a previous occasion, whilst the latter is the one under consideration. The present author's treatment of his subject is much that which would have been adopted by Paley if he had been living at the present day. Several previously accepted axioms are shown to be incompatible with the existing position of biological science, and their weakness is well brought forward. Other considerations of modern development are introduced, and it is in these that the difficulty of combining the two doctrines appears. For instance, the origin of moral evil is said to be "the conscious abuse of means, instead of using them solely for the ends for which they were designed." But on evolutionary principles, it can hardly be said that there are means for designed ends, because that peculiarity in an organ which is of service is the only one retained, insomuch that if the delicate sensitiveness of the conjunctiva of the eye were to prove of more value to the individual than its sight, the power of vision would most probably become lost at the expense of the developing tactile organ. "The continual effort of beings to arrive at mutual and beneficial adjustments" is said to be a great principle of nature; does not the term "struggle for existence" imply something very different from this? Again, that "animals and plants do not live where circumstances may be best suited to them, but where they *can*, or where other animals and plants will respectively let them live," is quoted by the author as an instance of Nature falling short of that absolute degree of perfection which may be conceived as possible; however, there cannot be many who think a locality a suitable residence, in which they are prevented from taking up their abode, or perhaps entering, by the animals and plants which inhabit it. In other places similar weaknesses may be found in the argument adopted. In one thing Mr. Henslow has done great good: he has shown that it is consistent with a full dogmatic belief, to hold opinions very different from those taught as natural theology some half century and more ago.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Transfer of the South Kensington Museum

I AM glad to see that an effective opposition is likely to be made to the ill-advised proposal of the Government to place the South Kensington Collections under the control of the fifty irresponsible Trustees of the British Museum.

In common with many other naturalists I had always hoped that the national collections of natural history, when removed to the new buildings in South Kensington, would be freed from the rule of the Trustees and placed under a responsible director. The memorial of which I enclose a copy, and the republication of which would, I think, be opportune at the present juncture, will serve to show that I am by no means alone in believing that such a change would be beneficial to Science.

It would seem, however, that the Government, so far from acceding to our views, have resolved to proceed in exactly the contrary direction, and to increase the power of the Trustees. I can only hope that we may succeed in preventing them from carrying this retrograde measure into effect.

P. L. SCLATER

44, Elvaston Place Queen's Gate, Nov. 17

"*Copy of a Memorial presented to the Right Hon. the Chancellor of the Exchequer*

"To the Rt. Hon. the Chancellor of the Exchequer

"Sir,—It having been stated that the scientific men of the metropolis are, as a body, entirely opposed to the removal of the natural history collections from their present situation in the British Museum, we, the undersigned Fellows of the Royal, Linnean, Geological, and Zoological Societies of London, beg leave to offer to you the following expression of our opinion upon the subject.

"We are of opinion that it is of fundamental importance to the progress of the natural sciences in this country, that the administration of the national natural history collections should be separated from that of the library and art collections, and placed under one officer, who should be immediately responsible to one of the Queen's Ministers.

"We regard the exact locality of the National Museum of Natural History as a question of comparatively minor importance, provided that it be conveniently accessible and within the metropolitan district.

GEORGE BENTHAM, F.R.S.
WILLIAM B. CARPENTER, M.D., F.R.S.
W. S. DALLAS, F.L.S.
CHARLES DARWIN, F.R.S.
F. DUCANE GODMAN, F.L.S.
J. H. GURNEY, F.Z.S.
EDWARD HAMILTON, M.D., F.L.S.
JOSEPH D. HOOKER, M.D., F.R.S.
THOMAS H. HUXLEY, F.R.S.
JOHN KIRK, F.L.S.
LILFORD, F.L.S.
ALFRED NEWTON, F.L.S,
W. KITCHEN PARKER, F.R.S.
ANDREW RAMSAY, F.R.S.
ARTHUR RUSSELL, M.P.
OSBERT SALVIN, F.L.S.
P. L. SCLATER, F.R.S.
G. SCLATER-BOOTH, M.P.
S. JAMES A. SALTER, F.R.S.
W. H. SIMPSON, F.Z.S.
J. EMMERSON TENNENT, F.R.S.
THOMAS THOMSON, M.D., F.R.S.
H. B. TRISTRAM, F.L.S.
WALDEN, F.L.S.
ALFRED R. WALLACE, F.Z.S.

"London, May 14, 1866"

Deep-sea Soundings and Deep-sea Thermometers

WILL you allow me to reply to a letter from Messrs. Negretti and Zambra that appeared in vol. viii. p. 529, in reference to my Casella-Miller Deep Sea Thermometer, in which they accuse me and the late respected Dr. Miller of "plagiarism."

I presume, by this remark, that they intend to convey the idea of their own introduction having been imitated, because they state also that "their thermometer is identical in every respect except in size." Without venturing to trespass upon your valuable space by now going into more detail to prove the contrary, I will merely remark that if you, or any of your numerous readers who may feel interested in this subject, will favour me with a visit to my establishment, I shall be happy to give the fullest explanation as well as show the great difference existing between the two, will point out the cause of failure in their arrangement, and also the reason of the complete success of my own thermometer.

Though perhaps it is unfortunate for your correspondents that their reference to Dr. Miller was not made during his lifetime, yet, admitting that he said he was not aware of their arrangement, I must ask in all seriousness, What had their thermometer accomplished to make any one acquainted with it?

Facts speak for themselves. Their arrangement still remains without result, whilst my thermometer, which has solved the great problem of the true temperature of the sea even at its greatest depths, has been adopted not only by our own Government, but also by all the principal Governments and scientific authorities throughout the world. LOUIS P. CASELLA

147 Holborn Bars, Nov. 3

Squalus spinosus

ON the 9th inst. the fishermen of Durgan, in Helford Harbour, sent for me to look at a fish new to them, which had been caught (with a ½*d.* hook) on the preceding night near its entrance. Congers had been numerous, but suddenly ceased to bite. The fish (a spinous shark) had been hooked in the corner of its mouth, out of the reach of its sharp teeth, had wound the line many times round its body, which was 7 ft. in length, and 30 in. in girth, being longer and more slender than one of which I sent a notice to the Royal Cornwall Institution 38 years ago. The back, sprinkled over with spines, was of a dark grey colour, the belly nearly white. It was a male fish. The lobes of the liver were 4 ft. in length. In the stomach was a partially digested dogfish, 2 ft. long. The upper lobe of the tail was muscular and long, perhaps to aid its ground feeding, the lower lobe more marked than in Dr. A. Smith's drawing, as given by Yarrel, and entirely unlike that of the Filey Bay specimen. Twelve hours or more after its capture, when all external signs of life had disappeared, I was surprised to observe the regular pulsations of the heart.

Prof. Huxley has not observed a correspondence between the mass and large convolutions of the brain of a porpoise and its intellectual power.

Several years ago a herd of porpoises was scattered by a net, which I had got made, to enclose some of them. It was strong enough to catch tigers if set in the straits of Singapore, across which they sometimes swim. The whole "sculle" was much alarmed, two were secured. I conclude that their companions retained a vivid remembrance of the sea-fight, as these cetacea, although frequent visitants in this harbour previously, and often watched for, were not seen in it again for two years or more.

Trebah, Falmouth, Oct. 27 C. FOX

Zodiacal Light

IT is a matter for regret that with the magnificent opportunities of investigating the character of the Zodiacal Light afforded to Maxwell Hall by his elevated position in Jamaica, he does not seem to have brought the powers of either the spectroscope or polariscope to bear on it.

I think the full importance of the inquiry is hardly appreciated by many. Taking the generally accepted theory of the light—that of a lens-shaped disc of luminous matter, with the sun for its centre and a diameter exceeding that of the earth's orbit—its matter, lying as it does in the plane of the elliptic, actually connects us with the sun, and may be the medium through which the solar magnetic forces act upon our own.

The intimate connection between solar outbursts, auroras, and terrestrial magnetism is an established fact.

To the aurora, the zodiacal light is by many conceived to be nearly allied, and I do not think the evidence hitherto adduced against this theory is at all conclusive. The remarkable wave of light seen by Maxwell Hall is strongly in favour of it; and though spectroscopic observations seem to point the other way, they are as yet so scanty in number that it would be as unfair to argue from them the want of connection between the two phenomena, as it would be to assert that the planets have no volcanic fires of their own because they only give us a reflected solar spectrum.

Assume the zodiacal light to consist of solid particles of matter—planet dust—shining by reflected light, and it is not difficult to imagine the aurora playing amongst these tiny worlds, each of which might have its own small magnetic system, swayed like our own by the master magnet, the sun.

So far as my own experience goes I can see no objections to this assumption. Though I have seen the light very brilliant in both its branches, I have never yet found it to have a decided outline. Nor have I been able to trace it either east or west to 180° from the sun. Granting that this can be done, however, the apparent vanishing point of the earth's shadow lies comparatively near us, and far within this again is the point at which the shadow would subtend only a degree or two of arc, and at which it would be very hard to discern mid the feeble light of this portion of the zodiacal light; so that a slight extension of the diameter of the disc would remove any objection that might be raised under this head.

Imagine one of Saturn's moons revolving in an orbit within his belts, and fairly embedded in the matter, which, for the sake of the argument, we must assume to be illuminated by the planet. To inhabitants of that satellite each night would bring a phenomenon closely resembling our zodiacal light, only far more brilliant. At midnight two cones of light would taper upwards east and west, and meet overhead. The brightest portion of each cone would be that along the axis and nearest the horizon. Towards the summit and on the borders, where the line of sight would lie through less depths of matter, the light would gradually fade away, but from the satellite being embedded in the belt, the entire sky would be more or less luminous.

Has it not been noticed on our earth that when the zodiacal light has been seen unusually bright, a "phosphorescence" of the sky was everywhere visible? May this not arise from our solar belt in a somewhat similar manner?

From my personal observations I see no reason to give a lenticular form to the disc. Parallel faces would afford a perspective such as the zodiacal light appears to me.

I would urge observers who may be fortunately situated, not to neglect opportunities. So far as I am able I shall do my best to aid the work of inquiry, and with the powerful instruments that Browning is forwarding me, placed at an elevation of more than 6,000 ft., under the clear skies of our Indian winter, I trust I shall be able to add something to our knowledge of the zodiacal light.

I should feel much indebted to any of your readers who would inform me which is the best adapted polariscope for such researches, and whose (amongst makers) speciality such instruments are. E. H. PRINGLE

Camp Udapi, South Canara, Oct. 3

Cold Treatment of Gases

ALLOW me to submit to your readers the following sketch o an apparatus for producing extreme cold, by which it might perhaps be practicable to liquefy or even solidify the elementary gases which have hitherto resisted the efforts of chemists.

The gas to be operated on is compressed to any required degree by means of one cylinder, is cooled to the lowest convenient degree in the ordinary way, passes into an expansion cylinder with a properly arranged cut-off, where in expansion its temperature is still further lowered. From the expansion cylinder it returns back to the compression cylinder, extracting the heat from the counter current proceeding from the compression cylinder, so that the latter will be always arriving at the expansion cylinder with a continually decreasing temperature.

As out here I have no possible means of trying whether there is anything in this idea, I offer it to any of your readers who may feel disposed to try it.

Graaff Reinet College, Cape Colony, July 19. T. GUTHRIE

The Relation of Man to the Ice-sheet

MR. TIDDEMAN has shown for Yorkshire what I proved six years ago for the South of England in a paper in the *Geological Magazine* (vol. iv. p. 193), that glacial conditions have obtained in this country since its occupation by Palæolithic man. Unfortunately an attempt which I made to explain this coincidence between his result and mine in a letter to the same periodical in February last was rendered abortive by a clerical (or perhaps printer's) error. I would press upon geologists to consider whether the point proved is not that a glacial period has intervened since the times of Palæolithic man and the present, rather than that man existed in this country before the glacial epoch, I think Mr. Tiddeman thinks as I do; but I take the liberty of stating this view more distinctly. O. FISHER

Wave Motion

IN NATURE, vol. viii. p. 506, Mr. Woodward has suggested a simple and ingenious illustration of wave motion. Could he, or any other correspondent, supply, or refer to, a popular explanation of the action of the particles upon each other, to which the propagation of the wave is due?

In the case of sound waves, the propagation is comparatively simple, and is fully and clearly explained in Dr. Tyndall's "Lectures on Sound," and elsewhere. Helmholtz, in his "Popular Lectures," has figured the motion of the individual particles of which a water wave is composed. And in Sir John Herschel's "Familiar Lectures," there is an elaborate and beautiful demonstration of the motion of the particles of ether in plane and circularly polarised light; but neither of these expositions appears to deal with the mode of propagation of the motion by which the wave is formed.

On the other hand, Sir Charles Wheatstone's ingenious model beautifully exemplifies the interaction of *waves* and their results. But here the waves are produced by the wooden wave forms introduced into the machine, the beads representing the particles remaining fixed in relation to each other. Neither, therefore, can this explain the manner and direction of the actual impact of each particle upon the adjacent one (beginning with those in contact with the source of motion itself), to which, combined with the tendency to yield in the direction of least resistance, the water wave must owe its form, and upon which the still more complicated conception of the light wave must ultimately depend.

Could a reference be given to any practical explanation of this point, it would confer a benefit on many who are not competent to follow the subject into the higher mathematics. M. F. E.

Sussex, Nov.

Elementary Biology

I, ALONG with many others, who are desirous of obtaining an insight into Nature, would esteem it a great favour, and it would be of the greatest benefit to us, if any of your scientific readers would undertake to give through your columns a short account of the various low forms of life included under the elementary stage of biology of the Science and Art Department. They might give instruction as to where the various objects could be seen, how inspected, names of the best text-books for the students' guidance, &c.

By so doing, they would secure the praise of many who at present cannot find out the modes of studying such subjects.

Hull, Nov. 8 BIOLOGY

Black Rain and Dew Ponds

CAN any of your readers explain the cause of this phenomenon? On Thursday, the 4th Sept., about 5 P.M., in the village of Marlsford, in the valley of the Thames, near Wallingford, a heavy storm of rain occurred: and the water which fell in several parts of the village was found to be nearly black. It is described as being of such a colour as would be produced by mixing ink with water. Another of these black water showers fell during the night of the following Friday.

Would any reader of NATURE also kindly set forth the theory upon which the utility of the dew ponds, found in many of the highest points of the Berkshire Downs, rests. They are circular ponds made with considerable care, and are supposed to receive so much dew as to supply all the water needed for the sheep in their neighbourhood through the driest summer.

Tiverton E. HIGHTON

ALBANY HANCOCK

THE brief announcement by which some of our readers may have first learnt of the decease of one of our greatest biologists is, in its simplicity, in singular harmony with the life the close of which it commemorates.

The retrospect of so serene a career leaves little to the biographer, for its points seem marked rather by phases of study, as indicated by important scientific memoirs, than by incidents which the world regards as striking or noteworthy.

Albany Hancock was born at Newcastle-on-Tyne on Christmas Eve, 1806. His father, Mr. John Hancock, died some six years later, and of the six little children thus left dependent on their mother, Albany was the third. He received a good education as times then went, and on leaving school was articled to a solicitor of good standing in Newcastle. Uncongenial as was the employment, he served his full term, passed the customary examinations in London, and even took an office in Newcastle with the view of establishing himself in practice. But the occupation was irksome, and he gave it up ere long to join a manufacturing firm, and this in turn circumstances led him soon to abandon. The simple fact probably was that neither occupation permitted him to follow the bent of his inclination, and that the desk and counting-house were alike distasteful to a mind pre-engaged as was his by other currents of thought. His early taste for natural history pursuits was probably in part derived from the collections, chiefly conchological, formed by his father, who was in many ways a man of superior ability, and had been something of a naturalist; and association with the late Mr. Robertson and Mr. Wingate, the one a botanist, the other an ornithologist, of repute; with the well-known Mr. Bewick; and above all with his near neighbour Mr. Alder, confirmed his inclination in this direction. He was, as a boy, clever with his fingers, and that manual dexterity which in later years served him so well when engaged with dissecting needle and pencil, exhibited itself in many of the pursuits of his early life.

The first mention we find of Mr. Hancock's devotion to natural history is in Mr. Alder's "Catalogue of Land and Fresh-water shells," published in 1830, in which the author handsomely acknowledges the obligations he is under to him and to Mr. John Thornhill "for the communication of many habitats observed during their active investigation of this as well as other branches of the natural history of the neighbourhood" of Newcastle. His earliest appearance as an author seems to have been in connection with two short papers in the first volume of "Jardine's Magazine of Zoology and Botany," published in 1836, the one a "Note on the Occurrence of *Raniceps trifurcatus* on the Northumberland Coast," the other a "Note on *Falco rufipes*, *Regulus ignicapillus* and *Larus minutus*." These notices were, comparatively speaking, of trifling significance, but they were the beginning of a long series of contributions to knowledge which only ceased when his last illness deprived him of the power of continuous work. It is unnecessary here to enumerate the successive memoirs that embody the results of his life's labour. A catalogue of the original papers of which he was author, or joint author, would extend to something over seventy titles.

Early association with Mr. Alder in the study of the mollusca led to the production between the years 1845 and 1855 of their magnificent "Monograph of the British Nudibranchiate Mollusca," which may still be taken as a standard of excellence amongst such publications. Many of Mr. Hancock's earlier papers were devoted to the elucidation of the boring apparatus of the mollusca, and these were followed by similar researches respecting the excavating power of a group of sponges (*Cliona* and allied genera) which until that time had been but little known or understood.

As an anatomist—and after all it was his large knowledge of minute anatomy and infinite skill in dissection that gave its especial value to most of his work—he was, perhaps, best known by his elaborate memoir on the Organisation of the Brachiopoda, published in the Philosophical Transactions for 1857; but many other papers of the same thorough and original character proceeded from his pen. Amongst them will be remembered the following:—"On the Olfactory Apparatus in the Bullidæ" (1852); "On the Nervous Systems of *Ommastrephes todarus*" (1852); "On the Anatomy and Physiology of the Dibranchiate Cephalopoda" (1861); "On the Structure and Homologies of the Renal Organ in the Nudibranchiate Mollusca" (1863); "On the Anatomy of Doridopsis" (1865); "On the Anatomy and Physiology of the Tunicata" (1867).

For some years previous to his death Mr. Hancock had devoted much attention to the fish of the Carboniferous period, and in conjunction firstly with Mr. T. Atthey, whose fine collection afforded ample material for the purpose, and subsequently with Mr. Howse, published a series of fifteen papers on these coal-measure fossils.

The promised Monograph of the British Tunicata, preparations for which had made some progress even before the death of Mr. Alder, had occupied much of his time; and though probably still unfinished, it may be hoped that the results of his investigations are so far complete in themselves, that the work, as far as it has gone, may be saved to science. A supplement to the Monograph of Nudibranchiate Mollusca had been a matter long on his mind, but one that he had never been able to devote himself to realising, beyond the collection of materials.

Allusion has been made to Mr. Alder, Mr. Atthey, and Mr. Howse, as having been associated with Mr. Hancock in certain of his papers; to these must be added the names of Dr. Embleton and the Rev. A. M. Norman as occasional colleagues.

On the establishment of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne in 1829, Mr. Hancock became an active supporter, and was one of the original staff of honorary curators; and on the formation of the Tyneside Naturalists' Field Club in 1846, he was one of its principal and most influential promoters. When the new College of Physical Science in Newcastle was instituted, his name, almost as a matter of course, was placed on the provisional committee; and it was only when this body had completed its labours and gave place to a permanent board, that he was permitted, on the ground of ill-health, to retire from active service in connection with the institution. He was a Fellow of the Linnean Society, a corresponding-member of the Zoological Society of London, an honorary member of the Imperial Botanico-Zoological Society of Vienna, and perhaps of some other similar bodies; but honours of this sort, though valued in their way, were thrust upon him rather than sought. Though living a retired life, no man more highly prized social intercourse. His kindly helping hand was held out to every young naturalist: Such were always welcome at his house; and when appealed to by them, as was often the case, he made their difficulties his own till he could help to solve them.

It is yet too soon to attempt to shake oneself free from a sense of his presence, or to essay to weigh in judicial balance the value of his contributions to human knowledge: considerations of this sort are overwhelmed in the sense of irreparable loss to science.

H. B. B.

FERTILISATION OF FLOWERS BY INSECTS*

IV.

On the two forms of flower of Viola tricolor, and on their different mode of fertilisation.

VIOLA *tricolor* presents a further example of the same kind of dimorphism as that described in the last article in the case of Lysimahcia, Euphrasia, and Rhinanthus. One of its two forms, illustrated by Fig. 15 in natural size, is more conspicuous than the other (Fig. 16), not only by its larger size, but also by the more striking colour of its petals. When the flower has just opened, its two upper petals are light violet, or, in rarer cases, nearly white; but they gradually become a deep violet, or even dark blue. Far more striking is, ordinarily, the change of colour in the two lateral petals and the lower one, which, immediately after the opening of the flower, are nearly white, while in a fully-developed state they are always violet. The petals of the small-flowered form of *Viola tricolor*, illustrated in natural size by Fig. 16, are, on the contrary, uniform in colour and nearly white during the whole time of flowering. The attractiveness for insects of the two kinds must therefore be very different, whereas those particular marks round the opening of the flower which serve as a guide to insects in search of the honey, the "Saftmal" of Sprengel, are nearly the same in the two varieties. That part of the lower petal immediately before the entrance of the flower (*y*, Fig. 21, 22) is in both dark yellow, and the lower petal is also marked by black streaks converging towards the same entrance. There is only this difference between the two forms as to their guide-mark (*Saftmal*), that in the large-flowered form seven black streaks on the lower petal, and three on each of the lateral ones point towards the entrance of the flower; whereas in the small-flowered form there are but five black streaks in the lower petal, and none at all on the lateral ones.*

Although these two forms have been generally known, at least since the time of Linnæus, all botanists who have published observations on the fertilisation of *Viola tricolor* have apparently turned their attention exclusively to the large-flowered form (Fig. 15), whose beautiful adaptations to cross-fertilisation by insects, have been, therefore, very accurately described; while the peculiarities in structure and fertilisation of the small-flowered form have not even been mentioned. If, in this case, we clearly see that even scientific inquirers have been far more attracted by the larger violet flowers than by the smaller whitish ones, we need not wonder that insects are influenced in like manner, and that from this cause smaller and less conspicuous flowers are so frequently quite overlooked by insects, that they would rapidly become extinct, unless slight modifications of structure and development enabled them to produce seeds by self-fertilisation.

Indeed, in *Viola tricolor*, as in those species hitherto considered, regular self-fertilisation in the small-flowered form is effected by such slight modifications of structure and development, that by far the larger number of the contrivances in the large and small-flowered forms are identical.

In both forms, honey is secreted by two long appendages (*n*) of the lower filaments (*fi*), from which it ascends by adhesion into the uppermost part of the hollow spur (*sp*); the style (*sty*, Fig. 22) is directed downwards on its base, slender and bent like a knee, while above it is straight and gradually thickened, but does not increase at all or only slightly in breadth, ending in a skull-like stigmatic knob (*k*), thick enough to completely stop the entrance of the flower. This knob is provided with a wide open moist stigmatic cavity (*st*,) and is protected from above by two sets of hairs (*pr*, Figs. 21, 22, Sprengel's "Saftdecke") on the two lateral petals, which at the same time defend the entrance of the flower against rain, and prevent insects from entering into the flower in any other way than by the lower side of the skull-like knob. In both forms the five anthers open inwards, are narrowed towards their

* Continued from vol. viii. p. 435.

* My description relates exclusively to those varieties of *Viola tricolor* which grow in the environs of Lippstadt. From Sprengel's, Bennett's, and other descriptions and illustrations, I am aware that in other localities somewhat different varieties are found. But I do not doubt that differences in the manner of fertilisation, identical or closely allied with those here to be described, will be found wherever a large-flowered and a small-flowered form of *Viola tricolor* co-exist.

end, and prolonged above into orange-coloured triangular appendages of their connectives (*c*, Figs. 21, 22), and lie so close together round the style, as to form a hollow cone containing the pollen, and overtopped only by the skull-like crest of the style. This position of the stigmatic knob rising out of the anther-cone but immediately below its summit, is secured by a remarkable contrivance, the skull-like knob being prevented from sliding into the anther-cone by two tufts of hairs, projecting like whiskers from its two cheek-like lateral surfaces. Thus a lifting up of the stigmatic knob, which must always be effected by insects seeking for honey or for pollen, and which is easily accomplished by them in consequence of the base of the style being slender and bent like a knee, will be more likely to tear off the filaments than to push the stigmatic knob into the anther-cone. Indeed, we find that by the swelling of the fertilised ovary the filaments are always torn off, whereas the anthers remain, enclosing like a hollow cone the narrow portion of the style, and the skull-like knob is never drawn between the anthers. If the anther-cone containing the pollen were densely closed all round, the pollen-grains would not fall out unless the anthers were separated from each other by lifting up the stigmatic knob; but there actually exists an opening on the lower side of the summit of the cone directed downwards, the appendages of the two lower anthers being cut out (*op*, Figs. 21, 22), by which nearly all the pollen may fall out spontaneously. When it has fallen out, a great part of the pollen is collected in the close hairy lining of the fore part of the spur.

Thus far the two forms of *Viola tricolor* are identical in structure; and the same, or nearly the same, insects may *a priori* be supposed and have really been observed, to visit the two forms. The distance between the closed entrance of the flower and the honey contained in the uppermost part of its spur being in both of the two forms 6-7 mm., an insect must be provided, in order to reach the honey, with a proboscis of at least that length, unless it be enabled by its small size to crawl with its whole body into the flower. A proboscis of 6-7 mm. length or larger is only to be met with among all our insects in Lepidoptera, Apidæ, and some few Diptera; insects sufficiently minute to be able to crawl into and out of the flowers, are to be found chiefly in the genera Thrips and Meligethes. It may therefore be supposed, *a priori*, that Lepidoptera, Apidæ, and Diptera provided with a proboscis of at least 6 mm. long, and very minute insects of the genera Thrips and Meligethes, will visit the two forms of *Viola tricolor* for honey, and that, besides, some other insects provided with shorter probosces will seek for their pollen. By direct observation this supposition has been thoroughly confirmed, as shown by the following list of visitors actually observed:—

I. As visitors of the large-flowered form, there have been observed:—(*a*) Lepidoptera: (1) *Pieris rapæ* L.* (12).†—(*b*) Apidæ: (2) *Bombus muscorum* L.* (10-15); (3) *B. lapidarius* L. ♀‡ (12-14); (4) *B. sp.*§; (5) *Anthophora pilipes* F.‖ (19-21); (6) *Andrena albicans* K. ♂ (2-2½), in vain seeking for honey.‡—(*c*) Diptera: (7) *Rhingia rostrata* L.§ (11-12); (8) *Syritta pipiens* L. (2-3), eating pollen.‡—(*d*) Thysanoptera: (9) Thrips.¶

II. As visitors of the small-flowered form, there have been observed:—(*a*) Lepidoptera: (1) *Pieris rapæ* L.;* (12), repeatedly; (2) *P. napi* L.* (11), repeatedly; (3) *Polyommatus Dorilis* Hfn.*—(*b*) Apidæ; (4) *Apis mellifica* L. (6) ☿; † (5) *Bombus hortorum* L. ♀* (18-21), perseveringly visiting the flowers for honey, although every flower is drawn down by the weight of this large humble-bee; (6) *B. Rajellus* Fll. ♀* (10-13), the same individual visiting sometimes *V. tricolor*, sometimes *Lamium purpureum*; (7) *B. muscorum* L. (*agrorum* F.) ♀ (10-14), visiting, without distinction, now the flowers of *V. tricolor*, now the nearly equally large and equally coloured flowers of *Lithospermum arvense*, while omitting the smaller ones of *Capsella busra-pastoris*, *Valerianella olitoria*, and *Myosotis versicolor*; (8) *Osmia rufa* L. ♂* (7-9), but once hastily visiting a flower for honey.—(*c*) Diptera; (9) *Rhingia rostrata* L.* (11-12), several specimens, repeatedly visiting flowers for honey.—(*d*) Coleoptera; (10) *Meligethes** crawling into the flowers.

Direct observation has thus shown that no essential difference exists between the fertilisers of the large and those of the small-flowered form. But it must appear a striking fact that not only an equal number of different species, but even one more species has been observed on the small than on the large-flowered form. All the visitors of the small-flowered form, with the exception of only one, having been observed by myself, I must add, as an explanation of this fact, that I have repeatedly watched at the most favourable weather, for several hours, a neglected field, in which, besides some other weeds, there grew an abundance of vigorous specimens of the small-flowered form of *Viola tricolor;* whereas I have never had an opportunity of watching the large-flowered form under favourable conditions. Therefore I have no doubt that, in spite of the incomplete observations hitherto made on this subject, the more conspicuous flowers are in this species also really far more frequently visited by insects than the less conspicuous ones. Otherwise the differences in structure and development of the two forms now to be described would be quite inexplicable. These differences are:—1. In the large-flowered form the stigmatic cavity lies somewhat more towards the top of the skull-like end of the style than in the small-flowered one (as shown by the comparison of Fig. 17 with Fig. 18, and of Fig. 19 with Fig. 20.)

(1) When the skull-like knob in the two forms is pressed against the lower petal, in the large-flowered form the opening of the stigmatic cavity is directed outwards, so that pollen-grains which have fallen out of the anther-cone spontaneously can never fall into the stigmatic cavity unless carried by insects; whereas in the small-flowered form the opening of the stigmatic cavity is directed inwards, so that pollen-grains falling out of the anther-cone spontaneously, fall directly into the stigmatic cavity.

(2) In the large-flowered form the opening of the stigmatic cavity (*st*, Figs. 17, 19, 21) bears on its lower side, as discovered by Hildebrand, a labiate appendage (*l*, Figs. 17, 19, 21) provided with stigmatic papillæ, so that a proboscis inserted into the flower, when charged with pollen of a previously visited flower, rubs off this pollen on to the stigmatic lip (*l*), thus regularly effecting cross-fertilisation; whereas, when withdrawn out of the flower, charged with its pollen, the proboscis presses the lip (*l*) against the stigmatic opening (*st*), thus preventing self-fertilisation. This nice adaptation to those visitors provided with a long proboscis (Lepidoptera, Apidæ, Rhingia) is completely wanting in the small-flowered form (Figs. 18, 20, 22).

(3) In the large-flowered form there is a black wedge-shaped streak (*w*, Figs. 17, 19) on the front side of the style, to which Mr. A. W. Bennett first called atten-

* By W. E. Hart (NATURE, vol. viii. p. 121).

† The numbers enclosed between parentheses after the names of the insects indicate the length of their probosces in millimetres.

‡ By myself ("Befruchtung der Blumen durch Insecten," p. 145).

§ By Ch. Darwin, who writes me, May 30, 1873:—"Between twenty and thirty years ago I observed, for two or three years, large beds (of *V. tricolor*) in the flower-garden, and saw several times *Rhingia rostrata*, and a nearly black humble-bee visit and fertilise the flowers. I say fertilise, because I had watched the flowers for a long time previously, and saw no insect visit them; but two or three days after the above visits a multitude of flowers withered and set capsules."

‖ By Delpino ("Ulteriori osservazioni," p. 62).

¶ By Sprengel ("Das entdeckte Geheimniss," p. 397), and Mr. A. W. Bennett (NATURE, vol. viii. p. 49).

* By myself (June 1873).

† By Sprengel (*loc. cit.*) and by myself (June 1873), perseveringly visiting the flowers for honey.

tion,* and which he has interpreted as a guide-mark for those visitors, which are diminutive enough to crawl entirely into the flower. This streak is also wanting in the small-flowered form (Figs. 18, 20).

(4) In the large-flowered form pollen-grains do not spontaneously fall out of the anther-cone before the flower has been fully developed for several days; whereas in the small-flowered form, in by far the majority of cases, a great number of pollen-grains fall spontaneously out of the anther-cone into the stigmatic cavity and there develop long pollen-tubes, even before the opening of the flower, in much rarer cases a short time after it has opened.

(5) When the visits of insects are prevented by a fine net, the flowers of the small-flowered form wither two or three

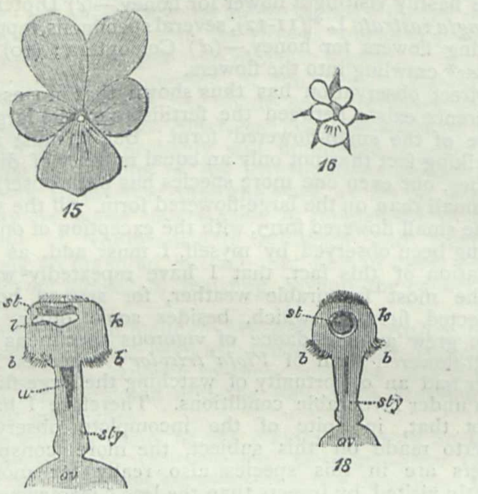

FIG. 15.—Front view of the more conspicuous flower of *Viola tricolor*, natural size. FIG. 16.—Front view of the less conspicuous flower. FIG. 17.—Pistil of Fig. 15, viewed on the under side, 12 times natural size. FIG. 18.—Pistil of Fig. 16. FIG. 19.—Lateral view of the pistil of Fig 15. FIG. 20.—Lateral view of the pistil of Fig. 16.

The following explanation of the lettering applies to all the figures:—
a, anthers; a^1, upper, a^2, lateral, a^3, lower anther; ap^1, appendage of the upper sepal; *b*, beard, *i.e.* tuft of hairs on the lateral surface of the skull-like crest of the style; *c*, appendage of the connective; *fi*, filaments; *k*, knob of the stigma; *l*, lip, labiated appendage of the stigmatic opening; *n*, nectary, *i.e.* honey-secreting appendage of the lower filaments; *op*, opening of the anther-cone; *ov*, ovary; *p*, petals; p^1, lower, p^2, lateral, p^3, upper petal; *po*, pollen-collecting hairs; *pr*, protective hairs (Sprengel's "Saftdecke"); *s*, sepals; s^1, upper sepal (with the appendage ap^1); s^2, lateral sepal; *sp*, the uppermost part of the spur, containing the honey; *st*, stigmatic cavity; *str*, streaks converging towards the opening of the flower; *sty*, style; *w*, wedge-shaped streak of the style; *y*, yellow coloured part of the lower petal.

days after opening, everyone setting a vigorous seed-capsule; those of the large-flowered form remain in full freshness more than two or three weeks, at length withering without having set any seed-capsule; when fertilised they wither also after two or three days.

Summary:—The more conspicuous flowers of *Viola tricolor* are adapted to regular cross-fertilisation by Lepidoptera, Apidæ, and Rhingia; whereas self-fertilisation by these visitors is prevented. Pollen-eating flies and diminutive insects crawling into the flower may effect both self- and cross-fertilisation; fertilisation by insects is possible from the opening of the flower for twenty days or more; spontaneous self-fertilisation never takes place. On the contrary the less conspicuous flowers are

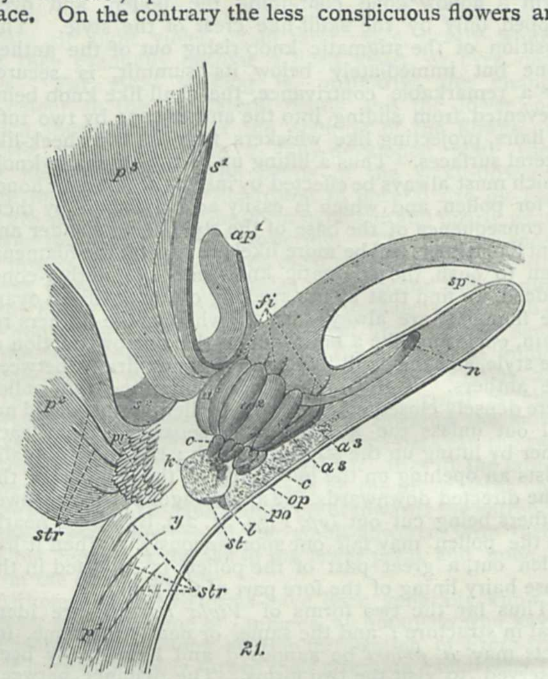

FIG. 21.—Lateral view of Fig. 15 after the half of its sepals and petals having been removed, 7 times natural size.

adapted to regular self-fertilisation; although visited now and then by the same insects as the more conspicuous flowers, cross-fertilisation by these visitors is by no means secured; in most cases it is even prevented by the pollen having previously fallen into the stigmatic cavity; it

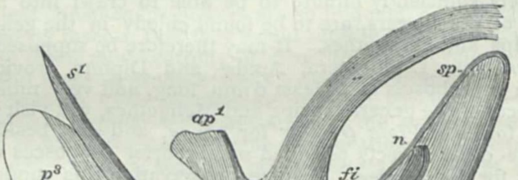

FIG. 22.—Lateral view of Fig. 16, but one lateral anther and the half of one lower anther have been removed and the pistil bisected longitudinally.

is possible only in those cases where the flower has opened before its pollen has filled the stigmatic cavity; and even in these rare instances the possibility of cross-fertilisation lasts but a few hours.

Lippstadt, October 1873 HERMANN MÜLLER

* In his interesting article on the Fertilisation of the Wild Pansy, NATURE, vol. viii. p. 49.

ON THE SCIENCE OF WEIGHING AND MEASURING, AND THE STANDARDS OF WEIGHT AND MEASURE *

VIII.

THE ordinary method of commercial weighing by putting the weights in one scale and the commodity to be weighed in the other, and then observing when a sufficient equilibrium is produced, is inadmissible for scientific weighings, as it is subject to errors arising from defects in the balance itself. To avoid any such errors, and obtain scientific precision in the results, a check is required which is found in a system of double weighing. There are two methods of double weighing for the comparison of two standard weights. One method, known as Borda's, and generally used in France, is that of *substitution*, or weighing first one of the standard weights to be compared, and then the other substituted for it, against a counterpoise placed in the other pan. The difference between the mean resting points of the index needle in these two weighings shows the difference of the two weights in divisions of the scale. The second method, known as Gauss's, but which was first invented by Le Père Amiot, and is now generally used in England and Germany, except for hydrostatic weighings, is that of *alternation*, or first weighing the two standards against each other, and then repeating the weighings, after interchanging the weights in the pans. By this second method no counterpoise weight is required, and *half* the difference between the mean resting points of the index needle shows the difference of the two weights, in divisions of the scale.

In all scientific weighings of standards with balances of precision, it is necessary that the weights to be compared should be so nearly equal that neither pan shall absolutely weigh down the other. The balance must merely oscillate so that the pointer does not exceed the limits of the index scale. In order to obtain an equipoise within this limit, it is requisite to provide small balance weights, most accurately verified, to be added to either pan, as may be found necessary.

The mode of reading adopted by the best authorities in the process of weighing by Gauss's method is as follows: —The comparing standard being in the left-hand pan, and the compared standard in the right-hand pan, and sufficient equipoise being obtained by adding small balance weights, if requisite, the balance is put in action, and the movement of the needle observed through a telescope. The reading at the first turn of the pointer is disregarded. The three next turns are noted, and the reading at the third turn of the pointer, and half the sum of the readings at the second and fourth turns are taken as the highest and lowest readings. Their mean is the resting point of the balance, or the reading of its position of equilibrium. The balance is then stopped, and the weights interchanged, when similar readings are taken and dealt with in the same manner. These two observations constitute one comparison. In cases where great accuracy is required, several successive comparisons are taken, in order to obtain a mean result. Some additional weighings are taken after adding a small balance weight to either pan, in order to ascertain the value of a division of the index scale. And if this balance-weight be added successively to each pan the weighings may be used as additional comparisons.

In using Gauss's method of weighing, it is very desirable to be able to transfer the pans and the weights contained in them from one end of the beam to the other without opening the balance case, and thus to avoid sudden changes of temperature of air within the balance case and consequent production of currents of air. For this purpose, the following plan is adopted. A grooved brass rod is fixed inside the balance case over and a little behind the beam. Upon this rod a brass slider is made to traverse by being attached to a slender brass rod drawn backwards or forwards from the outside of the case. A descending wire with a hook at the end is attached to the slider. For changing the weights, the slider and hook are brought to the right-hand end of the beam, when the pan and weight are lifted from the beam and transferred to the hook by means of a brass rod curved at the end and introduced through a small hole at the side of the balance case. The pan and weight are then slid to the other end of the beam, when the left-hand pan and weight are lifted in a similar manner from the beam and the right-hand pan and weight substituted. It only remains then to transfer the left-hand pan and weight to the right-hand end of the beam.

This method possesses a further advantage. In making a great number of comparisons between two standard weights, they are exposed to some risk of being injured

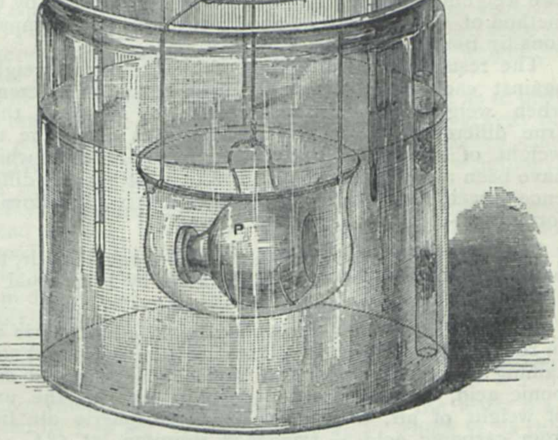

FIG. 18.—Mode of hydrostatic weighing (one-third size).

by wear, if they are taken up in the ordinary way with a pair of tongs. This risk is obviated by their being kept in the pans when lifted. Two light pans are used of as nearly as possible equal weight, each of which has a loop of wire forming an arch with the ends attached to the opposite sides of the pan, so that it can be easily lifted with the curved end of a brass rod. The pans are marked X and Y respectively. By interchanging the weights in the pans after a series of comparisons, and making a second series and taking the mean result, it gives the difference between the two weights, unaffected by any possible difference in the weight of the two pans. This contrivance is especially useful, when either of the weights to be compared consists of several separate weights. It was used by Prof. Miller for all his more important weighings during the construction of the imperial standard pound.

* Continued from p. 555.

The advantage possessed by Gauss's method of alternation over Borda's method of substitution has been proved by Prof. Miller as follows :—

Let P and Q be two standard weights of the same denomination to be compared, and C the counterpoise of each.

For Borda's method, let the readings of the index be denoted by (C, P), when C is in the left pan and P in the right pan, and by (C, Q), when C is in the left pan, and Q in the right pan.

For Gauss's method, let (Q, P) denote the readings when Q is in the left pan and P in the right, and (P, Q), when P is in the left pan and Q in the right pan.

Let e be the probable difference between the recorded and the true position of equilibrium, that is to say, the probable error of a *single weighing* (not of a comparison, which requires two weighings).

Then by Borda's method, (C, P) has a probable error e, and (C, Q) has a probable error e; and the two weighings give the value of P − Q with a probable error of $\sqrt{(e^2 + e^2)} = e\sqrt{2}$.

By Gauss's method, (Q, P) has a probable error e, and (P, Q) has a probable error e; and the two weighings give the value of P − Q with a probable error of $\frac{e}{2}\sqrt{2}$.

Thus the probable error of the result of two weighings by Borda's method is twice as great as by Gauss's method.

To obtain a value of P − Q by Borda's method with a probable error of $\frac{e}{2}\sqrt{2}$, we must make four comparisons of two weighings each. Therefore one comparison by the method of Gauss gives as good a result as four comparisons by Borda's method.

The result of this weighing of two standard weights against each other gives only their apparent difference when weighed in air. In order to ascertain their true difference, it becomes necessary to determine the weight of air displaced by each, from the data which have been already mentioned, and to allow for any difference of weight of air displaced, according to the following formula :—

If the weights P and Q appear to be equal in air, the weight of P − weight of air displaced by P is equal to the weight of Q − weight of air displaced by Q.

In determining the weight of ordinary atmospheric air in rooms where standard weights are compared, and containing a certain quantity of aqueous vapour and carbonic acid, the practice has been to take, as the unit of weight of air, a litre of dry atmospheric air free from carbonic acid, = 1·2932227 gramme, at 0° C., as determined by Ritter from the observations of M. Regnault in Paris, lat. 48° 50′ 14″, and 60 metres above the level of the sea, under the barometric pressure of 760 millimetres of mercury. Assuming that atmospheric air contains, on an average, carbonic acid equal to 0·0004 of its volume, and the density of carbonic acid gas being 1·529 of that of atmospheric air, the weight of a litre of dry atmospheric air containing its average amount of carbonic acid, under the stated circumstances, is 1·2934963 gramme.

Allowance should be made for the difference of the force of gravity in latitudes other than Paris, as well as for the difference of height of the place of observation above the mean level of the sea. Although the absolute weight varies with the latitude and with the height above or below the mean level of the sea, yet this variation is not felt in the comparison of standard weights in a vacuum, because the weights are equally affected on both sides of the beam. But in all weighings of standards in air requiring special accuracy, such variation must be taken into account in computing the weight of air displaced by each standard weight.

Mr. Baily has shown from his pendulum experiments * that if we take G to denote the force of gravity at the mean level of the sea in lat. 45°, the force of gravity in lat. λ, at the mean level of the sea

$$= G\,(1 - 0{\cdot}0025659 \cos 2\lambda).$$

And Poisson † has proved that the force of gravity in a given latitude at a place on the surface of the earth at the height z above the mean level of the sea—

$$= \left\{ 1 - \left(2 - \frac{3\rho'}{2\rho}\right)\frac{z}{r} \right\} \times \text{(force of gravity at the mean level of the sea in the same lat.)}$$

where r is the radius of the earth, ρ its mean density, and ρ' the density of that part of the earth which is above the mean level of the sea. If as is probable,—

$$\rho' : \rho = 5 : 11;\ 2 - \frac{3\rho'}{2\rho} = 1{\cdot}32 \text{ nearly};\ r = 6366198 \text{ metres},$$

it follows that the weight in grammes of a litre of dry atmospheric air containing the average amount of carbonic acid, at 0°, and under the pressure of 760 millimetres of mercury at 0°, at the height z above the mean level of the sea in lat. λ is—

$$1{\cdot}2930693\left(1 - 1{\cdot}32\,\frac{z}{r}\right)(1 - 0{\cdot}0025659 \cos 2\lambda).$$

At Cambridge, where Prof. Miller's observations for determining the weight of the new standard pound were made, in lat. 52° 12′ 18″, about 8 metres above the mean level of the sea (and for which place his tables were computed,) the weight of a litre of dry air containing the average quantity of carbonic acid was found by him to be 1·293893 gramme. This weight of air is therefore a little greater than at Paris. From similar data, after taking a further correction by Lasch of the weight of a litre of dry air at Paris = 1·293204 gramme, the weight of a litre of dry air at Berlin (lat. 52° 30′, and 40 metres above mean sea level) has been computed to be 1·29388 gramme.

The co-efficient of expansion of air under constant pressure between 0° and 50° C. is taken from Regnault's determination to be 0·003656 for 1° C., in other words between 0° and 50° C., the ratio of the density of air at 0° to its density at $t°$ is 1 + 0·003656 t.

With regard to the barometric pressure of the air and the allowance to be made for the pressure of vapour present in it, the density of the vapour of water is determined to be 0·622 of that of air ; that is to say, the ratio of the density of the vapour of water to that of air is 1 − 0·378.

Hence, if t be the temperature of the air, b the barometric pressure, v the pressure of the vapour present in the air, b and v being expressed in millimetres of mercury at 0° C., the weight of a litre of air at Cambridge becomes

$$\frac{1{\cdot}293893}{1 + 0{\cdot}003656\,t}\ \frac{b - 0{\cdot}378\,v}{760}$$

The ratio of the density of air to the maximum density of water is found by dividing the above expression by 1,000, as a litre of water is the volume of 1,000 grammes of water at its maximum density. Prof. Miller's Table I. gives the logarithms of this ratio at the normal barometric pressure of 760 millimetres, at the several degrees of temperature from 0° to 30°. These logarithms require to be diminished only by 0·000026 for weighings at the Standards Office, Westminster, lat. 51° 30′, and about 5 metres above the mean sea-level ; and when dimi-

* "Memoirs of the Astronomical Society," vol. vii. p. 94.

† "Memoires de l'Institut," tome xxi. pp. 91, 238.

nished by 0·000132, they may be used for the reductions of weighings at Paris.

The values of the pressure of vapour at the same temperatures in millimetres of mercury at 0°, according to Regnault's observations, are stated by Prof. Miller in a separate Table II. These values are given on the assumption that the pressure of vapour in rooms that are not heated artificially is two-thirds of the maximum pressure of vapour due to the temperature, as shown by the results of experiments on the authority of Biot, Regnault, and Bianchi.

The actual mode of ascertaining the weight of air displaced by two standard weights may now be described.

For determining the temperature of the air and of the two standard weights during the weighings, two standard thermometers are placed in the balance case, and their readings noted at the beginning and end of the weighings. The weight of air displaced by each of two standard weights is to be ascertained by the following formula:

Log. weight in grains of air displaced by P = log. h + log. At + log. $(1 + e\mathrm{P}t)$ + log. weight of P in grains − log. $\triangle$P.

Here t denotes the temperature of the air by the Centigrade thermometer;

b the barometric pressure of the air in millimetres of mercury at 0° C.;

v the maximum pressure of aqueous vapour contained in the air, also in millimetres of mercury;

$h = b - 0{\cdot}378 \times \frac{2}{3} v$;

At the ratio of density of air at $t°$ to the maximum density of water;

$e\mathrm{P}t$ the allowance for expansion in volume of P, or the ratio of its density at 0° to its density at t;

$\triangle$P the ratio of density of P at 0° to the maximum density of water.

By this formula, the required result is to be obtained. The logarithms of the three first terms may be found in Prof. Miller's tables, pp. 785-791 of his account of the construction of the new standard pound, Phil. Trans., part iii. of 1856.

Reference has already been made to the mode of ascertaining the volume or density of a standard weight by determining the difference of its weight in air and in water. The following practice for all such hydrostatic weighings was adopted by Prof. Miller when determining the densities of all the standard weights constructed under the sanction of the Commission for restoring the Imperial Standards, and is also followed in the Standards Department. In this process it is requisite to employ pure distilled water, and with this object the water used in the Standards Department is twice distilled in a still of the best construction, erected in the office, and the best chemical tests are employed for ascertaining that the water is free from any foreign substances.

The vessel for containing the distilled water is a glass jar, rather more than 6 inches in internal height and diameter. A stout copper wire is stretched across the mouth of the jar (see Fig. 18) in such a manner as to leave a circular space in the middle, large enough to admit the passage of the standard weight P, the density of which is to be ascertained. This copper wire supports two thermometers, adjustable as to their height, for determining the temperature of the water at the mean height of B during the weighings. It also serves to sustain a glass tube, open at both ends, and placed close to the side of the jar. A small glass funnel is inserted in the upper part of the tube, and in the lower part are one or two pieces of clean sponge.

The standard weight P is suspended from a hook under the right pan of the balance, specially constructed for hydrostatic weighings. A fine copper wire, the weight of which per inch is known, is attached to the hook by a loop, and has another loop at the other end. To this lower loop is attached a stout wire, bent and terminating in a double hook, which fits round P, and holds it securely. The counterpoise of P is next placed in the left pan of the balance. The glass jar is placed under the right pan of the balance, P being suspended in it, and the water is gently poured into the funnel and the jar filled to the requisite height above P. The bubbles of air are arrested by the pieces of sponge, and, ascending up the glass tube, are thus prevented from entering the jar. It is of importance to ascertain that no bubble of air is attached to P, and if so, it may generally be removed by the feather of a quill. But it sometimes happens that the weight P has an irregular surface, and air attaching to it cannot be thus dislodged. In such cases a small bell-shaped glass jar just large enough to hold P and its supporting wire, is used. This vessel is filled with water sufficient to cover P, and is suspended over the flame of a spirit lamp by a stout wire, bent at its lower end into a ring, into which the jar descends to its rim, and the water is allowed to boil until it is seen that the air has been entirely expelled. When cooled, the small jar containing P is immersed in the water, which nearly fills the large jar, and the small jar, with its wire, is then disengaged and lowered till P hangs clear of it, when it is removed. The transfer of P from the small to the large jar is thus effected without taking it out of the water.

For the actual weighing of P in water, after it has been counterpoised in air, weights equal to the difference of weight of P in water and in air, are placed in the right pan till equilibrium is produced, when the readings of the scale are observed. P is next removed, leaving its hook suspended in the water, and a volume of water equal to the volume of P is added to the water in the jar, so as to leave the same quantity of wire immersed as before. The requisite weights are then added to the right pan, until the equilibrium, which has been disturbed by the removal of P, is again produced, when the reading of the scale is observed and noted. This gives the actual weight in water of P.

The thermometers in the water are so placed as to give the temperature of the water at the centre of gravity of P. Another thermometer is placed in the balance case to give the temperature of the air during the weighings. The reading of the barometer is also noted.

Having determined the weight of P in air of ascertained density, its volume and density are calculated according to the following formula, the unit of volume being the volume of a grain weight of water at its maximum density:—

Let P in water at $t°$ appear to weigh as much as Q in air. Then the weight of water at $t°$ displaced by P = weight of P − weight of Q + weight of air displaced by Q.

Log. volume of P = weight in grains of the water displaced by P + log. W_t − log. $(1 + e\mathrm{P}_t)$; where W_t is the ratio of the maximum density of water to its density at t, and $e\mathrm{P}t$ is the expansion in volume of P at t. (The logarithms of these values are given in tables.)

Log. density of P = log. weight of P in grains − log. volume of P.

The actual weight of air displaced is to be ascertained by the method already stated.

As the true weight of P in air cannot be ascertained until its volume or density is known, an approximate value of the volume of P may be found by assuming the weight of P to be equal to its apparent weight in air; and this value of the volume of P may be used in reducing the weight of P, and thus a more accurate value of the volume of P obtained, by means of which a closer approximation to the values of the absolute weight of P, and of its density may be found. This process should be repeated when greater exactness is required.

H. W. CHISHOLM

(*To be continued.*)

EARTH-SCULPTURE*

AMONG the questions which may be treated as matters of strict science, and which yet cannot be wholly divested of the strong human, one might almost say personal, interest which belongs to them, is the birth of mountains and valleys. The familiar outlines of his dwelling-place have fixed the attention of man from the infancy of the race up to the present day. Long before science arose to deal with them they had become inwoven with his history, his habits, and his creed. The great mountains had been to him emblems of majesty and eternity, lifting up their fronts to heaven as they had done from the beginning, and would no doubt do to the end. They rose before him as monuments of the power of that great Being who had heaved them out of chaos. It was enough for him in that early time to feel their mighty influences; he had then no questions or doubts as to how or when they first appeared upon the earth.

Happily, in spite of questioning, exacting Science, these first natural and instinctive feelings are not yet dead within us. A knowledge even of all the laws of mountain-making cannot, if our minds are healthy and our hearts beat true, deprive us wholly of that first genuine child-like awe and wonder in presence of noble mountains,—crag and cliff sweeping in rugged and colossal massiveness above dark waves of pine, far into the keen and clear blue air;—the vast mantle of snow, so cloud-like in its brightness, yet thrown in many a solid fold over crest and shoulder; the dark spires and splintered peaks, half snow, half stone, rising into the sky, like very pillars of heaven; and then the verdure of the valleys below, the dash of waterfalls, the plenteous gush of springs, the laugh and dance of brook and river as they one and all hurry down to the plains—who can see these things for the first time, nay, for the hundredth time, without at least some sparkle of the simple child-like emotion of the olden time, or without appreciating, even if he cannot fully share, the feeling of the poet to whom they bring "dim eyes suffused with tears"?

These great dominant features of the land must indeed ever rivet our imagination, and yet when the questioning spirit of modern science asks to know how they came into being, we are no longer permitted to content ourselves with the early belief that they were but parts of the primæval outlines of the earth. The progress of inquiry and knowledge has destroyed that belief. We find, too, that both labour and patience are needed ere we can understand what has been put in its place. But the task of learning this is well repaid. However grandly the mountains rose when they were gazed at only in awe and wonder, they gain an added sublimity when the eyes which look upon them can trace some of the steps whereby their grim magnificence has been achieved.

We naturally associate the more lofty and rugged parts of the land with the operations of former earthquakes and convulsions by which the solid earth has been broken and ridged into these picturesque forms. This obvious inference was early adopted in geology, and though in many cases a mere belief rather than a legitimate deduction from observation, and springing from a conviction of what ought to be, rather than what has been proved to be the case, it has sturdily maintained its hold alike on the popular mind, and also to a very considerable extent in the orthodox geological creed.

Towards the end of last century, however, Hutton and Playfair, names never to be mentioned in Edinburgh without gratitude and pride, proclaimed views of a very different character. They maintained that the rocks of the land, originally accumulated under the sea, have been upheaved by underground movements, and without pretending to know in what external forms these rocks first appeared above the sea, they contended that the present contours of the land had arisen mainly from a process of sculpture,—the valleys having been carved out by rains, streams, and other superficial agents, while the hills were left standing up as ridges between. So satisfied were these bold and clear-sighted men that their idea was essentially true, that they gave themselves no concern in gathering detailed proofs in its support. They were content with general appeals to the face of nature everywhere as their best and irrefragable witness. But, as events proved, they were in advance of their time. The views which they promulgated on this subject were first opposed, then laid aside and forgotten. In the subsequent literature of the science for fully half a century they almost wholly disappear. An occasional reference to them may be met with, where, however, they are cited only to be dismissed, as if the writer seemed hardly able to restrain some expression of his wonder that men could ever have been found so Quixotic as to vent such notions, or that others could have been so gullible as to believe them.

Apart altogether from the truth or error of the Huttonian teaching regarding the origin of the earth's superficial features, no one who has the progress of geology at heart can regard without regret this almost contemptuous dismissal of the question from the range of scientific inquiry. For together with that teaching went all interest in, and even all intelligent appreciation of, the problem which Hutton had set himself to solve. Men turned back to vague notions about cataclysms, earthquakes, subterranean convulsions and fractures, of which they spoke, and sometimes still speak, with a boldness in inverse proportion to their knowledge of the actual conditions of the problem. They studied with praiseworthy assiduity and success the working of the various natural agents whereby the surface of the land is affected, but it was with the view rather of showing how the materials of new continents are gathered together, than of learning how the outlines of existing continents have been produced. The study of the origin of mountain and valley went out of fashion, and from the time of Playfair's Illustrations, published at the beginning of this century, received in this country but scant and haphazard attention until in recent years the subject has gradually revived, and has become one of the most prominent and interesting subjects of geological research.

It is not my purpose to give any historical sketch of the progress of inquiry on this question, although I ought not even to refer to it without an allusion to the names of Scrope, Ramsay, Jukes, Ruskin, Dana, Topley, Whitaker, Greenwood, the Duke of Argyll, Mackintosh, and others, who, though often differing widely in their views, have done so much to renew an interest in what will probably always prove one of the most alluring aspects of geology. Thoroughly convinced of the essential truth on which the Huttonian doctrines were based I wish, on the present occasion, first to define and illustrate some of the leading features of these doctrines as I hold them myself, and as I believe them to be held by the great body of active field geologists in Britain, and secondly, to review certain objections which have recently been reiterated against them.

At the outset it is necessary to ascertain what relation the internal arrangements of the rocks bear to the external forms of the land, in other words, the influence of what is called Geological Structure. It is obvious, as Hutton showed, that since the rocks have been formed as a whole under the sea, they must have been raised out of that original position into land, so that the first point we settle beyond dispute is that the mass of the land owes its existence to upheaval from below. But though we fix securely enough this starting point in our inquiry, it by no means follows that we thereby settle what was the original outline of the land so upheaved. The non-

* The Opening Address for the Session 1873-4 to the Edinburgh Geological Society, delivered Thursday, Nov. 6, by the President, Prof. Geikie, F.R.S.

recognition of this fact has involved not a few of the writers on this subject in great confusion and error.

Among the geologists of the present day there is a growing conviction that upheaval and subsidence are concomitant phenomena, and that viewed broadly they both arise from the effects of the secular cooling and consequent contraction of the mass of the earth. The contraction has not been uniform, as if the globe had been a cooling ball of solid iron. On the contrary, owing to very great differences in the nature and condition of the various parts of our planet and perhaps to features of the interior with which we are yet but imperfectly acquainted, some portions have sunk much more than others. These, having to accommodate themselves into smaller dimensions would undergo vast compression and exert an enormous pressure on the more stable tracts which bounded them. It could not but happen that after long intervals of strain, some portions of the squeezed crust would at length find relief from this pressure by rising to a greater or less height, according to their extent and the amount of force from which they sought to escape. These upraised areas would no doubt tend to occur in bands or lines across the direction of the pressure, much as the folds we produce in the sheets of an unbound book are more or less nearly parallel with the two sides from which we squeeze the paper. They would sometimes be broad folds—huge wide swellings of the earth's surface. At other times they might be long, lofty, and comparatively sharp ridges. In the one case they would give rise to high plateaux or table-lands, in the other they would be recognised as mountain-chains.

This is a rough-and-ready statement of what seems the probable explanation of the origin of the elevated tracts upon the earth's surface. It is evident that the pressure would be vastly greater a few hundreds or thousands of feet underground than at the surface, and hence that though the rocks deep down might be squeezed and crumpled, as we could crumple brown paper, yet that at the surface they might show little or no contortion. Certainly without further proof we could never affirm that a contorted mass of rock which now forms the surface of the ground rose as part of the surface during the time of upheaval and contortion. Intensely crumpled rocks would rather suggest a deeper position, with the subsequent removal of the rocks under which they originally lay.

As the earth has been cooling and contracting ever since it had a separate existence as a planet, its surface must have been exposed to a long series of such shrinkage movements as those we are considering. Apart, therefore, from local evidence, we should expect that ridges and depressions must have been impressed upon that surface in a long succession from the earliest periods downwards, and hence that the present mountain-chains and basins of the earth must be of many different ages. We cannot tell what the first mountains were made of, nor where they lay, although some of the existing ridges of the earth's surface are undoubtedly, even in a geological sense, very old. In not a few cases the same mountain-chain can be shown from its internal structure to be of many successive dates, as if it lay along a line of weakness which had served again and again as a line of relief from the severe earth-pressure.

These questions have been treated with much ability by Constant Prevost, Dana, Mallet, and others, to whose writings I refer for details. In stating them in this general way my object is to show that those geologists who, like myself, believe in the truth of the Huttonian doctrines of denudation, are most unfairly represented when they are said to ignore the influence of subterranean forces upon the exterior of the earth. None can recognise more clearly than they do how entirely have the great surface outlines of the globe been dependent upon the action of these forces, that is, upon the results which flow from the contraction of the planet and from the reaction of the heated interior upon the surface.

But a block of marble is not a statue, nor would a part of the earth's crust heaved up into land form at once such a surface of ridge, and valley, and nicely adjusted water system as any country of which we know anything on the face of the globe. In each case it is a process of sculpture, and the result varies not only with the tools but with the materials on which they are used. You would not expect the same kind of carving upon granite as upon marble. And so, too, in the great process of earth-sculpture, each chief class of rock has its own characteristic style. The tools by which this great work has been done are of the simplest and most everyday order—the air, rain, frost, springs, brooks, rivers, glaciers, icebergs, and the sea. These tools have been at work from the earliest times of which any geological record has been preserved. Indeed, it is out of the accumulated chips and dust which they have made, afterwards hardened into solid rock and upheaved, that the very framework of our continents has been formed. The thickness of these consolidated materials is to be measured, not by feet merely, but by miles. If the removed materials are so thick, they show what a vast mass of rock must have been carved away. And even before knowing anything of the way in which the various tools are used, we should be justified in holding it to be, at the least, extremely improbable that any land surface would long retain its original contour or even any trace of it.

But when we come to watch with attention how the tools really do their work, this improbability increases enormously. Adopting a method of inquiry suggested by Mr. Croll, I have elsewhere shown that even at their present state of progress the amount of geological change which they would accomplish in a comparatively small number of ages is almost incredible. On a moderate computation they would reduce the general mass of the British Islands down to the level of the sea in five or six millions of years, and might carve out valleys a thousand feet deep in a fourth part of that time. It is evident that though the upheaval of some parts of the continents may go back into the remotest geological antiquity, the forms of the present surface must be, comparatively speaking, modern.

There is reason to believe that many, if not most, of the great mountain chains of the globe are, in a geological sense, of recent origin. The Alps, for example, though they may have undergone many earlier movements, were ridged up into their existing mass long after the soft clays were laid down which cover so large an area of the low lands in the south of England, and on which London is built. It would require far more detailed work than has ever been bestowed upon these mountains to enable us even to approximate to what was the original form of the surface just after the upheaval, and before the array of sculpture-tools began their busy and ceaseless task upon these great masses of rock. We may believe that a series of huge parallel folds of curved and broken rock rose for thousands of feet into the air, that when, after the earth-throes had ceased, rain and snow and frost first laid their fingers on the new-born summits, these agents of destruction would have a most uneven surface to work upon, and would necessarily be guided by it in their working; and hence that some, at least, of the dominant earliest ridges and hollows would be perpetuated. Such a belief would carry probability in its favour, but it would certainly not amount to a proof of the supposed perpetuation. That would require to be corroborated by the internal and external evidence of the mountains themselves. In some tracts, as, for instance, among the singularly symmetrical ridges and furrows of the Jura, it would not be difficult to restore the original outline, and to fix exactly how far the subterranean movements had determined the present external forms of the ground, though even there, where this connection is so clear, we should see at the same time how greatly the tops and sides of the long saddle-shaped arches of rock have suffered from subsequent waste. But among the contorted, inverted, and broken rocks of the Central Alps the task would be infinitely more difficult.

We could not advance far, however, in such a quest before observing that one feature stands out conspicuously enough among the mountains, viz., that whatever might have been their original outlines, these were most certainly not the same as those which we see to-day. No part of the history of the ground can be made more self-evident than that, since the birth of these mountains, millions upon millions of cubic yards of rock have been worn off their crests and ridges, and carved out of their sides. There is not a cliff, crag, or valley along the whole chain of the Alps which does not bear witness to this great truth.

If then, even when dealing with the young Alps, we cannot be quite sure what were their first or infant features, how impossible must it be to decide as to the early outlines of such immensely more ancient uplands as those which date from palæozoic times! For, evidently, the higher their antiquity, and the longer, therefore, their exposure to ceaseless waste, the more must these outlines be changed. The general mass of land might still remain land, but trenched and furrowed and worn down, as the Alps are now suffering, until not a single vestige or indication of its first contour survived, the remaining portions being, as it were, merely the stump or base of what once was.

Now this is the position in which the question presents itself in Britain. The hills of the Highlands and Southern Uplands of Scotland, of the Lake district, and of Wales, are not mountains in the same sense as the Alps or Pyrenees, or other great continental mountain-chains. However much these long lines of elevated ground may have had their outlines modified by the universal waste of the earth's surface, their linear character, the general parallelism of their component ridges, the undulations of the strata along their flanks, as well as their internal geological structure, bear witness to the fact that they are but huge wrinkles upon the shrivelled globe—tracts which have been thrust up while the neighbouring regions have sunk down. But in Britain these characteristic features are wanting. In all probability there never was any true mountain-chain in our region. There is good reason to believe that in very ancient times, that is to say, previous to the Old Red sandstone, a wide plateau-like mass of land was upraised on the north coast of Europe, surviving portions of it being represented by the detached hilly regions of Britain and the great table-land of Scandinavia. The rocks underlying this upheaved tract underwent, at the time of elevation, enormous compression and consequent contortion. This could not happen without an infinite amount of resistance. The heat thus evolved among the grinding masses may have been amply sufficient even to melt them in part. And no doubt it was during this process that they became crystalline over such wide areas, and were injected with granite and other melted products. But all this had been wholly, or almost wholly, completed before the time of the Old Red sandstone, for the deposits of that geological system are formed out of the older altered rocks, and lie undisturbed upon them. Even now, in spite of all the subsequent denudation, the patches of old red conglomerate which remain show to what an extent the older rocks had been buried under it, for they are found rising here and there to a height of 2,000 or 3,000 ft. above the sea. But they prove further, not only that the contortion of the underlying rocks preceded the Old Red sandstone, but that these rocks had suffered a vast extent of waste at the surface, before even the oldest visible parts of the conglomerate were deposited upon them. This waste has been in progress ever since. We need not, therefore, hope to discover any vestige of the aboriginal surface. A geological section drawn across any part of the hills proves beyond question that the general surface of the country has had hundreds or even thousands of feet of solid rock worn away from it. Such a section shows moreover that our present valleys are not mere folds due to underground movements, but are really trenches out of which the solid rock has been carried away.

So far, this is a question of simple fact, and not merely of opinion. The language of Hutton may be literally true of Britain:—"The mountains have been formed by the hollowing out of the valleys, and the valleys have been hollowed out by the attrition of hard materials coming from the mountains." Our British hills, unlike the chains of the Jura and the Alps, are simply irregular ridges depending for their shape and trend upon the directions taken by the separating valleys. The varying textures of the rocks, their arrangements with relation to each other, their foldings and fractures, and the other phenomena comprised under what is termed "geological structure," have greatly modified this result, but the process has nevertheless, as I believe, been one of superficial sculpturing, and not of subterranean commotion and upheaval. On the details of this process it is not needful to dwell.

From these cursory statements, which express, I believe, the general concurrent opinions of the modern Huttonian school, it should be clear how far that school must be from ignoring the influence of subterranean forces. Hutton himself never did so, and his followers now know far more of these forces than he did. But on the other hand, they claim for the surface-agents in geology a potency great enough to cut down table-lands into mountain-ridges and glens, to carve out the surface of the land into systems of valleys, and in the end to waste a continent down to the level of the sea.

(*To be continued.*)

ASTRONOMY AT OXFORD

DR. DE LA RUE having, in the course of last summer, made a munificent offer of several astronomical instruments and apparatus, including a large reflecting telescope, to the University, the subject was brought under the consideration of the delegates of the Museum, who, at their first meeting in this term, appointed a committee to "report on the desirability of accepting the munificent offer of Dr. De La Rue to present to the University his celebrated reflecting telescope, on the probable cost of a building to receive the instrument, and on the precise purposes for which this instrument may be usefully employed, in distinction to the refracting telescope now being set up."

The committee, after full and careful examination of the whole subject, have sent in a report, to which they have unanimously agreed, and which the delegates recommended, with entire confidence, to the favourable consideration of the council. In consequence of this report, the following forms of decree will be submitted to a convocation to be held on Thursday, Nov. 27:—

1. That the reflecting telescope and other apparatus offered to the University by Dr. De La Rue be accepted; and that the Vice-Chancellor be requested to return the thanks of the University to Dr. De La Rue for his munificent gift. And that the curators of the University chest be authorised to pay to the delegates of the University Museum a sum not exceeding 1,500*l.*, to be expended by them on the erection of buildings in the park suitable for the reception and use of the telescope and other apparatus presented by Dr. De La Rue, as also of the instruments at present in the small observatory on the east side of the museum, according to plans and specifications prepared by Mr. Charles Barry, architect, and adjoining the observatory now nearly completed.

2. That the curators of the University chest be authorised to pay annually to the Savilian Professor of Astronomy during five years, or until provision is made from some other source, the sum of 200*l*. for providing an assistant and defraying the expenses incurred in the maintenance and use of the instruments in the observatory, an account of the expenditure of such sum to be annually submitted to the auditors of accounts.

We cannot doubt that Convocation will sanction a decree which promises to make Oxford first in the field in this country in the power of aiding the new astronomy which is dawning upon us—thanks to the spectroscope and the application of photography.

Such a position may not be thought much of now, but in the coming time Oxford men will refer to it as one of the things of which Oxford has the greatest reason to be proud.

NOTES

THE Copley Medal and the two Royal Medals in the gift of the Royal Society, have this year been awarded as follows:—The Copley Medal to Prof. Helmholtz, the distinguished physiologist, physicist and mathematician, of Berlin; a Royal Medal to H. E. Roscoe, F.R.S., Professor of Chemistry in Owens College, Manchester; and a Royal Medal to Dr. Allman, Professor of Biology in the University of Edinburgh.

THE Annual Meeting of the Royal Society will be held on December 1, when, after dining together, the Fellows will adjourn to their new apartments.

A DEPUTATION from the Council of the Society of Arts had an interview on Friday last with the Royal Commissioners of Scientific Instruction with reference to museums and galleries of science and art. The deputation consisted of Major-General F. Eardley-Wilmot, R.A., F.R.S. (Chairman of the Council), Mr. E. Chadwick, C.B., Colonel Croll, Mr. Hyde Clarke, the Rev. Septimus Hansard, Admiral Ommanney, C.B., F.R.S., Colonel Strange, F.R.S., Mr. Seymour Tewlon, with Mr. Le Neve Foster, Secretary. The Chairman of the Council stated that the object the Council had in view was to bring before, and ask the support of, the Commissioners to the action the society was now taking in reference to museums, and pointed out that this had special regard to the State giving increasing aid to existing museums, to aid in the multiplication of such museums, and rendering them available for educational purposes. He further pointed out the necessity for all such museums being placed under the control of a Cabinet Minister responsible to Parliament. He handed to the Commissioners a copy of resolutions embodying the views of the Council, stating at the same time that a large and influential committee was in the course of formation, and that a considerable number of members of both Houses of Parliament had already given in their names.

THE first award of the Grand Walker prize of 1,000 dols. was voted by the Council of the Boston Society of Natural History on October 1, to Alexander Agassiz, of Cambridge, U.S.A., for investigations on the embryology, structure, and geographical distribution of the Radiata, and especially of the Echinoderms, and the publication of the results as embodied in his recent work. The Annual Walker Prize of 60 dols. for 1873 was at the same meeting awarded to A. S. Packard for his essay on the development of the common house-fly. For the Annual Prize of 1874, the subject is "The Comparative Structure of the Limbs of Birds and Reptiles." Memoirs offered for competition must be forwarded on or before April 1, addressed to the Boston Society of Natural History, for the Committee of the Walker prizes, Boston, Mass., U.S.A., and each memoir must be accompanied by a sealed envelope enclosing the author's name, and superscribed by a motto corresponding to one borne on the M.S.

IN the examination for Foundation Scholarships at Trinity College, Cambridge, to be held at Easter, 1874, one or more Scholarships will be obtainable by proficiency in the Natural Sciences. The Examination in Natural Science will commence on Friday, April 10, and will include the subjects set forth in the regulations for the Natural Sciences Tripos. It will be open to all undergraduates of Cambridge or Oxford, and to persons not members of the Universities, provided that these last are under twenty years of age. Candidates who are not members of Trinity College must send their names to the Master, together with a certificate of age and good character, on or before Saturday, March 21.

WE congratulate the University of Edinburgh on being the first in the United Kingdom to recognise the duty of universities so to frame their regulations for degrees in science as to encourage original work in opposition to mere book-knowledge. The University of Edinburgh has just issued a regulation that every candidate for the degree of Doctor of Science shall in future be required to submit a Thesis containing some original research on the subject of his intended examination, and that such thesis shall be approved before the candidate is allowed to proceed to examination.

PROF. CHEVALLIER, for many years Professor of Mathematics and Astronomy in the University of Durham, died on the 4th inst., at the age of 80 years.

WE learn from *Ocean Highways* that Prof. Mohn, of the Meteorological Institute at Christiania, and Mr. O. Sars are preparing a plan for the investigation of the sea between Norway, the Farö Islands, Iceland, and Spitzbergen, the expense of which will, it is expected, be defrayed by a grant of the Norwegian Storthing.

DR. RUDOLPHE WOLF has recently published in the *Vierteljahrschrift* of the Zurich Society of Natural Science, the thirty-third number of his *Astronomische Mittheilungen*. The paper is important in reference to sun-spots chiefly, and as bringing out with great clearness the connection of these with variations in declination of the magnetic needle. The author gives a series of daily observations of sun-spots, during 1872, made at Zurich, Peckeloh, Münster, Palermo, and Athens. The mean relative number obtained is 101·7; and for the years 1866–72 inclusive, the series runs thus:—16·3, 7·3 (min. 1867), 37·3, 73·9, 139·1 (max. 1870), 111·2, 101·7. Dr. Wolf has constructed a formula by which the average yearly variations of magnetic declination, in a particular place, may be calculated from the relative sun-spot number (two constants for the place being given). In this way, for example, he obtains for Munich the quantity 10′·80 as representing the magnetic variation for 1872; the number got from observation is 10′·75, showing a close agreement. In the second portion of his paper Dr. Wolf discusses several points connected with the history of the telescope, the vernier, the pendulum clock, &c.; among other things, attributing to Bürgi (who lived in the early part of the sixteenth century), a share in the discovery of the isochronism of the pendulum. The last portion of the paper reproduces some of the earlier sun-spot literature. The same number of *Astronomische Nachrichten* contains a note by M. von Asten, furnishing evidence against the supposed identity of a cometary object observed by Goldschmidt on May 16, 1855, with Tempel's comet. (1867, II.)

THE recent meeting of the American Association for the Advancement of Science held at Portland, Maine, was considered on the whole a successful one. 157 papers were entered, and abstracts were received of all but nine; most of the remainder were passed by the sectional committees for reading, but a number of those that were read were not approved by the committees for publication, an example that might be very usefully followed in the case of our British Association. The general character of the meeting was stated to be decidedly scientific, and the discussions to have been carried on with good feeling, and free from personalities; though complaint was made that less sympathy was exhibited on the part of the citizens with the objects of the Association than at any previous meeting. The next meeting will be held at Hartford, Connecticut, on the second Wednesday in August 1874, when a report will be received from a special committee appointed to revise the constitution of the Association with a view to a better carrying out of its objects. The general officers for the meeting will be Dr. J. L. LeConte, president; Prof. C. S. Lyman, vice-president; Dr. A. C. Hamlin, general secretary; and Mr. J. W. Putnam, permanent secretary.

DR. BEKE writes to the *Times* as follows with respect to Dr. Livingstone:—"If the intelligence from the West Coast of Africa is to be depended on, we may very shortly expect the return of our great traveller, Dr. Livingstone, to his native country On the 1st and 4th inst. you inserted communications from me, to the effect that our countryman was detained a prisoner at a place about 300 miles from Embomma, on the Congo. According to the news brought by the last African Royal mail steamer, it was reported at St. Salvador that Livingstone was then in the interior, about 30 or 40 miles from that place. Now, as St. Salvador is only 80 miles from Embomma, the distance to the latter town from the spot at which, according to the later intelligence, our adventurous countryman was, is not more than 120 miles; and, Embomma being 70 miles from the mouth of the Congo, he would have been within 200 miles of the coast. As the hardy and energetic traveller is not in the habit of letting the grass grow under his feet, he may well be supposed to have come on nearly, if not quite, as quickly as the natives who brought the news of his whereabouts. Consequently, on the assumption that the intelligence received is founded on truth, we may not unreasonably look for the veteran traveller's arrival in England by the next mail steamer from the West Coast of Africa."

WE learn from the Journal of the Society of Arts, that one of the first results in the rise of the price of coal has been the formation of a company in France, whose object is to utilise the power of the ocean tides on the French coast by proper machinery. The first experiment is to be made at St. Malo, where the tide rises nearly 80 ft., and overflows many square miles of flats.

DR. GEORGE BURROWS, F.R.S., has been appointed one of the Physicians-in-Ordinary to Her Majesty, in the room of the late Sir Henry Holland.

AT a meeting of the Trustees of the Hunterian Collection of the College of Surgeons, held on Saturday, 8th inst., George Busk, F.R.S., was elected a member of the board, to fill the vacancy occasioned by the death of the Bishop of Winchester.

DR. LYON PLAYFAIR, C.B., F.R.S., M.P. for the Universities of St. Andrews and Edinburgh, has been appointed Postmaster-General in succession to Mr. Monsell. Dr. Playfair was a pupil of Liebig, was formerly Professor of Chemistry in the University of Edinburgh, and was at one time Government Inspector-General of Schools and Museums of Science and Art. We hope the new Postmaster-General will endeavour to introduce something like scientific method into the postal department.

THE promoters of the railway tunnel which is intended to cross the Mersey, the shafts for which have already been sunk, have always believed that they would have only a continuous mass of solid sandstone rock to penetrate. A paper has just been published in the transactions of the Liverpool Geological Society for 1872, by Mr. T. Mellard Reade, C.E., of Liverpool, in which he contends that in all probability a deep gorge, filled up with clay or sand, will be met with, being the site of an ancient river or torrent formed in or before the times when England was covered with ice, and when its valleys were filled with glaciers. Mr. Reade believes that the ascertained data warrant the hypothesis, that before the boulder clays and other recent strata were laid down, a river draining the land now drained by the Mersey flowed past Runcorn Gap, between land of some considerable elevation, to the sea.

WE have received, in the form of a neat little pamphlet of 20 pp., price only one penny, an exceedingly interesting lecture on "How Flowers are Fertilised," delivered by Mr. A. W, Bennett, F.L.S., at Manchester, on the 5th inst. It is one of a series of Science-Lectures for the People, published after delivery by Mr. Heywood of Manchester; they are carefully and neatly printed, and judging from the one before us, purchasers have a very good pennyworth indeed. The enterprise is very creditable to the publisher.

AMONG the papers presented to Parliament, says the *Times*, relating to the South Sea Islanders, is a report by Captain C. H. Simpson, of Her Majesty's ship *Blanche*, giving an account of his visit last year to the Solomons and other groups of islands in the Pacific Ocean. While at Isabel Island. Captain Simpson, with a party of officers, went a short distance inland to visit one of the remarkable tree villages peculiar, he believes, to this island. He found the village built on the summit of a rocky mountain rising almost perpendicular to a height of 800 ft. The party ascended by a native path from the interior, and found the extreme summit a mass of enormous rocks standing up like a castle, among which grow the gigantic trees, in the branches of which the houses of the natives are built. The stems of these trees lie perfectly straight and smooth, without a branch, to a height varying from 50 ft. to 150 ft. In the one Captain Simpson ascended the house was just 80 ft. from the ground; one close to it was about 120 ft. The only means of approach to these houses is by a ladder made of a creeper, suspended from a post within the house, and which, of course, can be hauled up at will. The houses are most ingeniously built, and are very firm and strong. Each house will contain from ten to twelve natives, and an ample store of stones is kept, which they throw both with slings and with the hand with great force and precision. At the foot of each of these trees is another hut, in which the family usually reside, the tree-house being only resorted to at night and during times of expected danger. In fact, however, they are never safe from surprise, notwithstanding all their precautions, as the great object in life among the people is to get each other's heads.

THE additions to the Zoological Society's collection during the past week include an Alligator Terrapin (*Chelydra serpentina*) from North America, presented by the Smithsonian Institution of Washington; a large Hill Mynah (*Gracula intermedia*) from North India, presented by Rev. T. Main; twelve Gray's Terrapins (*Clemmys grayi*) from Bussorah, presented by Captain Phillips; a Changeable Tree Frog (*Hyla versicolor*) from North America, presented by Prof. Rolleston; a Ground Rat (*Aulacodus swinderianus*) from West Africa; a Sharp-nosed Badger (*Meles leptorhynchus*) from China; a Telerang Squirrel (*Sciurus bicolor*) from the East Indies; two Mantchurian Crossaptilons (*Crossaptilon mantchuricum*) from North China, and two Blue-rowned Hanging Parrakeets (*Loriculus galgulus*) from Malacca, purchased; an Agile Gibbon (*Hylobates agilis*) from Sumatra, deposited.

SCIENTIFIC SERIALS

THE November number of the *Monthly Microscopical Journal* commences with a paper by Dr. R. L. Maddox on an organism found in Fresh-pond Water, which he thinks to be new. The accompanying illustration, as well as the description, shows that the monads under consideration are of the simplest structure, and amœboid in character, of a violet tint, and highly refracting. They vary in size, and contain great numbers of little granular bodies embedded in the gelatinous matrix. The name *Pseudo-amæba violacea* is proposed for the new form.—Mr. F. Kitton describes some new species of Diatomaceæ, including *Aulacodiscus superbus* from Barbadoes, and others of the genera *Sticto-discus*, *Isthmia*, *Nitzschia*, and *Tryblionella*.—Mr. Carruthers answers Dr. Dawson's comments on his interpretation of the microscopic appearances of *Nematophycus* (Carruthers) or *Protolaxites* (Dawson). As he remarks, the question whether the plant under consideration is a sea-weed or a conifer, is entirely an histological one. Dr. Dawson, in his sections of the fossil found "wood cells, showing spiral fibres and obscure pores;" Mr. Carruthers finds "elongated cylindrical cells of two sizes, interwoven irregularly into a felted mass," and the latter observer substantiates the correctness of his observations and his drawings, which prove the accuracy of his views as to the affinities of the plant.—Mr. J. J. Woodward explains the optical principles involved in the construction of Mr. Tolles' new immersion objective that has caused the contest between him and Mr. Wenham.—Dr. Braithwaite continues his description of bog mosses, treating of figuring *Sphagnum rigidum* and *S. molle*.—This paper is followed by one on the investigation of Microscopic Forms by means of the images which they furnish of external objects, by Prof. O. N. Rood, of Troy, N.Y., which gives an extremely ingenious and simple method of testing with certainty, when the refractive indices of the body examined and the fluid in which it is immersed, are known, of determining whether markings, as of *Coscinodiscus triceratium*, are depressions or elevations; by regarding the object as part of the optical system, and thence finding whether its influence is that of a convex or concave lense.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, Nov. 5.—Prof. Ramsay, F.R.S., vice-president, in the chair.—The following communications were read:—"On the Skull of a species of *Halitherium* from the Red Crag of Suffolk," by Prof. W. H. Flower, F.R.S. A description of this has been already given in NATURE, at p. 13 of the present volume.—"New Facts bearing on the Inquiry concerning Forms intermediate between Birds and Reptiles," by Henry Woodward, F.R.S. The author, after giving a brief sketch of the Sauropsida, and referring especially to those points in which the Pterosaurians approach and differ from birds, spoke of the fossil birds and land reptiles which he considered to link together more closely the Sauropsida as a class. The most remarkable recent discoveries of fossil birds are:—(I.) *Archæopteryx macrura* (Owen), (II.) *Ichthyornis dispar* (Marsh), (III.) *Odontopteryx toliapica* (Owen). The author then referred to the Dinosauria, some of which he considered to present points of structure tending towards the so-called wingless birds. (I.) *Compsognathus longipes* (A. Wagner), from the Oolite of Solenhofen. (II.) The huge carnivorous *Megalosaurus*, ranging from the Lias to the Wealden. The author next drew attention to the Frilled Lizard of Australia, *Chlamydosaurus Kingii* (Gray), which has its fore limbs very much smaller than the hind limbs, and has been observed not only to sit up occasionally, but to run habitually upon the ground on its hind legs, its fore paws not touching the earth, which upright carriage necessitates special modifications of the sacrum and pelvis bones. The Solenhofen Limestone, in which Pterosauria are frequent, and which has yielded the remains of *Archæopteryx* and of *Compsognathus*, has also furnished a slab bearing a bipedal track, resembling what might be produced by *Chlamydosaurus* or *Compsognathus*. It shows a median track formed by the tail in being drawn along the ground; on each side of this the hind feet with outspread toes leave their mark, while the fore feet just touch the ground, leaving dot-like impressions nearer the median line. Hence the author thought that while some of the bipedal tracks which are met with from the Trias upwards may be the "spoor" of struthious birds, most of them are due to the bipedal progression of the Secondary Reptiles.—"Note on the Astragalus of *Iguanodon Mantelli*," by J. W. Hulke, F.R.S. The author exhibited and described an astragalus of *Iguanodon* from the collection of E. P. Wilkins. The bone was believed to be previously unknown. The upper surface presents a form exactly adapted to that of the distal end of the tibia, so that the applied surfaces of the astragalus and tibia must have interlocked in such a manner as to have precluded all motion between them. The author remarked upon the interest attaching to this fact in connection with the question of the relationship between the Dinosauria and Birds.—"Note on a very large Saurian Limb-bone, adapted for progression upon land, from the Kimmeridge Clay of Weymouth, Dorset," by J. W. Hulke, F.R.S. The bone described by the author presents a closer resemblance to the Crocodilian type of humerus than to any other bone, and he regarded it as the left humerus of the animal to which it belonged. The author refers it provisionally to a species of *Ceteosaurus*, which he proposes to name *C. humero-cristatus*.—A despatch from Mr. Alfred Biliotti, British Vice-Consul at Rhodes (dated June 16, 1873), communicated by H.M. Secretary of State for Foreign Affairs, and relating to a volcanic outburst in the island of Nissiros, one of the Sporades, in which there existed a volcano supposed to be extinct. Shortly before June 10 new craters opened in this volcano, and from them ashes, stones, and lava were ejected; many fissures, from which hot water flowed, were produced in the mountain, and the island was daily shaken by violent earthquakes.

Royal Astronomical Society, Nov. 14.—Prof. Cayley, president, in the chair. Sir Geo. B. Airy, the Astronomer-Royal, explained the general state of the preparations for the transit of Venus. First, as to the selection of stations. He had originally selected five observing-stations, and in making his choice he had endeavoured to keep in mind what other Governments were likely to do. He had been induced to recommend another station in Northern India for the purpose of taking a series of photographic observations to be used in conjunction with the photographic records to be obtained at the southern stations. As the French would not support the station which he had selected in the Sandwich Islands, by an expedition to the Marquesas Islands, he had found it necessary to recommend to our own Government that there should be two subsidiary observing stations in the Sandwich Islands. The station which had originally been chosen was Honolooloo, at about the middle of the islands; the new stations were to be Ha-wai-i to the east and an island at the western extremity of the group. The three stations would thus be distributed over a distance of some 300 miles—a fact which would greatly add to their chances of fine weather. He had also been considering the propriety of establishing stations at Christmas Island, at Hurd Island, and in Whisky Bay, but at present they knew little of the chances of anchorage or fine weather at these places. The *Challenger* was, however, about to visit and survey them. It would then proceed to Australia, whence the results of their investigations would no doubt be telegraphed to England. As to the selection of stations in the extreme south, the Admiralty would have nothing to do with any station where there was no anchorage, and where there were no human beings. Any station which laboured under both disqualifications must undoubtedly be rejected as unsuitable. He felt himself borne out in this determination by the fact that other nations had adopted the same practical view in their selection of stations. The Astronomer Royal then enumerated and pointed out upon a globe the stations which had been selected: 8 American, 5 French, 4 German, 19 Russian, and 8 English, besides the private enterprise of Lord Lindsay. He then proceeded to give a description of the now well-known "black drop," which was sometimes described as being so large as to make Venus appear "pear-shaped," at other times the illegitimate connection between Venus and the limb consisted only of a narrow black strap or band. The Astronomer-Royal had had a working model prepared at Greenwich with a black disc moved by clock-work. The black ligament, or drop, came out as a very marked feature of the contact with the artificial limb. And he hoped that Capt. Tupman would be able, from a discussion of the observations of different observers with different telescopes, to determine in what proportion the phenomenon was due to the aperture of the telescope used, and to what he might call the personal equation of the observer. He then proceeded to explain how when Venus was upon the sun's limb measures are to be made of the common chord of Venus and the limb, and how these measures are affected by the formation of a "black drop" between the two images.—Lord Lindsay then showed some photographs of a model of Venus upon the limb, in which the "black drop" was photographed as a remarkable feature. He pointed out that when the exposure was longest the "black drop" was most marked; and he showed that its size might be greatly reduced by using a stop which only permitted the rays from the central parts of the lenses to reach the plate. Dr. De La Rue said it was quite wonderful to see the amount of preparations which were going forward at Greenwich. It was not right to throw out such insinuations as Mr. Proctor had done about "official obstructiveness." Mr. Proctor's last paper in the *Monthly Notices* was a disgrace to the Society. In former days such papers never appeared.—A paper was read by Mr. Lassell on the finding of longitude with small instruments.—Mr. Ranyard then read a note upon a remarkable spot observed by Pastorff upon the sun's disc of May 26, 1828. In June 1819 Pastorff observed a nebulous spot with a bright nucleus upon the sun, which has since been recognised as being the comet of 1819 projected upon the bright background of the photosphere. The drawing referred to by Mr. Ranyard contained a similar though smaller nebulous marking, with a bright centre. His object in bringing the drawing to the notice of the society was to inquire whether any small comet or known meteoric stream was between the earth and the sun on May 26, 1828.

Anthropological Institute, Nov. 11.—Prof. Busk, F.R.S., president, in the chair.—Mr. T. J. Hutchinson, F.R.G.S., H.M.'s consul at Callao, read a paper on "Explorations amongst ancient burial grounds, chiefly on the sea-coast valleys, of Peru," Part I. The object of the paper was to describe the "huacas" or burial-grounds, especially those lying beweent Arica and the Huatica Valley, and to expose some popular errors respecting them. Every bit of old wall, every heap of gravel, mound of earth, large or small cluster of ancient ruins of any kind is there called a "huaca." The term huaca (Quichua) is synonymous with Quilpa (Aymara) and means "sacred;" the title may therefore be considered as much applicable to the burying-grounds of Ancon, Pasamayo, and other places where there is no elevation above the country, as to those of Pando and Ocharán, large burial mounds in the valley of Huatica. The author proceeded to describe in detail the mode of interment and the various articles discovered. The celebrated Pacha-Cámác was described. Along the whole course of the Huatica Valley—from Callao to Chorillos—a distance of ten miles direct or sixteen miles round by Lima, there is no natural elevation that could be made available as a sub-structure for those colossal burial mounds. He gave at considerable length his reasons for concluding that there was no "Temple of the Sun" and no "House of the Virgins" of the Inca religion, and that every huaca was not a "Huaca de los Incas."—Dr. Simms, of New York, gave a most interesting and instructive communication on a flattened skull from Mameluke Island, Columbia River, and described minutely the practice of flattening the head in infancy. In reply to questions put to him, he said that the flattening does not seem to cause pain; that males and females are treated alike, although it had been supposed only males were so treated; that flattening is not apparently transmitted from parents to children; and that, judging from the general intelligence of the native Indians, the practice does not seem in any way to affect the brain or injure the health of the people.

MANCHESTER

Literary and Philosophical Society, October 7.—Edward Schunck, F.R.S., vice-president, in the chair. W. Boyd Dawkins, F.R.S., exhibited a fragment of a post struck by lightning on June 2, 1873. It was completely shattered, fragments being driven as far as the walls of the house, twenty-five yards off, and the downward direction of the loose splinters implied that the explosive force was exerted from below upwards, instead of from above downwards. Mr. Baxendell thought it was most probably due to the sudden conversion of a portion of the moisture in the post into steam of high tension by the heating action of the electrical discharge, and mentioned instances in which condensed vapour was said to have been seen rising from trees immediately after they had been struck by lightning.—"On the Relative Work spent in Friction in giving Rotation to shot from Guns rifled with an increasing, and a uniform twist," by Osborne Reynolds, M.A., Professor of Engineering, Owens College, Manchester, and Fellow of Queen's College, Cambridge. The object of this paper was to show that the friction between the studs and the grooves necessary to give rotation to the shot consumes more work with an increasing than with a uniform twist; and that in the case of grooves which develop into parabolas, such as those used in the Woolwich guns, the waste from this cause is double what it would be if the twist was uniform. The following conclusions were arrived at by Prof. Reynolds:—

1. That when the pressure of the powder is constant,

$$\frac{\text{Work spent in friction with parabolic grooves}}{\text{Work spent in friction with plane grooves} \ldots} = \frac{3}{2}$$

2. That when the pressure diminishes rapidly the above ratio = 2.

3. That this ratio may have any values between these two, but that it cannot go beyond these limits.

PARIS

Academy of Sciences, November 10.—M. de Quatrefages, president, in the chair.—The following papers were read:—An examination of the law proposed by Herr Helmholtz for the representation of the action of two elements in a current, by M. J. Bertrand.—Remarks on an historical point in relation to animal heat, by M. Berthelot.—On the foundation of a meteorological observatory at the foot of the peak Du Midi by the Ramond Society, by M. Ch. Sainte-Claire Deville.—An extract from a letter from M. de Lesseps to Lord Granville on the projected Central Asian Railway. In the letter M. de Lesseps argued against the supposed danger of a Russian invasion of India, and expressed a hope that the Viceroy would permit his son and Mr. Stuart to commence their surveys.—On the structure of the teeth of the *Helodermata* and *Ophidians*, by M. P. Gervais.—Memoir on the problem of three bodies, by M. E. Mathieu.—Note on magnetism, by M. J. M. Gaugain. This formed the fifth of the author's notes on this subject.—Researches on the absorption of ammonia by saline solutions, by M. Raoult. The author stated that the difference between the coefficient of solubility of this gas in pure water and in saline solutions of the same salt is proportional to the weight of the salt dissolved in a given volume.—On the transpiration of water by plants in air and in carbonic anhydride, by M. A. Barthelémy.—New researches on the upward transport of nourishment by the bark of plants, by M. Faivre.—On the development of swellings on the rootlets of the vine, by M. Max. Cornu.—On certain cases of intermittence of the electric current, by M. A. Cazin.—On a process for finding the nodes of a sonorous tube, by M. Bourbouze.—On the presence and estimation of titanium and vanadium in the basalts of Clermont-Ferraud, by M. G. Roussel.—A method of estimating sugar by means of iron, by M. E. Riffard.—Certain facts relating to the development of bony tissue, by M. Ranvier.—On the *Pemphigus* of *Pistacia terebinthus* compared with the *Phylloxera quercûs*, by M. Derbès.—On a new kind of fossil Lemur recently found in the Quercy deposits of tricalcic phosphate, by M. Filhol.—On the influence of the moon on meteorological phenomena, by M. E. Marchand.—On a method for the determination of the direction and force of the wind; abolition of weathercocks, by M. H. Tarry.

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