THURSDAY, FEBRUARY 5, 1874

SCIENTIFIC WORTHIES

II.—THOMAS HENRY HUXLEY

THOMAS HENRY HUXLEY was born at Ealing, on May 4, 1825. With the exception of two and-a-half years spent at the semi-public school at Ealing, of which his father was one of the masters, his education was carried on at home, and in his later boyhood, was chiefly the result of his own efforts. In 1842 he entered the medical school attached to Charing Cross Hospital, where, at that time, Mr. Wharton Jones, distinguished alike as a physiologist and oculist, was lecturing on Physiology. In 1845 Mr. Huxley passed the first M.B. examination at the University of London, and was placed second in the list of honours for Anatomy and Physiology, the first place being given to Dr. Ransome, now of Nottingham. After some experience of the duties of his profession among the poor of London, in 1846 he joined the medical service of the Royal Navy, and proceeded to Haslar Hospital. From thence he was selected, through the influence of the distinguished Arctic traveller and naturalist, Sir John Richardson, to occupy the post of Assistant-Surgeon to H.M.S. *Rattlesnake*, then about to proceed on a surveying voyage in the Southern Seas. The *Rattlesnake*, commanded by Captain Owen Stanley, with Mr. MacGillivray as naturalist, sailed from England in the winter of 1846. She surveyed the Inner Route between the Barrier Reef and the East Coast of Australia and New Guinea, and after making a voyage of circumnavigation, returned to England in November 1850. During this period Mr. Huxley investigated with a success known to all naturalists, the fauna of the seas which he traversed, and sent home several communications, some of which were published in the "Philosophical Transactions" of the Royal Society. The first which so appeared, presented by the late Bishop of Norwich, and read June 21, 1849, bears the title "On the Anatomy and Affinities of the Family of the Medusæ." This was, however, not Mr. Huxley's first scientific effort. While yet a student at Charing Cross Hospital, he had sent a brief notice to the *Medical Times and Gazette*, of that layer in the root-sheath of hair which has since borne the name of Huxley's Layer. Shortly after his return he was (June 1851) elected a Fellow of the Royal Society.

In 1853 Mr. Huxley, after vainly endeavouring to obtain the publication by the Government of a part of the work done during his voyage, left the naval service, and in 1854, on the removal of Edward Forbes from the Government School of Mines to the chair of Natural History at Edinburgh, succeeded his distinguished friend as Professor of Natural History in that institution, a post which he has continued to hold up to the present day. Since that time Mr. Huxley has lived in London a life of continued and brilliant labour. From 1863 to 1869 he held the post of Hunterian Professor at the Royal College of Surgeons. He was twice chosen Fullerian Professor of Physiology at the Royal Institution of Great Britain. In 1869 and 1870 he was President of the Geological Society, having previously served as Secretary. During the same period he was President of the Ethnological Society. In 1870 he filled the office of President of the British Association for the Advancement of Science, and in 1872 was elected Secretary to the Royal Society. He has been elected a corresponding member of the Academies of Berlin, Munich, St. Petersburg, and of other foreign scientific societies, has received honorary degrees from the Universities of Breslau and Edinburgh, and last year was presented with the Order of the Northern Star by the King of Sweden. Since 1870 he has been one of the Members of the Royal Commission on Scientific Instruction and the Advancement of Science. From 1870 to 1872 he served on the London School Board as one of the members for Marylebone, and during that time was Chairman of the Education Committee which arranged the scheme of education adopted in the Board Schools. In 1872 he was elected Lord Rector of the University of Aberdeen.

In this skeleton narrative of the career of this distinguished naturalist we have purposely omitted any list or any critical estimate of his writings; but we have great pleasure in laying before our readers, as a token of what is thought of him by those who are labouring in the same field of Science, the following communication from one who ranks in his own country as well as among ourselves as one of the very first of German naturalists.

The more general, year by year, the interest taken by all educated people in the progress of Natural Science, and the wider, day by day, the field of Science, the more difficult is it for the man of science himself to keep pace with all the advances made—the smaller becomes the number of those who are able to take a bird's-eye view of the whole field of science, and in whose minds the higher interest of the philosophical importance of the whole is not lost amid a crowd of fascinating particulars. Indeed if at the present moment we run over the names distinguished in the several sciences into which Natural Knowledge may be divided —in Physics, in Chemistry, in Botany, in Zoology—we find but few investigators who can be said to have thoroughly mastered the whole range of any one of them. Among these few we must place Thomas Henry Huxley, the distinguished British investigator, who at the present time justly ranks as the first zoologist among his countrymen. When we say the first zoologist, we give the widest and fullest signification to the word "zoology" which the latest developments of this science demand. Zoology is, in this sense, the entire biology of animals; and we accordingly consider as essential parts of it the whole field of Animal Morphology and Physiology, including not only Comparative Anatomy and Embryology, but also Systematic Zoology, Palæontology and Zoological Philosophy. We look upon it as a special merit in Prof. Huxley that he has a thoroughly broad conception of the science in which he labours, and that, with a most careful empirical acquaintance with individual phenomena, he combines a clear philosophical appreciation of general relations.

When we consider the long series of distinguished memoirs with which, during the last quarter of a century, Prof. Huxley has enriched zoological literature, we find that in each of the larger divisions of the animal kingdom we are indebted to him for important discoveries.

For portrait—see frontispiece.

From the lowest animals, he has gradually extended his investigations up to the highest, and even to man. His earlier labours were, for the most part, occupied with the lower marine animals, especially with the pelagic organisms swimming at the surface of the open sea. He availed himself of an excellent opportunity for the study of these, when on board H.M.S. *Rattlesnake* on a voyage of circumnavigation, which took him to many most interesting parts of tropical oceans little investigated, previously, by the zoologist; especially the coasts of Australia. Here he was able to observe, in their living state, a host of lower pelagic animals, some of which had not at all been studied, others but imperfectly. In the Protozoa, he was the first to lead us to satisfactory conclusions concerning the nature of the puzzling Thalassicollidæ and Sphærozoida. Our knowledge of Zoophytes has be n greatly extended by his splendid work on "Oceanic Hydrozoa," in which, chiefly, the remarkable Siphonophora, with their largely developed polymorphism and the instructive division of labour in their individual organs, are described with very great accuracy.

Already in his first work "On the Anatomy and the Affinities of the Medusæ," 1849, he directed attention to the very important point, that the body of these animals is constructed of two cell layers—of the Ectoderm and the Endoderm—and that these, physiologically and morphologically, may be compared to the two germinal layers of the higher animals. He has made us better acquainted with several interesting members of the class Vermes, Sagitta, Lacinularia, some lower Annulosa, &c. He was the first to point out the affinities of Echinodermata with Vermes. In opposition to the old view, that the Echinodermata belong to the Radiata, and, on account of their radial type, are to be classed with corals, m edusæ, &c., Huxley showed that the whole organisation of the former is essentially different from that of the latter, and that the Echinoderms are more nearly related, morphologically, to worms. Further he has essentially enlarged our knowledge of the important group of Tunicata by his researches on the Ascidians, Appendicularia, Pyrosoma, Doliolum, Salpa, &c.

Many important advances in the morphology of the Mollusca and Arthropoda are also due to him. Thus, *e.g.*, he has greatly elucidated the controverted subject of the homology of regions of the body in the various classes of Mollusca. He has considered the generation of vine-fretters from quite a new point of view, based on his "genealogical conception of animal Individuality." But it is the comparative anatomy and classification of the Vertebrata which, during the last ten years, he has especially studied and advanced. His excellent "Lectures on the Elements of Comparative Anatomy" afford abundant proof of this, to say nothing of his numerous important monographs, especially those on living and extinct fish, amphibians, reptiles, birds, and mammals.

Huxley's works on the comparative anatomy of the Vertebrata are the only ones which can be compared with the otherwise incomparable investigations of Carl Gegenbaur. These two inquirers exhibit, particularly in their peculiar scientific development, many points of relationship. They both belong to that small circle of morphologists which is marked by the names of Caspar Friedrich Wolff, George Cuvier, Wolfgang Goethe, Johannes Müller, and Carl Ernst von Baer.

More important than any of the individual discoveries which are contained in Huxley's numerous less and greater researches on the most widely different animals are the profound and truly philosophical conceptions which have guided him in his inquiries, have always enabled him to distinguish the essential from the unessential, and to value special empirical facts chiefly as a means of arriving at general ideas. Those views of the two germinal layers of animals which were published as early as 1849 belong to the most important generalisations of comparative anatomy; they already contain in germ, the idea of the "perfect homology of the two primary germinal layers through the whole series of animals (except protozoa)," which first found its complete expression, a short time since, in the "Gastræa theory;" also his researches on animal individuality, his treatment of the celebrated vertebral theory of the skull, in which he first opened out the right track, following which Carl Gegenbaur has recently solved in so brilliant a manner this important problem, and above all his exposition of the Theory of Descent and its consequences, belong to this class. After Charles Darwin had, in 1859, reconstructed this most important biological theory, and by his epoch-making theory of Natural Selection placed it on an entirely new foundation, Huxley was the first who extended it to man, and in 1863, in his celebrated three Lectures on "Man's Place in Nature," admirably worked out its most important developments. With luminous clearness, and convincing certainty, he has here established the fundamental law, that, in every respect, the anatomical differences between man and the highest apes are of less value than those between the highest and the lowest apes. Especially weighty is the evidence adduced, for this law, in the most important of all organs, the brain; and by this, the objections of Prof. Richard Owen are, at the same time, thoroughly refuted. Not only has the Evolution Theory received from Prof. Huxley a complete demonstration of its immense importance, not only has it been largely advanced by his valuable comparative researches, but its spread among the general public has been largely due to his well-known popular writings. In these he has accomplished the difficult task of rendering most fully and clearly intelligible, to an educated public of very various ranks, the highest problems of philosophical Biology. From the lowest to the highest organisms, from Bathybius up to man, he has elucidated the connecting law of development.

In these several ways he has, in the struggle for truth, rendered Science a service which must ever rank as one of the highest of his many and great scientific merits.

ERNST HAECKEL

ZOOLOGICAL NOMENCLATURE

The Object and Method of Zoological Nomenclature. By David Sharp. (E. W. Janson and Williams and Norgate, 1873.) Pp. 39.

ZOOLOGISTS and botanists universally adopt what is termed the binomial system of nomenclature invented by Linnæus. The essential principle of this system is, that every species of animal or plant is to have a name made up of two words, the second word—which is called the specific or trivial name, having exclusive reference to the species itself, the first word—which is called the generic name, indicating the genus, or small natural group, which comprises the species in question along with others. Thus the cat, the tiger, and the lion, belonging to one genus or small natural group of closely-allied animals, are called respectively, *Felis cattus*, *Felis tigris*, and *Felis leo*. The name of each species, therefore, shows us what group it belongs to, and thus gives us a clue to its affinities ; and the system of nomenclature is to this extent classificatory. But, as the true natural grouping of species has not yet been agreed upon by naturalists, and genera have been in a state of incessant change from the time of Linnæus to the present hour (or for about a century), the names of an immense number of species have been repeatedly altered ; and one of the first requisites of a good system of nomenclature—that the same object shall always be known by the same name—has been lost, in the attempt to make the name a guide to classification, while the classification itself has ever been fluctuating and still remains unsettled. As an example let us take the Snowy Owl. This has been placed by different ornithological authors in the genera Bubo, Strix, Noctua, Nyctea, Syrinium, and Surnia ; and at the same time, owing to carelessness or error, a number of different specific or trival names have also been used, such as *scandiaca*, *artica*, *nivea*, *erminea*, *candida*, and *nyctea;* and the various combinations of these two sets of names have led to the use of about twenty distinct appellations for this single species of bird. This example is by no means a very extreme one ; and it represents what occurs over and over again, in varying degrees, in every department of zoology and botany.

In order to determine in every case which of the names which are or have been in use is the right name, and so arrive at uniformity of nomenclature, certain rules have been pretty generally agreed upon, the most important of which is that of "priority." This means that the first name given to a species is to be the name used, even when it has never come into general use, but is now discovered in some scarce volume dated 80 or 100 years ago. But this absolute law of priority only applies to the specific or trivial name ; in the case of the generic name no such absolute priority has been thought possible, because the genera of the old authors were very extensive groups, which have now been divided, in some cases into hundreds of genera. This process of division has, however, gone on step by step, one author dividing an old genus into three or four new ones, with new names ; another dividing some of these still further, with more new names ; another perhaps discovering that these genera were not natural, and grouping the species into genera on altogether different principles, and again giving new names. Genera have been thus subdivided to such an extent that the owls, for example, which Linnæus classed as one genus, now number more than fifty ; and the ten British owls have to be placed in nine distinct genera.

In the very ingenious and careful essay which has led to these remarks, Mr. David Sharp, a well-known entomologist, advocates a mode of attaining the great desideratum of naturalists—a fixed and uniform nomenclature of species—which has not, so far as we are aware, been suggested before, although it is at once simple and logical. He proposes that, not merely one-half, but the entire name of every species once given, should be inviolable, until by general consent some permanent classificatory system of naming species, analogous to that used in chemistry, is arrived at. The insect named by Linnæus *Papilio dido* should, for example, retain that name, although it must find its classificatory place in the genus Colœnis and the family Nymphalidæ ; while the glossy starling of the East should retain the name *Turdus cantor*, given to it by Gmelin, although it is no thrush, and belongs to the genus Calornis. The name would thus remain fixed, however the place of the species in our classifications might be changed ; and the very errors of the original describers might help us to remember the object referred to by directing our attention to the cause of their error in classifying it. A beginner might, it is true, be misled, but the mistake once pointed out, the very inappropriateness of the name would serve us an aid to memory, as in the well-known "*lucus a non lucendo*." It is also pointed out that the value of the binomial nomenclature as a guide to the affinities of a species is now almost lost, owing to the minute subdivision of the old well-marked groups and the consequent multiplication of genera. No one can remember the names of all the genera of beetles now that they exceed ten thousand, unless he devotes his life to their study ; and even then the fixity of the names of all the old and well-known species would be a great help in the study of new classifications, or the use of modern catalogues.

A great evil of the present system is, that while professing to keep the specific or trivial name inviolable, it often compels an entire change of name. This happens whenever, by a new arrangement, a species has to be placed in a genus which already contains the same trivial name. Two species thus come to have the same name, and one of these must be wholly changed. The evil of this system of perpetually changing names is not so much the trouble it gives us to find out what object a name really refers to (though that is serious) as the enormous waste of labour involved in the elaborate working out of synonomy, rendered many fold more difficult by the complication of changes in both the generic and specific names, from a variety of causes. These difficulties are much greater in the case of genera than in that of species ; and this portion of synonomy would be almost got rid of if it were decided that the first binomial name given to a species should never be changed. We should then avoid the absurdity of having hundreds of familiar names abolished, because a mere compiler of an early catalogue, who had perhaps never seen the objects themselves, divided them up almost at random into a number of named groups, or because some modern student thinks it advisable to split up every large genus into dozens of smaller ones.

These appear to be weighty arguments in favour of Mr. Sharp's proposal, yet we are far from thinking that it will be adopted. For, after all, the changed names are but few in comparison with those which remain unchanged for considerable periods ; and the charm of a nomenclature which is to a considerable extent classificatory is so great, that most naturalists will strongly object to giving it up. So long as the old name keeps within the bounds of the modern family (which is in most cases a stable and well-defined group) there might be little objection to retaining it; but when it leads to the use of a name indicating a distinct and often quite unrelated family—as *Silpha scabra* for one of the Lamellicornes, (*Trox scabra*) in the example given by Mr. Sharp—the system will, we apprehend, be almost unanimously rejected.

Many minor details of nomenclature are discussed in the essay before us, and on some of these the author's views are more likely to meet ultimately with general acceptance. He objects strongly, for example, to the common practice among classical purists of altering all names which they consider to be not properly spelt or not constructed on true classical principles. For, as he justly remarks, the emenders can give no guarantee that their alterations will be permanently accepted, since others may come after them who will have different views as to classical orthography and propriety of nomenclature. He points in particular to the inconvenience of placing an H before many names which were originally spelt with a vowel, thus altering their places in an alphabetical arrangement, and creating a synonym for no useful purpose whatever.

Although it appears to us pretty certain that the plan of returning to the first generic name given to a species will not be adopted, the proposal to do so may lead to a reconsideration of the practice of applying the law of priority to generic names, as all are agreed it must be applied to specific or trivial names. If the generic part of the name may be altered any number of times in accordance with altered views as to classification, the principle of priority in the mere name is so totally given up, that it seems absurd to use it for the purpose of resuscitating the obsolete appellations of early writers. When an author is admitted to have defined a natural genus, he should have full power to give a name to that genus, because it is really a new thing; and it is both illogical and inconvenient to reject his name because some former writer has given another name to a group, not the same, but which merely happened to contain some one or more of the same species. Again, we think Mr. Sharp's arguments suggest the advisability of opposing the splitting up of large genera into many smaller ones otherwise than provisionally; the old generic name continuing to be used till there is a concurrence of opinion as to the necessity of adopting the new ones. The older authors were often modest enough to do this; indicating natural divisions of large genera, but not naming them; whereas modern naturalists, as a rule, feel bound to give a new name to every fragment they can split off an established genus.

It appears, then, to the present writer, that the plan best adapted to lead speedily to a fixed nomenclature, and at the same time one that will least offend the prejudices of zoologists, is as follows:—

1. To adopt, absolutely and without exception, the principle of priority as regards specific or trivial names.

2. To adopt the same principle for genera only so long as the generic character or definition of the genus remains unaltered; but whenever an original investigator defines a genus more completely than has been done before, he is to be left free to name it as he pleases. Every consideration of utility and common sense will of course lead him to retain a name already in use when the new genus does not materially differ from an older one: but of that he is alone the judge, and it should be absolutely forbidden to any third party to say that a name so given must be changed.

3. Whenever genera which are widely recognised are split up into a number of proposed smaller ones, the old generic name should continue in use till further investigation determines whether the new groups are sufficiently well defined and natural to supplant the old one.

In conclusion, it may be suggested that if zoologists who have paid attention to this subject would, after a careful consideration of Mr. Sharp's paper, state their own conclusions in the form of short propositions, accompanied by their reasons for them, a notion might be obtained, not only as to which system is intrinsically the best, but, what is of equal or perhaps greater importance, which is most likely to command general assent.

ALFRED R. WALLACE

RESULTS OF THE FRENCH SCIENTIFIC MISSION TO MEXICO

Mission Scientifique au Mexique et dans l'Amerique Centrale. Recherches Zoologiques publiées sous la direction de M. Milne-Edwards. Livraisons 4. (Paris: 1870-72.)

THE ill-fated attempt of the Second Empire to establ- Imperialism in Mexico has had at least one good result in the work now before us, in which the labours of a Scientific Mission originally sent out under the shadow of the French Army are given to the world. The materials accumulated by M. Bocourt and his Fellow-Naturalists, were deposited in the National Museum of the Jardin des Plantes, and the elaboration of them entrusted to special workers in the different branches of science. In 1870 three livraisons were issued, each forming the commencement of a separate section of the work, as planned out under the direction of M. Milne-Edwards. These relate to the terrestrial and fluviatile Molluscs, by MM. Fischer and Crosse; to the Orthopterous Insects and Myriapods, by M. Henri de Saussure; and to the Reptiles and Batrachians, by MM. Auguste Duméril and Bocourt. The fall of the Empire and German occupation stopped the immediate progress of the work, but we are glad to see it has now been resumed. A second livraison of the section devoted to the Myriapods, prepared by MM. H. de Saussure and Humbert, has been lately issued, and we believe it is fully intended to bring the work to a conclusion. It will be observed that authors engaged on the various sections are all well-known authorities on the subjects of which they treat, and that the figures and illustrations are of an elaborate character. We are the more glad to call the attention of our readers to the revival of this work, because it does not appear to be very generally known to naturalists, and because it has lately been the subject of a most unjustifiable attack in an English scientific periodical.* After a general condemnation of the work we are there informed that it is "a lamentable exhibition of the very backward state of zoological science in

* Ann. Nat. Hist. for August 1873.

the French capital." As to the justice of this remark we need only appeal to the recent numbers of the "Annales des Sciences Naturelles" and the "Nouvelles Annales du Musée," which are replete with zoological memoirs of the highest interest, and to the great work on fossil birds, by Alphonse Milne-Edwards, recently completed, which is alone sufficient to refute such a sweeping accusation. That the spirit of scientific enterprise is still alive in France is, moreover, sufficiently manifest by the grand researches of Père David in Chinese Tibet, and of Grandidier in Madagascar, while there is certainly no lack of scientific experts to bring their discoveries before the public. A more baseless and unjust attack was certainly never penned against the savants of a sister nation.

But when our English critic proceeds to suggest that either the general editor of the present work, Prof. Milne-Edwards, or the joint author of the part devoted to the Reptilia—the late Prof. Dumeril (for his remarks may be intended for either of these gentlemen)—has appropriated the funds devoted to its preparation and left the labour to be performed by some inferior subordinate, the matter becomes still more serious. It is, however, sufficient to reply that no sort of evidence is given to support these statements, and that the value of Dr. Gray's *ipse dixit* is not sufficiently appreciated among naturalists to induce them to accept such an impossible supposition.

OUR BOOK SHELF

Sahara and Lapland. Travels in the African Desert and the Polar World. By Count Goblet D'Alviella. Translated from the French by Mrs. Cashel Hoey. (London: Asher and Co., 1874.)

AT first sight it would seem that no two countries had less in common than the two about which this book is written; but Count D'Alviella ingeniously and correctly shows, in his thoughtful preface, that they, or rather the Lapps and Arabs, have many circumstances in common. These two peoples "lead the same vagabond existence; they live exclusively upon their herds, they carry with them all they have and that they possess, and they make analogous migrations at the changes of the seasons—the Lapps from the Swedish steppes to the Norwegian valleys, the Arabs from the plains of Sahara to the pastures of Tell. In this manner of life they have both acquired the same strength of constitution, or rather the same power of resisting such fatigue, privations, and weather as would kill the most robust European. . . . Both the Lapps and the Arabs—who are rather the slaves than the masters of Nature—owe their consciousness of isolation and powerlessness to the same superstitions, the same beliefs in spirits, to the 'evil eye,' in amulets, and in incantations. . . . Both races—restricted for centuries to a form of society unsuitable to any kind of progress—affect the same respect for the routine of their ancestors, and the same disdain for the arts of civilisation." The author concludes rightly, we think, that both peoples, incapable as they are of transformation or civilisation, are doomed to disappearance. Many attempts have been made by the Swedish and French Governments to get these nomads to settle down into civilised life, but invariably without success. The author, on the authority of M. Charles Martins, relates that the French Government gave to a number of the poorest Arabs of the Sahara some fertile fields with a ready-built village, and even a mosque in the middle of it. They reserved the houses for their flocks, and pitched their tents in the streets; until one day the nostalgia of the desert seized upon them, and they returned rejoicing to their wandering life.

Count D'Alviella tells the narrative of his travels in these two regions very pleasantly. He is a cheerful and observant and somewhat philosophic guide, and we can assure anyone who cares to buy this work, that he will get the value of his money in enjoyment and information. The narrative of the Lapland journey is especially interesting, and contains information about a people and a country that we believe many know but little about. Here will be found an account of the mode of life of a people that in many respects may be taken as the living type of the men who, ages ago, struggled for existence amid conditions very different from those which now obtain in Europe, and whose implements and remains come within the province, not of the historian, but of the geologist.

Mrs. Hoey deserves credit for her excellent translation. The volume contains a number of fairly executed illustrations.

LETTERS TO THE EDITOR

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

M. Barrande and Darwinism

IN the article in NATURE (vol. ix. p. 228) on M. Barrande's "Trilobites," published in 1871, several statements are made which require not only considerable modification as to the facts then known, but which are entirely misleading when made to appear to represent the state of our knowledge of these facts at the present time. M. Barrande is well known to be a determined opponent to the theory of evolution, and doubtless this strong bias has prevented him from seeing and accepting many facts which would otherwise, to so keen and careful an observer, have seemed inconsistent with such strong views. The list of fossils given by him from the Cambrian formation, and which is reproduced in NATURE, is most incomplete and inaccurate when made to refer to the Cambrian fauna of this country, as will be evident at once by referring to p. 249 of the same work, where a list of fossils discovered by me in the "Harlech or Superior Longmynd" group of Wales is given, and which includes several trilobites; and yet in the above-mentioned article it is stated "that no trace of a trilobite has been found in the Cambrian formation." Surely no English geologist will be bold enough to deny to the name Cambrian its right to these Harlech and Longmynd rocks, whatever else it may not be entitled to. Nor, indeed, did Sir R. Murchison and the Geological Survey ever attempt such a breach, and I cannot believe that M. Barrande has realised what such an assumption means, or what it would lead to; nor can I believe that it is possible for him to have followers in this country in such a "violation of historic truth," and, as observed by Prof. Sterry Hunt (in the *Canadian Naturalist*, vol. vi. p. 448), for no other reason than "that the primordial fauna has now been shown by Hicks to extend towards their base." Surely this country, which has not only given to scientific nomenclature the name Cambrian, but which has given to all other countries the groundwork upon which to build up theirs, should have a right to explain the succession in its own way, and especially when it is proved that its succession of these rocks is clearer and more natural than has been hitherto found to be the case in any other country. Indeed it is quite clear that M. Barrande has not yet succeeded, in Bohemia, in reaching this early fauna, and it is evident also that his first zone of life is only equal in order of appearance to the latter part of our *second zone*, and hence the mistake to attempt to correlate our fauna with his zone.

At St. David's in South Wales, the Cambrian of the Geological Survey, consisting of red, purple, and green rocks, attains a thickness of over five thousand feet of beds resting conformably, and of these beds over four thousand feet have yielded evidence, in the form of fossils, of life having existed in the seas in which they were deposited. The forms of life comprised annelids, brachiopods, pteropods, bivalve crustaceans, trilobites, and sponges, and I think it would be seen on examination that the picture offered by this early fauna is not one in discordance with Darwinism, as assumed in the article in question. But as M.

Barrande and the author of the article have restricted their remarks almost entirely to the trilobites, I will only ask to be allowed space to reply to the facts stated with regard to these forms of life. Trilobites have now been discovered as low down as 4,000 ft. in these red and green rocks at St. David's, that is, in the very earliest fauna known, and amongst them are forms hitherto not discovered in any other country; still if we are to believe with some that we are here near the beginning of life on the globe, or even of trilobitic life, we can expect but little evidence to support or to disprove Darwinism. For, considering the frequent changes in the sea bottom which must have taken place at this period, to produce at one time a shingle, then a sand or grit, and then a fine muddy deposit, and such beds frequently repeated, we cannot possibly expect that during all these physical changes an unbroken record of these forms should be preserved to us. No, rather we should expect to find that the necessary migrations would produce alterations in the forms, and that they should now and again return modified and altered in proportion to the time which had intervened and the circumstances which surrounded them. And this is really what we do find, and which is apparent at once to the palæontologist, who is prepared to allow and to recognise in these very marked physical changes a controlling influence capable of greatly affecting the life of the period. Again, can any one really believe, when thinking of the enormous time which must have elapsed during the accumulation of the great Laurentian series, and possibly of other series previous and succeeding, all antecedent to the time when the Cambrian fauna made its appearance, that the seas in which these were deposited were entirely barren of life? Surely not; therefore, why so readily jump at conclusions when there is so much room for doubt? Again, this Cambrian fauna is not without evidence in favour of evolution. Trilobites we know develope by increase of the body segments, and therefore M. Barrande says that the earliest trilobites should have the smallest number of segments in the thorax—"but that those of the primordial fauna are generally characterised by the opposite condition, while the number is less in those of the succeeding faunas." Now it does not seem to have occurred to M. Barrande that trilobites show every indication of having culminated at or about this period, that they had attained their maximum size and development, and that from this time they seem to have gradually diminished in size, and to have degenerated, doubtless much in the order in which they had previously progressed. This will explain also why the number of segments should, as he says, diminish in number in the genera of succeeding faunas. One of the very earliest trilobites we know of is the little *Agnostus*. It is also the simplest and apparently the most rudimentary of the group. It has no eyes, only two segments to the thorax, and usually an ill-defined glabella. In tracing a species of *Paradoxides* from the earliest stage upwards, I was struck with the very great resemblance which, at an early stage, it had to the little *Agnostus*. The glabella was indistinct, and much shorter in proportion to the length of the head than in the fully grown specimen, and the eye very faint, scarcely marked out, and the outline of the head more evenly rounded, with scarcely any indication of spines. Before the discovery of the Cambrian fauna at St. David's, no genus of trilobites had been found with four segments to the thorax, therefore we had to jump from one with two to one with six, as in *Trinucleus* or *Ampyx*. Now, however, since the discovery of *Microdiscus* with four segments, the gap has been filled up, and the genus, unfortunately for those holding M. Barrande's views, appears in our earliest fauna, and where the evolutionist would be most inclined to look for it. It is also a most interesting and instructive genus. It is somewhat larger than *Agnostus*, but like it, has no eyes. The *glabella* is better formed, more distinctly marked off from the cheeks, and instead of being irregularly grooved, as is usually the case with *Agnostus*, it is furrowed regularly as in an advanced stage in the development of *Paradoxides*. In the caudal portion the axis is partly divided into segments, and in one species the lateral lobes are slightly grooved as if into rudimentary pleuræ. It is very plentiful in the beds at St. David's, and since its discovery there, species have also been found in Canada and elsewhere.

From this stage forms have been found to represent every step in development as to the number of segments, and indeed often to show marked stages in other parts. *Anopolenus* is really a *Paradoxides* with enormous eyes, reaching to the hinder margin, and with several of the hinder pleuræ consolidated together to form a large spinous pygidium. Another *Paradoxides* has the eyes nearly as large as *Anopolenus*, but with a few more segments to the thorax, and a smaller pygidium. Other species show various gradations in the eyes and in the pygidium until we attain to *P. Davidis*, which has small eyes, a small pygidium, and the greatest number of thoracic segments. Indeed there are forms to represent almost every stage, and there can I think be no doubt that in the fauna of the Tremadoc group, which is separated from the earlier Cambrian by several thousand feet of deposits indicating a period of very shallow water in which large brachiopods and phyllopod crustaceans were the prevailing forms of life, we witness a return to very much the same conditions as existed in the earlier Cambrian periods, and with these conditions a fauna retaining a marked likeness to the earlier one, and in which the earlier types are almost reproduced, though of course greatly changed during their previous migrations. The *Niobe* (?) recently found in the Tremadoc rocks is truly a degraded *Paradoxides*, retaining the glabella and head spines, but with the rings of the thorax, excepting eight, consolidated together to form an enormous tail. Instead therefore of having here, as stated by M. Barrande, "a very important discord between Darwinism and facts," we find in these early faunas facts strongly favouring such a theory, and in support of evolution.

Hendon, Jan. 27 HENRY HICKS

ACCORDING to a notice in NATURE, vol. ix. p. 228, a distinguished continental naturalist finds an important discordance between Darwinism and certain facts connected with Trilobites and other fossil crustaceans. But his argument appears to be based on an assumption that we are acquainted with a "primordial fauna," that we are justified in dating the beginnings of life at or near some known geological period. This, however, the whole history of geology ought to make us less and less inclined to believe. It is one of those assumptions, essentially based on ignorance, on which so little dependence can rightly be placed. We have no right to call any fauna the *earliest*, merely because, as it happens, we know of none *earlier*.

A point is made of the fact that the earlier known Trilobites have more segments than the later, while individual Trilobites, as they develope, increase in number of their body segments. It may be granted at once that in this case the development of the individual is not an accurate picture of the past development of the species. But Fritz Müller has long ago shown that we could not, on principles of Darwinism, expect it always to be so; and surely, if Trilobites have been gradually developed rather than abruptly created, there must have been Trilobites with *few* before there were Trilobites with *many* segments, so that after all, the development of the individual will carry us back to an early stage in the history of the family. It could scarcely be expected to give us all the alternations and complications which that history may have presented in its whole course.

Those who on other grounds accept the theory of evolution, far from finding any obstacle to it in the large number of genera of Silurian Trilobites, will consider the largeness of that number clear evidence that life in general, and Trilobite life in particular, must have flourished on the globe for a very long period prior to the Silurian age.

The argument that we do not find connecting links between different genera has little immediate force. It must await the verdict of time and further investigation. Of 252 species o Trilobites, 61 are assigned to England. The true reading of this piece of statistics must surely be that that which great research has done for a small area may be equalled, and far surpassed, when as close a scrutiny is applied to the whole available surface. If no gaps between species, and genera, and orders are filled by the results of such a search, then it will be time to say that we have "an important discord between Darwinism and facts."

Torquay, Jan, 27 THOMAS R. R. STEBBING

Perception in Lower Animals

I RELATE the following, as it has some bearing on a question lately ventilated in NATURE.

A friend and myself were watching on one occasion the actions of two half-bred Persian cats on seeing for the first time a freshly caught cobra, which had been placed in a wire-gauze covered box near the verandah. First of all one of the cats, a black one, stalked carefully up to the box in which the snake was keeping up a perpetual "swearing," with extended hood, and after a minute survey, crept away about 7 or 8 ft. off and sat down with its back to the snake. The other cat, a white one, now caught sight of the strange object, and, in a like stealthy manner, advanced to within a few inches of the gauze, and was in the act of examining the cobra, when my friend, to see the result of a sudden sound—for up to this time we had both been still as mice—moved his feet on the gravel. Had the effect been due to electricity, it would not have been more instantaneous, nor more startling. At the first grate of the pebbles the white cat flung himself backwards, tumbling—to use expressive terms—" heels over head" and "all of a heap" for about a couple of yards; whilst the black cat shot vertically upwards to somewhere near four feet in height, the impulse given by the spring of his hind legs being sufficient to throw these and his tail higher than his head.

Now both these cats are tame, and bold to such a degree that they reign supreme over all the dogs in the house, so that their great timidity on this occasion was evidently due to a perception of danger. I have since found, however, that all snakes are not equally feared by them. They will let the harmless green tree snake (*Fasserita mycterizans*) twine round them without showing any signs of repugnance, and some other harmless snakes receive but little notice from them. Why is this? Is it that the hood of the cobra renders it so frightful an object, or have the cats in their nocturnal wanderings been struck at by cobras? Such is possible, for we know that in nine cases out of ten the strike is made without intention to exert the deadly power of the fangs. I believe indeed that unless irritated by an attacking enemy, or to secure active prey such as rats, &c., the cobra never strikes viciously. Experience of the ease with which its fangs are drawn and its helplessness without them would teach it to be careful of them.

Mangalore, Sept. 17 E. H. PRINGLE

Earthquake in New Guinea

WHEN crossing the main land of New Guinea, from the Geelvinks Bay in the north, to the south coast, I slept on the night of the 12th to the 13th of June, 1873, in the swamps of the MacCluer Gulf (famous for the murder of some of the crew and the ship's-doctor of H.M.S. *Panther* and *Endeavour*, Capt. MacCluer, in 1791, and by the attack on Signor Cerruti, the Italian traveller, several years ago). About 2 A.M. of the 13th I awoke, in consequence of a rattling noise like that of gun-shooting. I roused my six Malay companions, who slept around me in a small native prouw, seized my guns, and listened to what would follow. But nothing happened. It was unintelligible to me what had been the cause of this noise, the natives of these parts having no guns, so far as I knew, and even if they had intended an attack, would not announce their arrival by firing their guns, instead of approaching in silence. On the other hand, when sleeping in a virgin forest like that which bordered these swamps, crashing noises from falling trees and from animals breaking down rotten branches often occur, but never so many together.

Nothing more being heard we fell asleep again. At about 4 A.M. the same thing happened once more. I remained awake. At dawn the Papooas, whom I had brought with me from the north coast—ten men—came back to my resting-place; they had left me, to sleep apart, had heard the noise, but could not understand it either.

When on the 13th I came back to Papooan houses at the River Takasi, which falls into the MacCluer Gulf—a minute description of which will be published very soon in "Petermann's Mittheilungen"—I heard the account of a heavy earthquake, which had taken place the night before; this of course explaining the noises we had heard: many trees having broken down at the same moment in consequence of the movement of the ground. We did not feel the earthquake in our small boat, because it lay entirely in the swamp, which had not propagated the shock.

On the 18th I was back at my little schooner, which was at anchor in the Geelvinks Bay, near a place called Passim. The earthquake had been felt here at the same time, accompanied by heavy underground thunder, and I could make out that the direction had been N.W. to S.E.

After some days I came to a place just at the foot of the so much spoken of Arfak Mountains, called Audai; the earthquake had been heavy here, and even more shocks were felt on the following day. The direction had been W.E. Several native houses, built on very high poles near the slope of a hill, were destroyed, the Papooas (Arfaks) still frightened and of opinion that the earthquake had been "made" by their enemies, another tribe on the mountains.

But in the Bay of Dorey, which has so often been visited by expeditions to New Guinea and by naturalists, where I arrived a fortnight later, the shocks appeared to have been the heaviest. All the Papooas in the different settlements there were living on shore in small shelters or huts, hastily erected, whereas they are known always to live in those large houses on the water so often described. Several of these large houses had broken down, and the natives were still very much frightened; they would not remove into their houses on the water. On the island of Manaswari (Mansinam), in the Bay of Dorey, the seat of a missionary, the shocks had been from S.W. to N.E. I afterwards sought information about the extent of this earthquake, and made out that it was felt at Amberbaki, on the North coast of New Guinea, at Salwatti, the island in the North-west, and on the island of Tobie, in the east. The centre had been undoubtedly on the Arfak Mountains. Light earthquakes sometimes occur in New Guinea, heavy ones seldom. The destruction by the last heavy one in 1864 could even be seen by me in 1873 along the seashore from Dorey to Wariab, and up the Arfak Mountains, in the south of the bay of Dorey. Volcanic eruptions in these parts are not known or recorded from earlier times. But one of the tops of these mountain chains bears in the native language the name of "Fire Mountain," and some of my hunters pretended to have seen on one of their excursions (some thousands of feet high) the ground split open quite fresh, in consequence of the earthquake, as they believed.

This earthquake has not been felt in Halmabeira and the Molukkos Islands, where shocks occurred some weeks afterwards, so that the convulsions, referred to above, appear to have been local ones in New Guinea. DR. A. B. MEYER

Sensitive Flames at the Crystal Palace Concerts

LAST Saturday, Jan. 31, at the Crystal Palace, while Mr. Vernon Rigby was singing Beethoven's "Adelaida," I heard what I thought was strangely out of place—an accompaniment to the song played on the highest notes of a violin, sometimes closely following the air note for note, at other times being one-third lower. I soon found that this proceeded from one or two sensitive gas jets, notwithstanding they were at the end of the winter concert-room farthest away from the orchestra. The very perfect manner in which they responded to every note, no matter how *piano*, was curious.

It happened that the gas pressure had just been increased. Had this occurred earlier the effect of Mdme. Norman-Neruda's fine performance of Mendelsohn's violin concerto would have been totally destroyed, as far as regards a large part of the audience. This shows that it is a matter of no small importance in a concert-room to have the size and number of the gas-burners properly proportioned to the gas supply.

King's College, Feb. 3 W. N. HARTLEY

THE PHOTOGRAPHIC SOCIETY

THE metropolitan photographic journals contain evidence that the Photographic Society of London is menaced with revolution or dissolution. If both were to befall it, the interests of Science would hardly suffer, since a more singularly inefficient organisation, under the guise of a scientific body, it would be difficult to find, or one whose results in the scientific world are so trivial.

It is difficult indeed to conceive that a society into whose hands, *faute de mieux*, the recognition and fostering of research in so important a branch of science as photography has fallen, should have done absolutely nothing for so many years but organise itself into a pocket borough in the direction of which no man of eminent scientific capacity takes part; which not only has no scientific reports or even investigations, but seems to care only to make of itself a weak mimicry of an art club, the chief objects of which are to prove that a photographer ought to have a chance for the Royal Academy, to discuss the most effective style of getting up portraits to revive the trade demand, and to discuss such questions as to whether portraits may be re-touched or not, and whether the printing of a photograph from a half-dozen negatives, more or less, is to be regarded as a work of design or not.

It is not sufficient to put the names of two or three well-known men of science on the council of a society if the society show no care for science; and if the Photographic Society can do nothing more to merit the nominal position which it holds (without filling it), it is time that it should retire and give place to another. Photography has now become one of the most important aids to research in many fields of Science; every new discovery which shall develop this assistance and make its efficiency more complete is of importance to the whole world—of an importance which makes it almost incredible that the Photographic Society should not only take no part in the investigations which would lead to discovery, but should never even take recognition of them even when made, while the petty jealousies of the dominant clique have driven out of the society most of the really capable and successful investigators who have ever been in it. If the efforts at reform now being made should lead to success and the society become what it should be, a scientific body, so much the better; but if not, it is time that some new organisation should be formed to take in hand seriously the exploration of the still untried fields of chemical research, and make Photography a real branch of Science, and not deal with it merely as an amusement or a trade.

ASTRONOMY IN THE ARGENTINE CONFEDERACY

DR. GOULD, the director of the new Observatory in the Argentine Confederacy, continues to send encouraging accounts of the progress of the great astronomical works that he has there undertaken. Having laboured to determine accurately the relative brightness of all the stars in the southern heavens visible to the naked eye, he announces that a few weeks will enable him to begin the preparation of this work for publication. Great care has been taken to make a thorough and accurate comparison of the results of the four assistants, and the rule has been to determine the brightness of all the stars down to the 7·3 magnitude, in order to make sure of losing none as bright as the seventh.

The labour of the Uranometry was undertaken before the arrival of the large meridian instrument, and as soon as the latter was established (namely, on Sept. 9, 1872), the observations of the zones of all stars as bright as the ninth magnitude were commenced in earnest. Each night three zones are observed whose lengths average about one hundred minutes, the entire observations for the year occupying at least eight hours. The weather is described as having been exceedingly unfavourable for astronomical work during the winter and early spring, until March, April, and May of the present year, when magnificent opportunities were enjoyed. Dr. Gould states that he has observed in all during the past year about fifty thousand stars, and considers that somewhat more than half of the work of observing is already finished.

Astronomers, however, know how great a labour of computation still awaits Dr. Gould and his assistants before his results can be put into that form which is most convenient for use. The photographic work undertaken by him at his own private expense has been prosecuted with all the success that could be expected with a broken lens. Finally, however, he concluded to bespeak another object-glass, which will be purchased for the use of the observatory; and the new lens having arrived in perfect order, he hopes before long to be able to resume his labours under better auspices.

The Cordoba Meteorological Bureau, established at his urgent representation by the national Government, has been organised and brought into working condition as rapidly as was practicable; but as the instruments were necessarily ordered from foreign countries, not more than half of them had arrived at the latest advices. Dr. Gould has, however, had the gratification of finding two gentlemen who have each carried on an uninterrupted series of observations for some dozen years past—one in Buenos Ayres, and the other near the Patagonian frontier—and he has secured the co-operation of about fifteen correspondents. The programme issued for the instruction of his observers differs apparently but little from that of the Smithsonian Institution, the hours of observation being seven, two, and nine, local time.

*THE COMMON FROG**

IX.

THE muscles connected with the human lingual apparatus are sufficiently complex. One such muscle—the *stylohyoid*—passes downwards on each side, from a process of the base of the skull to the corniculum of the os-hyoides

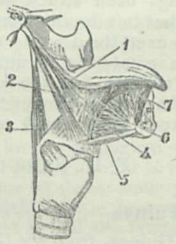


FIG. 63.

FIG. 64.

FIG. 63.—Muscles of the Right Side of the Tongue. 1, stylo-glossus; 2, stylo-hyoid; 3, stylo-pharyngeus; 4, hyo-glossus; 5, genio-hyoid; 6, genio-glossus; 7, lingualis.

FIG. 64.—Head of the Frog *Phyllomedusa*, showing the tongue fixed in front, but free posteriorly.

or tongue-bone. The tongue-bone of the frog is, as we have seen, relatively far greater than is that of man, and the same may be said for the muscles attached to it, since we have no less than four muscles descending from the skull, and implanted into it, on each side.

This fact might well be supposed to bear direct relation to the size and mobility of the frog's tongue. This organ in the frog and toad is singularly different from the tongues of most familiar animals, in that it is not free and moveable in front, but *behind*. These Batrachians take their food by suddenly throwing forwards, out of the mouth, the free hinder end of the tongue. The insect or other small animal struck by it, adheres to it, on account of a viscid saliva with which it is coated. The prey is then suddenly drawn into the mouth and swallowed.

Here then is a ready explanation of the development of the *os-hyoides* and its muscles. There is a difficulty however in that two toads already described, the Pipa and the African form *Dactylethra* (Figs. 11 and 12), have no tongue whatever.

Moreover, there is another toad (*Rhinophrynus*) which is even more exceptional in its order than these two; in that its tongue is not free behind, but, like that of ordinary vertebrates, in front (Fig. 13.)

The fact is, that the large tongue-bone of these animals serves, with the muscles attached to it, as much to facilitate respiration as nutrition.

It has already been said that the frog has no ribs by the elevation and depression of which it may alternately fill and empty its lungs. Neither does it possess that transverse muscular partition, the diaphragm, or midrif, which in man's class is the main agent in carrying on that function.

The lungs of the frog are inflated as follows:—The

* Continued from p. 189.

mouth is filled with air through the nostrils and kept shut while the internal openings of the nostrils are stopped by the tongue, and the entrance to the gullet is closed. Then, by the contraction of the muscles attached to it, the os-hyoides is elevated; and every other exit from the mouth being closed, except that leading to the larynx, air is thus driven down the glottis into the lungs.

Thus for pulmonary respiration it is necessary to the frog to keep the mouth shut; and in this way, but for the action of the skin, the animal might be choked by keeping its mouth open.

It has been already stated that the typical segmentation of the limbs is wanting in all fishes, but present in all Batrachians that have limbs at all. Similarly in all Batrachians that have limbs at all the muscles of those limbs have essentially and fundamentally the same arrangement as in higher animals. In the higher animals, as in man, the muscles of the limbs belong to different categories named from the kinds of motion to which their contractions give rise.

Thus, when two bones are united by a moveable joint (as the thigh-bone and shin-bone) muscles which, by their contraction, tend to make the angle formed by such bones

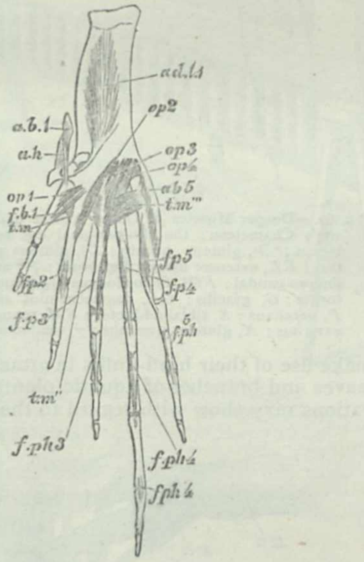


FIG. 65.—Deep muscles of exor surface of Frog's hind foot. (The numbers indicate the digits to which the muscles belong.—No. 1 indicating the first digit or great toe). *ab*, abductors; *ad*, adductor; *fb*, flexor brevis; *fp*, flexores profundi; *fph*, flexores phalangium; *op*, opponens muscles; *tm*, transverse muscles.

acute are termed "flexors." Those, on the contrary, which tend to open out such an angle are termed "extensors."

In the forearm of man, and allied animals, there are muscles which tend by their contraction to place the hand in a position either of *pronation* or of *supination*.

When the arm and hand hang down, the *palm* being directed forwards, the position is that of supination, and the bones of the forearm are situate side by side.

When the arm and hand hang down, but the *back* of the hand is turned forwards, the position is that of pronation, and the radius crosses over the ulna. When we rest on the hands and knees, with the palms to the ground, the forearms are in pronation.

Muscles which tend to place the forearm and hand in the position of pronation are termed *pronators*; those which, by their contraction, tend to render it supine are called *supinators*.

It is somewhat surprising to find in an animal so nearly related to fishes as *Menobronchus* definite flexors, extensors, pro- and supi-nators essentially like those of higher animals; and these distinctions once established, persist up to man himself with increasing complications.

The muscular conformity between the highest and lowest of typically-limbed vertebrates is strikingly shown by the structure of the thigh and leg, the leading muscles of these parts in the frog being so like those of man that the practice of calling them by the same name is abundantly justified.

The perfection of man's hand has been justly the theme of panegyric, esteemed as widely as it is known. The delicacy and multiplicity of the motions of which it is capable are of course greatly due to the number and arrangement of the muscles with which it is provided.

One of the most important of these motions is that of the thumb as placed in opposition to the fingers, and effected by a muscle termed *opponens pollicis*.

An "opponens" muscle is one which passes from the bones of the wrist to one or other of the bones of the middle of the hand called *metacarpals*, and the *opponens pollicis* passes of course, as its name implies, to the metacarpal of the pollex or thumb.

No other finger of man's hand is furnished with such a muscle except the little finger, which possesses an *opponens minimi digiti*, passing from the wrist to the fifth metacarpal. The same condition obtains in the apes, though in them the opponens of the thumb is smaller and weaker than in man. Though the foot of man is furnished with many muscles, like the hand, yet not one of the toes is provided with an "opponens" or muscle, passing from the bones of the ankle to one or other of the bones of the middle of the foot, which latter are called metatarsals. The same is the case with the apes, except that the Orang-utan has a small "opponens" attached to the great toe.

This being premised, the foot of the Frog may well excite surprise as to its rich muscular structure. In addition to very numerous other muscles on both surfaces every one of the toes is provided with a separate opponens muscle, each having a muscle which passes from the bones of the ankle to its middle foot bone or metatarsal.

The question naturally occurs on beholding this prodigality of muscles—What special purpose is served by the Frog's foot? Surely mere jumping and swimming cannot require so elaborate an apparatus.

In fact, however, the Frog *does* make use of his feet for a purpose requiring actions no less dexterous and delicate than nest-building.

In 1872 Dr. Günther observed a Frog busily occupied, and industriously moving its hind legs in a singular manner. On approaching closely he found it had constructed for itself a shelter in the shape of a little bower, constructed of dexterously interwoven blades of grass. The circumstances have been kindly transmitted to the author by the observer, in a private letter, as follows:—

"The 'nest-building' Frog was a large example of *Rana temporaria*, or *esculenta* (I forget which), which I had brought into the garden behind my house. It had taken up its abode in grass, near the edge of a tank, from which the turf sloped abruptly to the level of the garden. When I first disturbed the Frog from its lair, I found that it had lain in a kind of nest, which I cannot better describe than by comparing it to the form of a hare, with the grass on the edges so arranged that it formed a sort of roof over it. Sometimes the animal returned to it, sometimes it prepared a new form close to the old one, which remained visible for several days until it was obliterated by the growing grass.

"When in its nest, nothing could be seen of the Frog but the head.

"One day I poked the Frog out of its lair; after two or three jumps it returned to the old spot, and, squatting down on the grass, by some rapid movements of the hind legs it gathered the grass nearest to it, pressing it to its sides, and bending it over its body so as to be partially hidden.

"In all these operations no material was collected by the animal for its nest, but only the growing grass was either pressed down, or arranged so as to form a complete retreat.

"Unfortunately, the Frog soon disappeared altogether."

It is very probable that other functions, as yet unnoticed, may be performed by these members, since though the observation just above related is the first known observation of the kind, yet the manœuvre recorded is no doubt a constant habit of the animal.

Doubtless, also, the very singular actions performed by the male *Pipa* and *Obstetricans* are performed by the help of the hinder extremities.

At the same time that the Frog shows so startling a resemblance in its leg muscles to higher animals, it shows

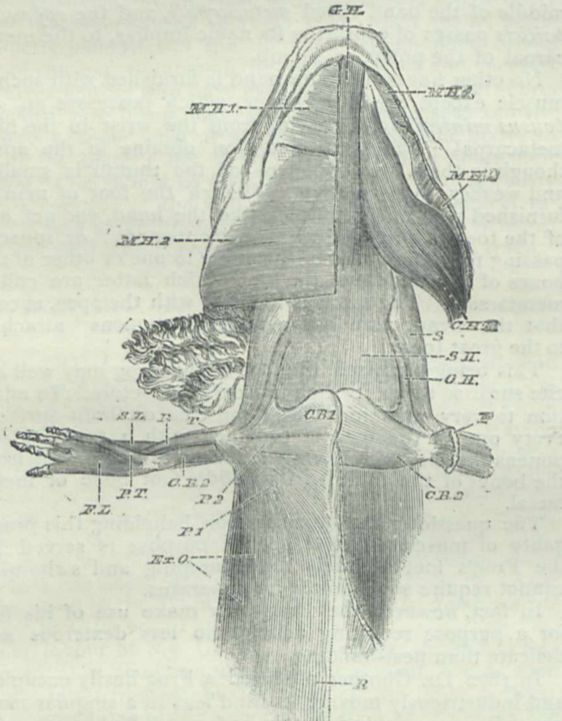


FIG. 66.—Muscles of Ventral Surface of *Menobranchus*. On the right side, superficial muscles; on the left side, deeper muscles, the mylo-hyoidei, pectoralis, and external oblique being removed. Also superficial flexor muscles of right pectoral limb of *Menobranchus*. *B*, biceps; *CB*[1] and *CB*[2], coraco-brachialis; *CHE*, cerato-hyoideus externus; *EO*, external oblique; *FL*, flexor longus; *GH*, genio-hyoid; *MH*[1] and *MH*[2], mylo-hyoideus; *OH*, omo-hyoid; *P*, *P*[1], and *P*[2], pectoralis; *R*, rectus; *S*, subclavius; *SH*, sterno-hyoid; *SL*, supinator longus; *T*, triceps.

as striking a difference from the leg muscles of animals with which it is nearly allied,—namely, with those of its class-fellows, the *Urodela*.

In Reptiles we meet with a muscle which takes origin from beneath the joints of the tail, and is inserted with the thigh-bone, and which has no certain representation amongst mammals, and is called the *femoro-caudal*.

In the Urodela we also meet with a *femoro-caudal*, but no such structure exists in the Anoura. This is not so surprising when we recollect the abortive condition of the tail of the Frog. It might, however, have been expected that in the Tadpole, during the co-existence of the tai with the hind legs, and while it thus externally resemble an eft—such a muscle would transitorily exist. Such, however, is not the case, and the distinction is a very remarkable one.

In one point, however, the Efts resemble the Frogs, namely, in the greater number and greater complexity as well as the greater size of the muscles of the hind-limbs than of the fore-limbs. It is well known that the Efts

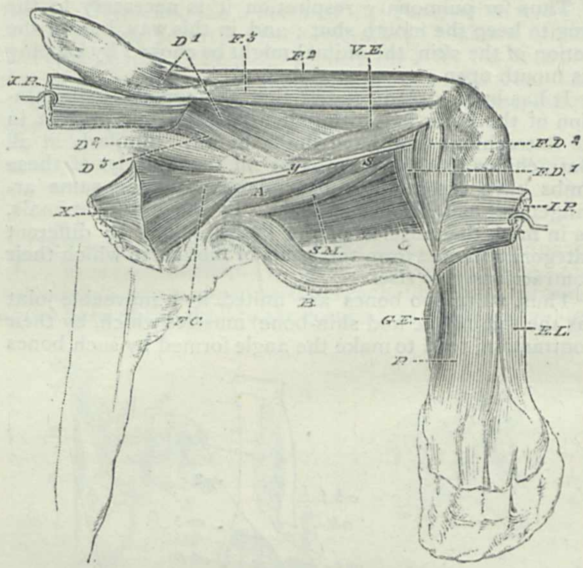


FIG. 67.—Deeper Muscles of Outer Aspect of Right Pelvic Limb of Parson's Chameleon; the ilio-peroneal cut reflected. *A*, adductor; *B*, biceps; *D*[1], gluteus primus; *D*[2], gluteus secundus; *D*[3], gluteus tertius; *EL*, extensor longus digitorum; *F*[2] and *F*[3], rectus femoris; *FC*, femoro-caudal; *FD*[1], flexor longus digitorum; *FD*[2], flexor tertius digitorum; *G*, gracilis; *GE*, gastrocnemius externus; *IP*, ilio-peroneal; *P*, peroneus; *S*, tibial adductor; *SM*, semi-membranosus; *VE*, vastus externus; *X*, gluteus maximus; *y*, tendon of femoro-caudal.

make use of their hind-limbs in attaching their eggs to the leaves and branches of aquatic plants; and further observations may show with regard to these animals facts as to

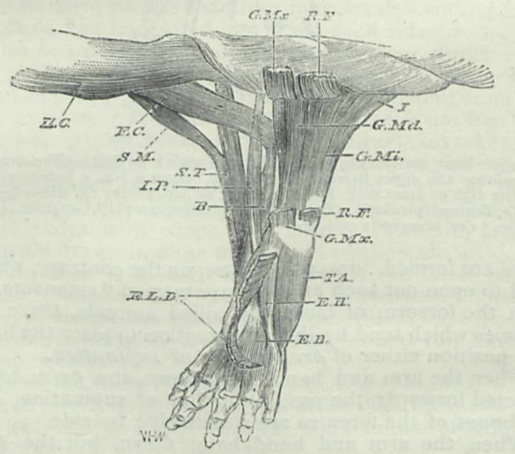


FIG. 68.—Deeper Muscles of Extensor Surface of Right Leg of Menopoma. *B*, biceps; *EB*, extensor brevis; *EH*, extensor hallucis; *ELD*, extensor longus digitorum; *FC*, femoro-caudal; *GMd* and *GMi*, muscles like the lesser glutei; *GMx* and *RF*, great extensors of the thigh; *I*, muscle resembling the iliacus; *IIC*, ilio-caudal; *IP*, ilio-peroneal; *SM* and *ST*, muscles like the semi-membranosus and semi-tendinosus respectively; *TA*, tibialis anticus.

the use of the members, as novel and interesting as the one just cited with regard to the nest-building actions of the Frog.

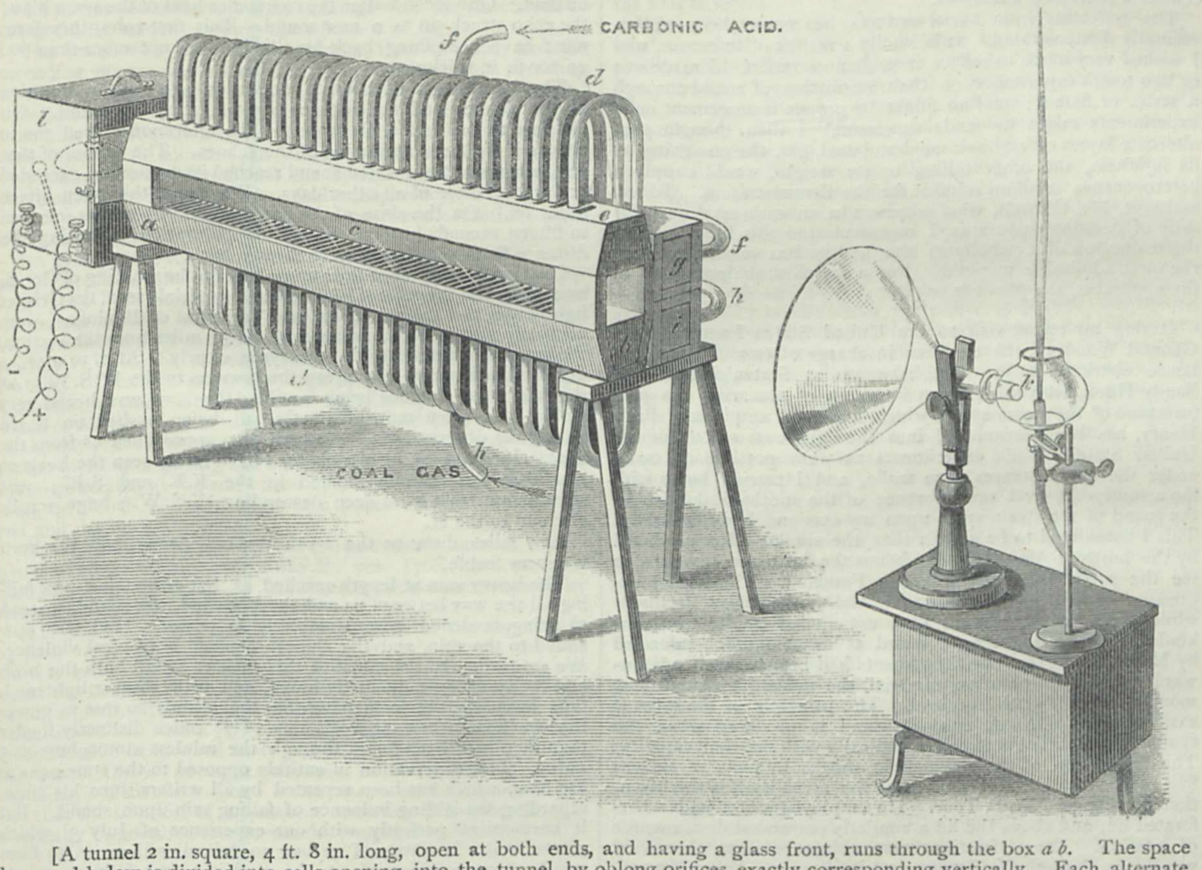
ST. GEORGE MIVART

(*To be continued.*)

THE ACOUSTIC TRANSPARENCY AND OPACITY OF THE ATMOSPHERE *

II.

We have now to consider the complementary side of the phenomena. A stratum of air, 3 miles thick, on a perfectly calm day, has been proved competent to stifle both the cannonade and the horn-sounds employed at the South Foreland; while the observations just recorded, one and all, point to the mixture of air and aqueous vapour as the cause of this extraordinary phenomenon. Such a mixture could fill the atmosphere with an impervious *acoustic cloud* on a day of perfect *optical* transparency. But, granting this, it is incredible that so great a body of sound could utterly disappear in so short a distance, without rendering any account of itself. Supposing, then, instead of placing ourselves behind the acoustic cloud, we were to place ourselves in front of it, might we not, in accordance with the law of conservation, expect to receive by reflection the sound which had failed to reach us by transmission? The case



[A tunnel 2 in. square, 4 ft. 8 in. long, open at both ends, and having a glass front, runs through the box *a b*. The space above and below is divided into cells opening into the tunnel by oblong orifices exactly corresponding vertically. Each alternate cell of the upper series—the 1st, 3rd, 5th, &c.—communicates by a tube (*d d*) with the upper reservoir (*g*), its counterpart in the lower series having a free outlet into the air. In like manner the 2nd, 4th, 6th, &c., of the lower series of cells are connected with the lower reservoir (*i*); and each has its direct passage into the air through the cell immediately above it. The gas distributors are filled from both ends at the same time; the upper with carbonic acid gas, the lower with coal-gas, by branches from their respective supply pipes (*f*, *h*). A well-padded box (*l*) opening upon the end of the tunnel forms a little cavern, whence the sound-waves are sent forth by an electric bell. A few feet from the other end of the tunnel, in a direct line, is a sensitive flame (*k*), provided with a funnel as sound collector, and guarded from chance currents by a shade.

The bell was set ringing. The flame, with quick response to each blow of the hammer, emitted a sort of musical roar, so regular were its alternate shortenings and lengthenings as the successive sound-pulses reached it. The gases were then admitted. Twenty-five flat jets of coal gas ascended from the tubes below, and twenty-five cascades of carbonic acid poured down from the tubes above. That which was a homogeneous medium had now fifty limiting surfaces, from each of which a portion of the sound was thrown back. In a few moments these successive reflections became so effective that not a single sound-wave having sufficient power to affect a flame so sensitive as to be knocked down, crushed, as it were, by a chirrup, or jingle, at twenty feet distance, could pierce the clear, optically-transparent, but acoustically-opaque atmosphere in the tunnel. So long as the gases continued to flow, the flame remained perfectly tranquil. When the supply was cut off, the gases rapidly diffused into the air. The atmosphere of the tunnel became again homogeneous, and therefore acoustically transparent, and the flame bowed down to each sound-pulse as before. Alternate layers of common air and air saturated with various vapours produce the same effect.]

would then be strictly analogous to the reflection of light from an ordinary cloud to an observer placed between it and the sun.

My first care, in the early part of the day in question, was to assure myself that our inability to hear the sound did not arise from any derangement of the instruments. At one P.M. I was rowed to the shore, and landed at the base of the South Foreland cliff. The body of air which had already shown such extraordinary power to intercept sound, and which manifested this power still more impressively later in the day, was now in front of us. On it the sonorous waves impinged, and from it they were sent back to us with astonishing intensity. The instruments, hidden from view, were on the summit of a cliff

* Royal Institution, Friday evening Discourse by Prof. Tyndall, D.C.L. LL.D., F.R.S. Jan. 16. (Continued from page 253.)

235 feet above us, the sea was smooth and clear of ships, the atmosphere was without a cloud, and there was no object in sight which could possibly produce the observed effect. From the perfectly transparent air the echoes came, at first with a strength apparently but little less than that of the direct sound, and then dying gradually and continuously away. The remark of my companion, Mr. Edwards, was: "Beyond saying that the echoes seemed to come from the expanse of ocean, it did not appear possible to indicate any more definite point of reflection." Indeed, no such point was to be seen; the echoes reached us as if by magic, from absolutely invisible walls. Arago's notion that clouds are necessary to produce atmospheric echoes is therefore untenable.

The reflection from aërial surfaces has never been experimentally demonstrated. It is wholly a matter of inference, and I wished very much to reduce it to demonstration. I made one or two rough experiments on the transmission of sound through a series of flames; and no doubt by proper arrangement such experiments might be made successful. I then thought that alternate layers of carbonic acid and coal gas, the one rising by its lightness, the other falling by its weight, would supply a heterogeneous medium suitable for the demonstration. To my assistant, Mr. Cottrell, who possesses in an eminent degree the skill of devising apparatus, I communicated this idea, leaving the realisation of it wholly to him, and he has carried it out in the most admirable manner. (For a sketch and description of the apparatus, see previous page).

During my recent visit to the United States I accompanied General Woodruff, the engineer in charge of two of the lighthouse districts, to the establishments at Staten Island and Sandy Hook, with the express intention of observing the performance of the steam-syren, which, under the auspices of Prof. Henry, has been introduced into the lighthouse system of the United States. Such experiments as were possible to make under the circumstances were made, and I carried home with me a somewhat vivid remembrance of the mechanical effect of the sound of the steam syren upon my ears and body generally. This I considered to be greater than the similar effect produced by the horns of Mr. Holmes; hence the desire, on my part, to see the syren tried at the South Foreland. The formal expression of this desire was anticipated by the Elder Brethren, while their wishes were in turn anticipated by the courteous kindness of the Lighthouse Board at Washington. Informed by Major Elliott that our experiments had begun, the Board forwarded to the Corporation, for trial, the noble instrument now mounted at the South Foreland. The principle of the syren is easily understood. A musical sound is produced when the tympanic membrane is struck periodically with sufficient rapidity. The production of these tympanic shocks by puffs of air was first realised by Dr. Robison. But the syren itself is the invention of Cagniard de la Tour. He employed a box with a perforated lid, and above the lid a similarly perforated disc, capable of rotation. The perforations were oblique, so that when wind was driven through the disc was set in motion. When the perforations coincided a puff escaped, when they did not coincide the current of air was cut off. The regular succession of impulses thus imparted to the air produce a musical note. Even in its small form, the instrument is capable of producing sounds of great intensity. The syren has been improved upon by Dove, and notably developed by Helmholtz.

In the steam syren patented by Mr. Brown of New York, a fixed disc and a rotatory disc are also employed, radial slits being cut in both discs instead of circular apertures. One disc is fixed across the throat of a trumpet-shaped tube, 16½ ft. long, 5in. diameter where the disc crosses it, and gradually opening out till at the other extremity it reaches a diameter of 2 ft. 3 in. Behind the fixed disc is the rotating one, which is driven by separate mechanism. The trumpet is mounted on a boiler. In our experiments steam of 70 lbs. pressure has for the most part been employed. Just as in the air-syren, when the radial slits of the two discs coincide, a puff of steam escapes. Sound-waves of great intensity are thus sent through the air; the pitch of the note produced depending on the rapidity with which the puffs succeed each other; in other words, upon the velocity of rotation.

On October 8 I remained some time at the Foreland, listening to the echoes. Of the horn-echoes I have already spoken: those of the syren were still more extraordinary. Like the others they were perfectly continuous, and faded as if into the distance gradually away. The single sound seemed rendered complex and multitudinous by its echoes, which resembled a band of trumpeters first responding close at hand, and then retreating rapidly from us towards the coast of France. The syren echoes had eleven seconds duration, those of the horn eight seconds. With sounds of the same pitch the duration of the echo might be taken as a measure of the penetrative power of the sound.

I moved away from the station so as to lower the power of the direct sound. This was done by dropping into the sound-shadow behind an adjacent eminence. The echoes heard thus were still more wonderful than before. In the case of the syren, moreover, the reinforcement of the direct sound by the echo was distinct. One second after the commencement of the syren blast, the echo struck in as a new sound. This first echo, therefore, must have been flung back by a body of air not more than 600 or 700 ft. in thickness.

There appears to be a direct connection between the duration of the echoes and the distance penetrated by the sound. On October 17 the perfect clearness of the afternoon caused me to choose it for the examination of the echoes. The echoes of that day, when our transmitted sound reached its maximum, exceeded in duration those of all other days. We heard the syren fifteen miles off. On the close of the day we found its echoes fourteen to fifteen seconds in duration, this long duration indicating the distance from which they were thrown back.

The visual clearness of the atmosphere on the morning of Oct. 8, was very great, the coast of France was very plainly seen, the Grisnez lighthouse, and the monument and cathedral of Boulogne, were distinctly visible to the naked eye. At 5¼ miles from the station, the horn was heard feebly, the syren clearly. At 2.30 P.M., a densely black scowl overspread the heavens to the W.S.W. At this hour, the distance being 6 miles, the horn was heard very feebly, the syren more distinctly, all being hushed on board during the observations. A squall now approached us from the west. In the Alps, or elsewhere, I have rarely seen the heavens blacker. Vast cumuli floated in the N.E. and S.E.; vast streamers of rain were seen descending W.N.W.; huge scrolls of cloud to the N.

At 7 miles distance the syren was not strong, and the horn was very feeble.

The heavy rain at length reached us, but although it was falling all the way between us and the Foreland, the sound, instead of being deadened, rose perceptibly in power. Hail was now added to the rain, and the shower reached a tropical violence. We stopped. In the midst of this furious squall both the horn and the syren were distinctly heard, and as the shower lightened, thus lessening the local pattering, the sounds so rose in power that we heard them at a distance of 7½ miles distinctly louder than they had been heard through the rainless atmosphere at 5 miles. This observation is entirely opposed to the statement of Derham, which has been repeated by all writers since his time, regarding the stifling influence of falling rain upon sound. But it harmonises perfectly with our experience of July 3, which proved water in a state of *vapour* so mixed with air as to form non-homogeneous parcels, to be a most potent influence as regards the stoppage of sound. Prior to the violent shower, the air had been in this flocculent condition, but the descent of the rain and hail restored in part the homogeneity of the atmosphere, and augmented its transmissive power. There may be states of the atmosphere associated with rain unfavourable to sound, but to rain itself I have never been able to trace the slightest deadening effect.

The observations continued till November 25. Up to that date we had no fog, but the experience of July 1 and of October 30, entirely destroy the notion that optical transparency and acoustic transparency go hand-in-hand. Both were days of haze sufficiently thick to hide the cliffs of the Foreland, but on the former the sounds reached 12¾, and on the latter 11½ miles.

Reflection from the particles of fog and haze has been hitherto held to blot out sound. The late dense fog in London enabled experiments to be made which entirely controvert this conclusion. On December 10 I made some experiments over the Serpentine. The fog was very dense. Mr. Cotterell stood on the walk below the south-west end of the bridge dividing Hyde Park from Kensington Gardens, while I went to the eastern end of the Serpentine. He blew a dog-whistle, and an organ-pipe sounding Mi_3, which corresponds to 380 waves a second. I heard both distinctly. I then changed places with him, and listening attentively at the bridge, heard for a time the distinct blast of the whistle only. The organ-pipe at length sent its deeper note to me across the water. It sometimes rose to great distinctness, and sometimes fell to inaudibility. These fluctuations, of which various striking examples have been observed, are due to the drifting of acoustic clouds, which act upon a source of sound, as the drifting of ordinary clouds upon the sun. The whistle showed the same intermittence as to period, but in the opposite sense, for when the whistle was faint the pipe was strong, and *vice versâ*.

There seemed to be an extraordinary amount of sound in the air. It was filled with a resonant roar from the Bayswater and Knightsbridge roads. The railway whistles were extremely distinct, while the fog-signals exploded at the various metropolitan stations kept up a loud and almost constant cannonade. I could by no means reconcile this state of things with the statements so categorically made regarding the influence of fog.

The water was on this day warmer than the air, and the ascending vapour was instantly in part condensed, thus revealing its distribution. Instead of being uniformly diffused, it formed wreaths and striæ. I am pretty confident that had the vapour been able to maintain itself as such, the air would have been far more opaque to sound. In other words, I believe that the very cause which diminished the optical transparency of the atmosphere augmented its acoustical transparency.

This conclusion was confirmed by numerous observations made while the fog lasted.*

On Dec. 13 the fog was displaced by a thin haze. We could plainly see from one bank of the Serpentine to the other, and far into Hyde Park beyond. There was a wonderful subsidence of the sound of the carriages, church bells, &c. Being at the bridge I listened for the sounds excited at the end of the Serpentine. With the utmost stretch of attention I could hear nothing. I walked along the edge of the water towards Mr. Cottrell, and when I had lessened the distance by one half, the sound of his whistle was not so distinct as it had been at the bridge on the day of the densest fog. Hence the optical cleansing of the air by the melting of the fog had so darkened it acoustically, that a sound generated at the end of the Serpentine was lowered to at least one-fourth of its intensity at a point midway between the end and the bridge.

This opportune fog enabled me to remove the last of a congeries of errors which, ever since the year 1708, have attached themselves to this question. As regards phonic coast-signals, we now know exactly where we stand.

It is worth observing here that the solution of the deportment of hail, rain, snow, haze, and fog, as regards sound, depends entirely upon observations made on the 3rd of July, which was about the last day that one would have chosen for experiments on fog-signals. Indeed, it had been distinctly laid down that observations on such a day would be useless; that they might indeed enable us to weed away bad instruments from good ones, but could throw no light whatever on the question of fog-signaling. That the contrary is the case, is an illustration of the fact that the solution of a question often lies in a direction diametrically opposed to that in which it appears to lie. †

* Since the first notices of this lecture appeared in the newspapers, strong confirmatory evidence has been received.

† The foregoing report was compiled from the notes of Prof. Tyndall. It is published with Prof. Tyndall's sanction, but was not written by himself.

EXTRACTS FROM AN ADDRESS BY SIR W. THOMSON, TO THE SOCIETY OF TELEGRAPHIC ENGINEERS

I HAVE advisedly, not thoughtlessly, used the expression "terrestrial electricity." It is not an expression we are accustomed to. We are accustomed to "terrestrial magnetism;" we are accustomed to atmospheric electricity. The electric telegraph forces us to combine our ideas with reference to terrestrial magnetism and atmospheric electricity. We must look upon the earth and the air as a whole—a globe of earth and air—and consider its electricity whether in rest or in motion. Then, as to terrestrial magnetism, of what its relation may be to perceptible electric manifestations we at present know nothing.

You all know that the earth acts as a great magnet. Dr. Gilbert, of Colchester, made that clear nearly 300 years ago; but how the earth acts as a great magnet—how it is a magnet,—whether an electro-magnet in virtue of currents revolving round under the upper surface, or whether it is a magnet like a mass of steel or load-stone, we do not know. This we do know, that it is a variable magnet, and that a first approximation to the variation consists in a statement of motion round the axis of figure—motion of the magnetic poles, round the axis of figure, in a period of from 900 to 1,000 years. The earth is not a uniformly magnetised magnet with two poles, and with circles of symmetry round those poles. But a first expression—as we should say in mathematical language the first "harmonic term"—in the full expression of terrestrial magnetism is an expression of a regular and symmetrical distribution such as I have indicated. Now, this is quite certain, that the axis of this first term, so to speak, or this first approximation, which, in fact, we might call the magnetic axis of the earth, does revolve round the axis of figure.

When the phenomena of terrestrial magnetism were first somewhat accurately observed about three hundred years ago, the needle pointed here in England a little to the east of north; a few years later it pointed due north; then, until about the year 1820, it went to the west of north; and now it has come back towards the north. The dip has experienced corresponding variations. The dip was first discovered by the instrument maker, Robert Norman, an illustration, I may mention in passing, of the benefits which abstract science derives from practical applications—one of the most important fundamental discoveries of magnetism brought back to theory by an instrument maker who made mariner's compasses. Robert Norman, in balancing his compass cards, noticed that after they were magnetised one end dipped, and he examined the phemonenon and supported a needle about the centre of gravity, magnetised it, and discovered the dip. When the dip was first so discovered by Robert Norman it was less than it is now. The dip has gone on increasing, and is still increasing; but about 50 years ago the deviation from true north was greatest. Everything goes on as if the earth had a magnetic pole revolving from west to east round the true North Pole, at a distance of 20° from it. About three hundred years ago its azimuth from England was a little to the east of the north pole: then it came round, moving eastwards on the far side of the north pole, and round in a circle towards us on the left-hand side of the north pole, as looked to from England. That motion in a circle round the north pole has already been experienced within the period during which somewhat accurate measurements have been made—has been experienced to the extent of rather more than a quarter of the whole revolution; and we may expect that about 200 years from the present time the magnetic pole will be between England and the North Pole; so that the needle will thus point due north, and the dip be greater than it has been for 1,000 years, or will be for another. It is one of the greatest mysteries of science, a mystery which I might almost say is to myself a subject of daily contemplation—what can be the cause of this magnetism in the interior of the earth? Rigid magnetisation, like that of steel or the loadstone, has no quality in itself in virtue of which we can conceive it to migrate round in the magnetised bar. Electric currents afford the more favoured hypothesis; they are more mobile. If we can conceive electric currents at all, we may conceive them flitting about. But what sustains the electric currents? People sometimes say, heedlessly or ignorantly, that thermo-electricity does it. We have none of the elements of the problem of thermo-electricity in the state of underground temperature which could possibly explain, in accordance with any knowledge we have of thermo-electricity, how there could so be sustained currents round the earth. And if there were currents round the earth, regulated by some cause so as to give them a definite direction at one time, we are as far as ever from explaining how the channel of those currents could experience that great secular variation which we know it does. Thus we have merely a mystery. It would be rash to suggest even an explanation. I may say that one explanation has been suggested. It was suggested by the great astronomer, Halley, that there is a nucleus in the interior of the earth, and that the mystery is explained simply by a magnet not rigidly connected with the upper crust of the earth, but revolving round an axis differing from the axis of rotation of the outer crust, and exhibiting a gradual precessional motion independent of the precessional motion of the outer rigid crust. I merely say that has been suggested. I do not ask you to judge of the probability: I would not ask myself to judge of the probability of it. No other explanation has been suggested.

But now, I say, we look with hopefulness to the practical telegraphist for data towards a solution of this grand problem. The terrestrial magnet is subject, as a whole, to the grand secular variation which I have indicated. But, besides that, there are annual variations and diurnal variations. Every day the needle varies from a few minutes on one side to a few minutes on the other side of its mean position, and at times there are much greater variations. What are called "magnetic storms" are of not very unfrequent occurrence. In a magnetic storm the needle will often fly twenty minutes, thirty minutes, a degree, or even as much as two or three degrees sometimes, from its proper position—if I may use that term—its proper position for the time; that is, the position which it might be expected to have at the time according to the statistics of previous observations. I speak of the needle in general. The ordinary observation of the horizontal needle shows these phenomena. So does observation on the dip of the needle. So does observation on the total intensity of the terrestrial magnetic force.

The three elements, deflection, dip, and total intensity, all vary every day with the ordinary diurnal variation, and irregularly with the magnetic storm. The magnetic storm is always associated with a visible phenomenon, which we call, habitually, electrical;—aurora borealis, and, no doubt, also aurora of the southern polar regions. We have the strongest possible reasons for believing that aurora consists of electric currents, like the electric phenomena presented by currents of electricity through what are called vacuum tubes, through the space occupied by vacuums of different qualities in the well-known vacuum tubes. Of course, the very expression, "vacuums of different qualities" is a contradiction in terms. It implies that there are small quantities of matter of different kinds left in those nearest approaches to a perfect vacuum which we can make.

Well now, it is known to you all that aurora borealis is properly comparable with the phenomena presented by vacuum tubes. The appearance of the light, the variations which it presents, and the magnetic accompaniments, are all confirmatory of this view, so that we may accept it as one of the truths of science. Well now—and here is a point upon which, I think, the practical telegraphist not only can, but will, before long give to abstract science data for judging—is the deflection of the needle a direct effect of the auroral current, or are the auroral current and the deflection of the needle common results of another cause?

With reference to this point, I must speak of underground currents. There again I have named a household word to everyone who has anything to do with the operation of working the electric telegraph, and not a very pleasing household word I must say. I am sure most practical telegraphers would rather never hear of earth currents again. Still we have got earth currents; let us make the best of them. They are always with us; let us see whether we cannot make something of them, since they have given us so much trouble. Now, if we could have simultaneous observations of the underground currents, of the three magnetic elements, and of the aurora, we should have a mass of evidence from which, I believe, without fail, we ought to be able to conclude an answer more or less definite to the question I have put. Are we to look in the regions external to our atmosphere for the cause of the underground currents, or are we to look under the earth for some unknown cause affecting terrestrial magnetism, and giving rise to an induction of those currents? The direction of the effects, if we can only observe those directions, will help us most materially to judge as to what answer should be given.

It is my desire to make a suggestion which may reach members of this society, and associates in distant parts of the world. I make it not merely to occupy a little time in an inaugural address, but with the most earnest desire and expectation that something may be done in the direction of my suggestion. I do not venture to say that something may come from my suggestion, because, perhaps, without any suggestion from me, the acute and intelligent operators whom our great submarine telegraph companies have spread far and wide over the earth, are fully alive to the importance of such observations as I am now speaking of. I would just briefly say that this kind of observation is what would be of value for the scientific problem—to observe the indication of an electrometer at each end of a telegraph line at any time, whether during a magnetic storm or not, and at any time of the night or day. If the line be worked with a condenser at each end, this observation can be made without in the slightest degree influencing, and therefore without in the slightest degree disturbing, the practical work throughout the line. Put on an electrometer in direct connection with the line, connect the outside of the electrometer with a proper earth connection, and it may be observed quite irrespectively of the signalling; when the signalling is done, as it very frequently is at submarine lines, with a condenser at each end. The scientific observation will be disturbed undoubtedly, and considerably disturbed by the sending of messages, but the disturbance is only transient, and in the very pause at the end of a word there will be a sufficiently near approach to steadiness in the potential at the end of a wire connected with the electrometer to allow a careful observer to estimate with practical accuracy the indication that he would have were there no working of the line going on at the time. A magnetic storm of considerable intensity does not stop the working, does indeed scarcely interfere with the working, of a submarine line in many instances when a condenser is used at each end.

Thus, observations, even when the line is working, may be made during magnetic storms, and again, during hours when the line is not working, if there are any, and even the very busiest lines have occasional hours of rest. Perhaps, then, however, the operators have no time or zeal left, or, rather, I am sure they have always zeal, but I am not sure that there is always time left, and it may be impossible for them to bear the strain longer than their office hours require them. But when there is an operator, or a superintendent, or a mechanic, or an extra operator who may have a little time on his hands, then, I say, any single observation or any series of observations that he can make on the electric potentials at one end of an insulated line will give valuable results. When arrangements can be made for simultaneous observations of the potentials by an electrometer at the two ends of the line, the results will be still more valuable.

And, lastly, I may just say that when an electrometer is not available, a galvanometer of very large resistance may be employed. This will not in the slightest degree interfere with the practical working any more than would an electrometer, nor will it be more difficult to get results of the scientific observations not overpoweringly disturbed by the practical working if a galvanometer is used than when an electrometer is available. The more resistance that can be put in between the cable and the earth in circuit with the galvanometer the better, and the sensibility of the galvanometer will still be found perhaps more than necessary. Then, instead of reducing it by a shunt, let steel magnets, giving a more powerful direction to the needle, be applied for adjusting it. The resistance in circuit with the galvanometer between cable-end and earth ought to be at least twenty-times the cable's copper-resistance to make the galvanometer observations as valuable as those to be had by electrometer.

I should speak also of the subject of atmospheric electricity. The electric telegraph brings this phenomenon into connection with terrestrial magnetism with earth currents, and through them with aurora borealis, in a manner for which observations made before the time of the electric telegraph, or without the aid of the electric telegraph, had not given us any data whatever. Scientific observations on terrestrial magnetism, and on the aurora, and on atmospheric electricity, had shown a connection between the aurora and terrestrial magnetism in the shape of the disturbances that I have alluded to at the time of magnetic storms; but no connection between magnetic storms and atmospheric electricity, thunderstorms, or generally the state of the weather—what is commonly called meteorology—has yet been discovered.

The one common link connecting these phenomena hitherto known to us is exhibited in the electric telegraph. A telegraphic line—an air line more particularly, but a submarine line also—shows us unusually great disturbances, not only when there are auroras and variations of terrestrial magnetism, but when the atmospheric electricity is in a disturbed state. That it should be so electricians here present will readily understand. They will understand when they consider the change of electrification of the earth's surface which a lightning discharge necessarily produces.

I fear I might occupy too much of your time, or else I would just like to say a word upon atmospheric electricity, and to call your attention to the quantitative relations which questions in connection with this subject bear to those of ordinary earth currents and the phenomena of terrestrial magnetism. In fair weather, the surface of the earth is always, in these countries at all events, found negatively electrified. Now the limitation to these countries that I have made suggests a point for the practical telegraphists all over the world. Let us know whether it is only in England, France, and Italy that in fine weather the earth's surface is negatively electrified.

The only case of exception on record to this statement is Prof. Piazzi Smyth's observations on the Peak of Teneriffe. There, during several months of perfectly fair weather, the surface of the mountain was, if the electric test applied was correct, positively electrified; but Prof. Piazzi Smyth has, I believe, pointed out that the observations must not be relied upon. The instrument, as he himself found, was not satisfactory. The science of observing the atmospheric electricity was then so much in its infancy that, though he went prepared with the best instrument, and the only existing rules for using it, there was a fatal doubt as to whether the electricity was positive or negative after all. But the fact that there has been such a doubt is important. Now I suppose there will be a telegraph to Teneriffe before long, and then I hope and trust some of the operators will find time to climb the Peak. I am sure that, even without an electric object, they will go up the Peak. Now they must go up the Peak with an electrometer in fine weather, and ascertain whether the earth is positively or negatively electrified. If they find that on one fine day it is negatively electrified, the result will be valuable to science; and if on several days it is found to be all day and all night negatively electrified, then there will be a very great accession to our knowledge regarding atmospheric electricity.

When I say the surface of the earth is negatively electrified, I make a statement which I believe was due originally to Peltier. The more common form of statement is that the air is positively electrified, but this form of statement is apt to be delusive. More than that, it is most delusive in many published treatises, both in books and encyclopædias upon the subject. I have in my mind one encyclopædia in which, in the article "Air, Electricity of," it is said that the electricity of the air is positive, and increases in rising from the ground. In the same encyclopædia, in the article "Electricity, Atmospheric," it is stated that the surface of the earth is negatively electrified, and that the air in contact with the earth, and for some height above the earth, is, in general, negatively electrified. I do not say too much, then, when I say that the statement that the air is positively electrified has been at all events a subject for ambiguous and contradictory propositions; in fact, what we know by direct observation is, that the surface of the earth is negatively electrified, and positive electrification of the air is merely inferential.

Suppose, for a moment, that there were no electricity whatever in the air—that the air were absolutely devoid of all electric manifestation, and that a charge of electricity were given to the whole earth. For this no great amount would be necessary. Such amounts as you deal with in your great submarine cables would, if given to the earth as a whole, produce a very considerable electrification of its whole surface. You all know the comparison between the electricity of one Atlantic cable—the electro-static capacity of one of the Atlantic cables—with the water round its gutta-percha for outer coating, and the earth and air with infinite space for its outer coating.* I do not remember the figures at this moment; in fact, I do not remember which is the greater. Well, now, if all space were non-conducting—and experiments on vacuum tubes seem rather to support the possibility of that being the correct view—if all space were non-conducting, our atmosphere being a non-conductor, and the rarer and rarer air above us being a non-conductor, and the so-called vacuous space, or the interplanetary space beyond that (which we cannot admit to be really vacuous) being a non-conductor also, then a charge could be given to the earth as a whole, if there were the other body to come and go away again, just as a charge could be given to a pith ball electrified in the air of this room. Then, I say, all the phenomena brought to light by atmospheric electrometers, which we observe on a fine day, would be observed just as they are. The ordinary observation of atmospheric electricity would give just the result that we obtain from it. The result that we obtain every day in observations on atmospheric electricity is precisely the same as if the earth were electrified negatively and the air had no electricity in it whatever.

Well, now I have asserted strongly that the lower regions of the air are negatively electrified. On what foundation is this assertion made? Simply by observation. It is a matter of fact; it is not a matter of speculation. I find that when air is drawn into a room from the outside, on a fine day, it is negatively electrified. I believe the same phenomena will be observed in this city as in the old buildings of the University of Glasgow, in the middle of a very densely-peopled and smoky part of Glasgow; and therefore I doubt not that when air is drawn into this room from the outside, and a water-dropping collector is placed in the centre of the room, or a few feet above the floor, and put in connection with a sufficiently delicate electrometer, it will indicate negative electrification. Take an electric machine; place a spirit lamp on its prime conductor; turn the machine for a time; take an umbrella, and agitate the air with it till the whole is well mixed up; and keep turning the machine, with the spirit-lamp burning on its prime conductor. Then apply your electric test, and you find the air positively electrified.

Again—Let two rooms, with a door and passage between them, be used for the experiment. First shut the door and open the window in your observing room. Then, whatever electric observations you may have been performing, after a short time you find indications of negative electrification of the air. Then, during all that time, let us suppose that an electric machine has been turned in the neighbouring room, and a spirit-lamp burning on its prime conductor. Keep turning the electric machine in the neighbouring room, with the spirit-lamp as before. Make no other difference but this—shut the window and open the door. I am supposing that there is a fire in your experimenting room. Then, when the window was open and the door closed, the fire drew its air from the window, and you got the air direct from without. Now shut the window and open the door into the next room, and gradually the electric manifestation changes. And here somebody may suggest that it is changed because of the opening of the door and the inductive effect from the passage. But I anticipate that criticism by saying that my observation has told me that the change takes place gradually. For a time after the door is opened and the window closed, the electrification of the air in your experimenting room continues negative, but it gradually becomes zero, and a little later becomes positive. It remains positive as long as you keep turning the electric machine in the other room and the door is open. If you stop turning the electric machine, then, after a considerable time, the manifestation changes once more to the negative; or if you shut the door and open the window the manifestation changes more rapidly to negative.

It is, then, proved beyond all doubt that the electricity which comes in at the windows of an ordinary room in town is ordinarily negative in fair weather. It is not always negative, however. I have found it positive on some days. In broken weather, rainy weather, and so on, it is sometimes positive and sometimes negative. Now, hitherto, there is no proof of positive electricity in the air at all in fine weather; but we have grounds for inferring that probably there is positive electricity in the upper regions of the air. To answer that question the direct manner is to go up in a balloon, but that takes us beyond telegraphic regions, and therefore I must say nothing on that point. But I do say that superintendents and telegraph operators in various stations might sometimes make observations; and I do hope that the companies will so arrange their work, and provide such means for their spending their spare time, that each telegraph-station may be a sub-section of the Society of Telegraph Engineers, and may be able to have meetings, and make experiments, and put their forces together to endeavour to arrive at the truth. If telegraph operators would repeat such experiments in various parts of the world, they would give us most valuable information.

And we may hope that besides definite information regarding atmospheric electricity, in which we are at present so very deficient, we shall also get towards that great mystery of nature —the explanation of terrestrial magnetism and its associated phenomena,—the grand secular variation of magnetism, the magnetic storms, and the aurora borealis.

* The earth's radius is about 630 million centimetres, and its electrostatic capacity is therefore 630 microfarads, or about that of 1,600 miles of cable.

NOTES

We have frequently had occasion to refer to the energy and work of the Perthshire Society of Natural Science, and we rejoice to see that at its last meeting it has shown an example which we hope will be followed sooner or later by all scientific societies; it has resolved to make its influence felt in parliamentary elections. On the motion of the secretary, Dr. White, the following resolution was unanimously adopted:—"That in respect that Britain is apparently rapidly losing that commercial and manufacturing supremacy which has heretofore distinguished her, and that it is high time that the Government of this country should take steps to retain that supremacy, and that means towards that desirable end is the appointment of a *responsible Minister of Education* whose duty it will be to see that our education machinery in all departments, both in extent and in efficiency, is kept up to the wants of the age, and that a thorough general education in the scientific principles on which the arts are founded (and without which training mere technical schools are of no use), is put within the reach of all, this society resolves that the candidates for the representation in Parliament of the County and City of Perth be respectfully requested to state, whether, in the event of their being elected they will use their influence to urge upon the Government; (1) the appointment of such responsible Minister of Education; (2) the promotion of scientific exploration expeditions, such as that of an Arctic expedition which the late Government was in vain requested to promote; (3) the providing of means for carrying on unremunerative scientific research." The secretary was accordingly directed to communicate with the candidates.

THE post of Hydrographer to the Navy has been bestowed by Mr. Goschen on Capt. J. O. Evans, R.N., C.B., F.R.S., in succession to Rear-Admiral Richards, C.B., F.R.S., who has retired.

THE first four wranglers on this year's Cambridge Mathematical Tripos, are, George C. Calliphronas, of Gonville and Caius College; Walter W. R. Ball, of Trinity College; James R. Harris, of Clare College; and Andrew Craik, of Emmanuel College.

THE following lectures in Natural Science will be given at Trinity, St. John's, and Sidney Sussex Colleges, during Lent Term, 1874:—On Sound and Light (for the Natural Sciences Tripos), by Mr. Trotter, Trinity College, in Lecture-room No. 11 (Monday, Wednesday, Friday, at 11, commencing Wednesday, Feb. 4); On Electricity and Magnetism (for the first part of the Natural Sciences Tripos and the special examination for the Ordinary Degree), by Mr. Trotter, Trinity College, in Lecture Room No. 11 (Tuesday, Thursday, Saturday, at 11, commencing Thursday, Feb. 5); On Inorganic Chemistry, by Mr. Main, St. John's College (Tuesday, Thursday, Saturday, at 12, in St. John's College Laboratory, commencing Thursday, January 29). Attendance on these lectures is recognised by the University for the certificate required by medical students previous to admission for the first examination for the degree of M.B. Instruction in practical chemistry will also be given. On Palæontology (the Annuloida, &c.), by Mr. Bonney, St. John's College (Tuesdays and Thursdays, at 9, commencing Tuesday, February 3). On Geology (for the Natural Sciences Tripos, Physical Geology), by Mr. Bonney, St. John's College (Mondays, Wednesdays, and Fridays, at 10, commencing Wednesday, February 4). Lithology: demonstrations with the microscope every Saturday at 11, commencing February 7. The class will be limited to six, and preference gived to members of the above colleges. Elementary Geology (for the First Part of the Tripos and the special examination) (Tuesdays and Thursdays, at 11. commencing Thursday, February 5). On Botany, for the Natural Sciences Tripos, by Mr. Hicks, Sidney College (Tuesday, Thursday, Saturday, at 11, in Lecture-room No. 1, beginning on Tuesday, February 3). The lectures during this term will be on Vegetable Histology and Physiology. A Course of Practical Physiology, by the Trinity Prælector in Physiology (Dr. Michael Foster) at the new museums. Lectures on Tuesday, Thursday, Saturday, at 12, commencing Tuesday, January, 27. This course is a continuation of that given last term.

DR. SCHMIDT, Professor of Astronomy in the University of Athens, has just completed his great map of the Moon. It is *two metres* in diameter, and is a marvel of accurate mapping and minute draughtmanship. The shading is so exquisite that any part of the map may be examined by a lens without the appearance of coarse or rough work. The map represents the labour of thirty-four years. and is without doubt one of the greatest astronomical results of the century.

THE discourse at the Royal Institution on Friday next, Feb. 6, at 9 P.M., will be by Mr. A. H. Garrod, Fellow of St. John's College, Cambridge, "On the Heart and the Sphygmograph."

A MARINE and Fresh-water Aquarium is to be established in the Central Park, New York, in connection with the Free Museum and Menagerie already erected there; it will be placed under the superintendence of Mr. W. Saville Kent, F.Z.S., who was, until a short time ago, Curator of the Brighton Aquarium. It is intended to raise the requisite funds by public subscription, and we are very pleased to be able to add that it is proposed to endow the Institution, so that it may be made available for the purposes of scientific research.

A PROJECT is on foot for the erection of a public aquarium at Liverpool, and a Company has been formed for this purpose; a suitable site has been secured close to the Philharmonic Hall, and operations will, we believe, be commenced at once. It is estimated the building will cost about 45,000*l*.

THE exhibition of appliances for the economic consumption of coal, which has been formed in the Peel Park, Salford, by the Society for Promoting Scientific Industry, was formally opened on Friday. Mr. J. Lowthian Bell, who had been announced to open the Exhibition, was prevented from being present, but forwarded the copy of an address which he had intended to deliver. This was read by the secretary, Mr. Larkins. The Exhibition will remain open for some weeks, and wil doubtless receive its share of public notice when the elections are occupying less attention than they are at present.

WE learn from the *Athenæum* that the Trustees of the British Museum have agreed to resign their patronage into the hands of the Government.

AN interesting peculiarity in the habits of some Indian Siluroid fishes has been noticed at a recent meeting of the Zoological Society by Surgeon F. Day, which will be described in full in the forthcoming Part of the Proceedings of that Society. Mr. Day, when fishing at Cassegode, found that, after having caught a large number of specimens of various species of *Arius* and *Osteogeniosus*, there were several siluroid eggs at the bottom of the boats, and in the fish-baskets. These eggs were, on an average, half-an-inch in diameter; and on looking into the mouths of several of the males of both genera, from fifteen to twenty eggs were seen in each; those in the boats and baskets having evidently dropped out from a similar situation. The eggs were in different stages of development, some advanced so far as to be just hatched. They filled the mouth, extending as far back as the branchiæ. No food was found in the alimentary canal, though in the females it was full of nutriment.

IN a paper on the Meteors of January 2, read before the American Philosophical Society, by Prof. Daniel Kirkwood, the author states, founding on data extending from A.D. 849 to 1864, that the meteors of this group have probably a period of thirteen years; that the mean distance is 5.53, aphelion 10.06; and that the source of the meteors may be the fourth comet of 1860, which in its ascending node approaches very near the point passed by the earth about January 3. If the period be thirteen years, the comet should have returned in the latter part of 1873, and the maximum fall of the associated meteors should occur about 1877.

Two legacies have recently been left to the French Academy of Sciences for the purpose of founding prizes. The one, a perpetual legacy of 2,500 francs, has been bequeathed by the late M. Gay, to be awarded as a prize in physical geography; and the other, a sum of 10,000 francs, the interest of which is to be awarded to the author of an astronomical work.

A GENTLEMAN in Glasgow who does not wish his name to be given, has just made a donation to Glasgow University of 1000*l*., for the better endowment of the chairs of astronomy, botany, and natural history.

AT the meeting of the Academy of Sciences at Paris on Monday, January 26, the place in the section of Anatomy and Zoology, vacant by the death of M. Coste, was filled up. M. P. Gervais was elected, but M. Alph. Milne-Edwards was a good second, obtaining 24 votes to M. Gervais's 33.

A NEW work by Mr. F. W. Burbidge, on "Cool Orchids, and how to grow them," is announced by Mr. Hardwicke, Piccadilly. It will be illustrated by coloured plates and wood engravings, and will be furnished with a copious list, in the shape of an index, of what are termed cool Orchids.

IN a despatch from Mr. Williams, H.M.'s Consul at Samoa, to the Foreign Secretary, dated Sydney, Oct. 28, 1873, it is stated that gold in quartz has been found in a valley in that island, about three miles from the Port of Apia; the samples assayed yielded at the rate of 3000 ozs. to the ton.

MR. J. F. GARDNER, geographer to Prof. Hayden's survey, in giving a short sketch of the method adopted by him to determine the altitude of the various points occupied by the party in the Rocky Mountains, states that the experience of the surveys of California and of the fortieth parallel show that in the determination of the altitude of any point a mercurial barometer is liable to an error varying from 150 to 300 feet, even when the base barometer is at the foot of the peak, and only 3000 feet below the summit. In connection with Professor Whitney (chief of the California Survey), the following plan was adopted for correcting the errors of barometrical work. Four points were chosen at successive levels of from one to 14,000 feet. These stations were carefully connected by levellings with a spirit level, and were occupied as permanent meteorological stations. The observations taken by field parties are classified according to their heights, and each class is referred to the base station which is nearest its own elevation; the lower station being Denver, the fourth the summit of Mount Lincoln (14,000 feet), where are a number of silver mines worked by Captain Breese. The central position of this peak admirably fits it for the base of reference. Besides the barometric determination of heights, two connected systems of trigonometric levelling have been carried over the whole area surveyed, and the check observations are so arranged that the probable error can be easily determined, and it is hoped that the system will prove accurate enough to throw some light on the amount of refraction at great elevations. By these methods the altitudes of many high points have been determined, from which to construct a map in contours 200 vertical feet apart, on a scale of two miles to one inch.

SIGNALLING between the earth and the planet Venus is a suggestion made in all good faith by a French astronomer, M. Charles Cros, who considers the coming transit of Venus to be a good opportunity for ascertaining whether there are inhabitants on that planet, and, if so, entering into relations with them. He says: "It is possible that Venus is inhabited; that amongst its inhabitants are astronomers; that the latter judge the passage of their planet across the solar disc to be an object to excite our curiosity; finally it is possible that these *savants* will strive in some way to make signals to us at the precise moment when they might suppose that many telescopes will be levelled at their planet."

IN a recent communication to the Connecticut Academy of Arts and Sciences, Prof. Marsh gave a statement of the results of his recent expedition to the Far West in search of fossil remains of extinct vertebrates. He said the richest field for exploration was found in the great basin of the pre-historic lake which is now drained by the Colorado River. This body of water was originally as large as all the present lakes of the North-West combined, and had existed so long that the sand washed down from the surrounding hills had accumulated to the depth of a mile. In the different strata of this bed at least ten distinct groups of extinct animals could be detected, among them some extremely remarkable forms. One of these was a rhinoceros with two horns; but these, not like those of the modern rhinoceros, in the axis of the body, but transversely. In a space of 10 ft. square he had sometimes found the bones of 30 different animals. The number of species of extinct mammals in these remains he estimates to be three times as great as that at present inhabiting the same locality.

A PAPER on Electrical Warfare will be read by Mr. Nath. J. Holmes, at the Society of Telegraph Engineers, on Wednesday, the 11th inst.

THE new Holmes' Shipwreck Distress Signal, of great power, will be exhibited from Primrose Hill on Thursday evening, 12th, at 8.30, in presence of the Marine Secretary of the Board of Trade. This signal is self-igniting in water, and inextinguishable.

THE Naples correspondent of the *Times*, writing on Jan. 25, states that Prof. Palmieri has just published the following letter in answer to the numerous applications sent to him for information:—"The activity of Vesuvius continues to increase in the crater towards the N.E. Frequent globes of smoke issue from the bottom of it, with a kind of hissing sound, accompanied by an unpleasant odour of chloridic and sulphuric acids. Not far from it, at the commencement of the grand fissure of 1872, alkaline sublimates make their appearance. Meanwhile the fire does not yet show greater activity at the bottom of the crater, where it will probably manifest itself, unless some eccentric eruption should occur before the internal resistance of this crater is overcome. The great subterranean energy now at work does, indeed, appear to be making an attempt at an outlet in various parts. On the 21st inst. a slight undulatory shock of earthquake was felt at Casamicciola, in the island of Ischia, and during the last week many have heard the low continuous mutterings of the mountain at a distance of 15 miles. As I write, however, the sismograph, which has been very agitated for some days, is more quiet." He also reports the melancholy death at Casamicciola of Mr. Moggridge, who having bathed in the open sea, died on his road to the hotel.

WE have received the Report of the *Senckenbergische naturforschende Gesellschaft* for 1872-73, a society of long standing, and with several eminent names in its list of members. The membership, we are glad to learn, shows a considerable increase during the year; though M. v. Fritsch states, in his report, that the efforts of the society are sadly hampered for lack of funds, and that "we exist and vegetate, rather than live." He laments, also, that the museum, which once stood fifth in importance in Europe, is being quickly surpassed by other like institutions, and thrown into the background; which is hardly creditable to a city of such wealth and culture as Frankfort. Among the researches detailed in this *Bericht*, we note a paper by Dr. Koch on the Arachnida of North Africa, especially those (hitherto unstudied) of the Atlas region, and the coast of Morocco; the material having been collected by Drs. von Fritsch and Rein. The new types are not very numerous, but the remarkably wide distribution of spider-species is confirmed; and good illustration afforded of the influence of climate and other local conditions in modifying type forms. Dr. Rein describes some plants found in the neighbourhood of Mogador, and also furnishes a sketch of the vegetation of the Bermudas. A new species of perforating cirripede, *Kochlorine hamata* N., is described by Dr. Knoll; M. Scheidel contributes a note on lake dwellings and their inhabitants; and there are interesting accounts of journeys to Iceland, and to the Puglia Petrosa, in Italy.

WE have received the first Annual Report of the "Haileybury Natural Science Society." It contains preliminary lists of the fauna and flora of the place, together with observations on the meteorology of the locality, and a humorous description of an experimental dinner at which the principal dish consisted of esculent snails which had been specially fed and fattened for the purpose by certain members of the Society. It need scarcely be added, that the repast amply rewarded the members for their generous devotion to the cause of Science.

THE additions to the Zoological Society's Gardens during the past week include three Mauge's Dasyures (*Dasyurus maugæi*) from Australia, presented by Mr. J. Shaw; two Vulturine Guinea Fowl (*Numida vulturina*) from East Africa, presented by Dr. J. Kirk; a Chilian Sea-Eagle (*Geranoaëtus aguia*) from Bahia, presented by Mr. J. Judge; an Indian Leopard (*Felis pardus*) presented by Mr. G. D. Elphinstone; two Orang Outangs (*Simia satyrus*) from Borneo, and a Ungko Gibbon (*Hylobates variegatus*) from Sumatra, deposited; two Wanderoo Monkeys (*Macacus silenus*) from the Malabar Coast; a Brown Monkey (*Macacus brunneus*) and two Adjutants (*Leptoptilus argala*) from India, two Pheasant-tailed Pigeons (*Macropygia phasianella*) from N.S. Wales, and two Jambu Fruit Pigeons (*Ptilonopus jambu*) from the Indian Archipelago, purchased.

SCIENTIFIC SERIALS

American Journal of Science and Arts, December 1873.—In a paper on the magnetic permeability (that is "conductivity," according to Faraday), and the maximum of magnetism of iron, steel, and nickel, by Mr. Henry Rowland, C.E., the results are expressed, and the reasoning is carried out in the language of Faraday's lines of magnetic force. The quantity introduced, in mathematical theories of induced magnetisation, depending on the magnetic properties of the substance, is in these treated as a constant; but it was shown, in twelve cases of iron and two of nickel, to vary between wide limits. The author finds that the magnetisation of good iron can never exceed 175,000 times the unit magnetic field (on the metre, gramme, second, system), nor can nickel exceed 63,000 times; and from these data, and with aid of a formula of Prof. Maxwell's for tension of lines of force, it is inferred that the greatest weight which can be sustained by an electro-magnet with an infinite current, is, for iron, 354 lbs. per square inch of section, and for nickel 46 lbs. The results of experiment closely agreed with this.—Prof. Henry Draper communicates a note on diffraction-spectrum photography, accompanied with a photograph printed by the Alberttype process. (See NATURE, vol. ix. p. 223.)—We note several geological papers, one of them, by Prof. Fontaine, describing a remarkable deposit of bituminous matter, termed Grahamite, in Ritchie County, West Virginia, chemically resembling the mineral Albertite of New Brunswick, but differing considerably from this in its geological relations.—The age of the Lignitic formation of the Rocky Mountain region is far from decided, owing to the contrary evidence afforded by fossil plants and animals; and the editors propose to cite the arguments from various sources, in order, if possible, to bring about agreement. They give in this number the conclusions of M. Lesquereux from fossil plants. He refers the Lignitic beds to the Upper and Lower Eocene; and he gives a number of facts showing the disconnection of American Eocene flora from that of the Cretaceous, indicating truly separate formations.—Mr. Comstock describes the geology of Western Wyoming.—Mr. Verrill communicates the results of a recent dredging expedition on the coast of New England. It was ascertained that the body of cold bottom water approaches so nearly to the Coast of Maine as to manifest itself distinctly within twelve or fifteen miles of Cape Elizabeth, both by its highly Arctic fauna, and its icy temperature, even in summer.—In a letter from Cordoba, dated Sept. 8, 1873, Dr. Gould describes a remarkable swarm of locusts then occurring.

Astronomische Nachrichten, No. 1970, Jan. 14, contains the following papers:—On the determination of longitude by star-occultation and the telegraphically determined longitude between Madras, Singapore, and Batavia, by Dr. Oudemans. The author mentions his observations in 1859 as giving a longitude for Batavia of 7h. 7m. 12·5 s., also others in later years giving rather a less result. In 1870-71, however, the telegraphic communication with Singapore was used, giving a mean result of 11m. 40·895 s. longitude from that place. The same author gives a note on Kaiser's original proof of Foucault's pendulum researches. The proof is given by Prof. Oudemans, by which the plane of motion of the pendulum moves in azimuth in 1 sec., 15″. sin ϕ. It is too long to give in full here, but appears simple and good. Prof. Oudemans has also two other papers on position observation made during the eclipse of Dec. 1871 at Java, and on the Spheroidal form of the earth, which consist chiefly of equations and tables which we have not space to introduce.—Dr. Holetschek gives ephemerides of a number of the minor planets.

Der Naturforscher, December 1873.—This number contains notes from the Bothkamp Observatory. In one of them M. Vogel gives observations of the spectra of several fixed stars, comparing the results obtained by Huggins and Miller. Another treats of periodic changes in the atmosphere of Jupiter. The observation that the occurrence of certain coloured stripes in Jupiter, and of bright egg-shaped spots in his equatorial zone coincided with the maximum epoch of sunspots, appears to be confirmed by a number of fresh data collected by the writer, Dr. Lohse. A third note describes observations of Venus in 1871-73, by M. Vogel, who thinks it probable that the planet is surrounded with an atmosphere in which floats a thick and dense layer of condensation products, so that little insight is afforded to the planet's surface, and the observation of spots helps but little to ascertaining the time of rotation or the position of the axis of rotation.—In physics, we have a note on the curious fact which M. Budde has recently studied, viz., that chlorine, when acted on by very refrangible rays of light, undergoes expansion and heating. Some experiments, made by M. Hirn, on the optical properties of flame, tend to show that flame is not perfectly transparent to light (as Arago and M. Offret have affirmed), but that particles in the glowing state are; the weakening of light in its transmission through flames is due to the various refractions it undergoes, and consequent dispersion. The author is led to some speculations on the sun's temperature, and he puts the case thus: If the glowing parts of the photosphere are intransparent, the temperature must (according to mathematical calculation), be nearly six million degrees; if they are transparent, it must be considerably less; and the lower, the greater the transparency. The problem is one for experimental physics, the question being, Are all solid or liquid bodies transparent and diathermanous when brought to a very high temperature? M. Hirn, we have seen, inclines to reply in the affirmative. We find accounts of Prof. Guthrie's discovery of a new relation between heat and electricity, and M. Herwig's experiments on pulverisation of electrodes in the voltaic arch.—Chemistry is represented by papers on the laws governing water of crystallisation, and the reduction of carbonic acid by phosphate of iron.—The action of camphor on plant life has been recently studied by M. Vogel at Munich, in a series of experiments which confirm an almost forgotten observation by Barton in the last century, that camphor has a stimulant effect on plants analogous to that of spirituous liquors or opium, in certain quantity, on the human system. There are also botanical notes on the influence of CO_2 on verdant growth of plants (M. Böhm), and on the geographical distribution of the Cupuliferæ (M. Oersted); and, in technology, M. Riche discusses the physical properties of certain alloys.

SOCIETIES AND ACADEMIES

London

Geological Society, Jan. 21.—Prof. P. Martin Duncan, F.R.S., vice-president, in the chair.—"The secondary rocks of Scotland (second paper). On the ancient volcanoes of the Highlands and their relations to the Mesozoic strata," by J. W. Judd, F.G.S. That the rocks forming the great plateaux of the Hebrides and the north of Ireland are really the vestiges of innumerable lava-streams, is a fact which has long been recognised by geologists. That these lavas were of *subaerial* and not *subaqueous* origin is proved by the absence of all contemporaneous interbedded sedimentary rocks, by the evidently terrestrial origin of the surfaces on which they lie, and by the intercalation among them of old soils, forests, mud-streams, river-gravels, lake deposits, and masses of unstratified tuffs and ashes. From the analogy of existing volcanic districts, we can scarcely d ubt that these great accumulations of igneous products, which must originally have covered many thousands of square miles, and which still often exhibit a thickness of 2,000 ft., were ejected from great volcanic mountains; and a careful study of the district fully confirms this conclusion, enabling us, indeed, to determine the sites of these old volcanoes, to estimate their dimensions, to investigate their internal structure, and to trace the history of their formation. The following is Mr. Judd's conclusion on the subject of his paper:—It appears that during the Newer Palæozoic and the Tertiary periods, the north-western parts of the British archipelago were the scene of displays of volcanic activity upon the grandest scale. During either of these, the eruption of felspathic lavas, &c., preceded, as a whole, that of the basaltic; and in both the volcanic action was brought to a close by the formation of "puys." The range of Newer-Palæozoic volcanoes arose along a line striking N.E. and S.W.; that of the Tertiary volcanoes along one striking from N. to S.; and each appears to have been connected with a great system of subterranean disturbance. It is an interesting circumstance that the epochs of maximum volcanic activity, the Old Red sandstone and the Miocene, appear to have been coincident with those which, as shown by Prof. Ramsay, were characterised by the greatest extent of continental land in the area. The Secondary strata were deposited in the interval between the two epochs of volcanic activity, and the features which they present have been largely influenced by this circumstance. Apart from this consideration, however, the volcanic rocks of the Highlands are of the highest interest to the geologist, both from their enabling him to decipher to so great an extent the "geological records" of the district, and from the light which they throw upon some of the obscurest problems of physical geology.—Remarks on fossils from Oberburg, Styria, by A. W. Waters, F.G.S. The author noticed the limited occurrence of Eocene deposits in Styria, and referred briefly to the researches of Prof. Reuss and Prof. Stur upon them. He then indicated certain species of fossils which he had detected in these beds, adding about nine species to Stur's list.

Anthropological Institute, Jan. 27.—Prof. Busk, F.R.S., president, in the chair.—Aniversary Meeting.—Before proceeding to read his address, the president referred to the financial condition of the Institute, which, although it showed that the receipts were adequate for the necessary expenditure on the present economical principles of management, would not admit either of paying off any more of the debt or of increasing the scope and usefulness of the Institute. Until the unfortunate and utterly indefensible secession of members early in 1873, on a purely personal question, the Institute, since its formation, had paid off the combined debts of the two old societies at the rate of 100*l*. a year. He appealed to the loyalty of the members now forming the Institute to make a united effort finally to extinguish the debt of 800*l*. A year's income would do it, and it was suggested that if each member contributed one year's subscription, that great result would be attained and the Institute would certainly before long occupy a high position amongst the scientific bodies of the kingdom. As an encouragement to the body of members and as an earnest of the sincerity and vigour of his colleagues in management, the president had much pleasure in announcing that nearly 250*l*. had been promised by members present at a council-meeting held that day, provided the sum of 500*l*. be contributed by other members of the Institute.—The president then delivered the annual address, in which he viewed the work done during 1873 by English and foreign anthropologists. Amongst a large number of topics he adverted at considerable length to the important contributions to craniometry, by Dr. H. von Jhering and Dr. Paul Broca, criticising the respective methods employed by those distinguished anthropologists; and concluded that part of his address with the observation that the study of craniology is almost futile when applied to highly civilised, and consequently much mixed peoples, and that its results are the more certain in proportion to the purity of race. That purity at the present time was rapidly disappearing, and with it the surest data for the determination of the problems involved in the antiquity and physical origin of man.—The following was the list of officers and council elected to serve for 1874:—President—Prof. Geo. Busk, F.R.S. Vice-presidents—John Evans, F.R.S.; Col. A. Lane Fox, F.S.A.; A. W. Franks, M.A.; Francis Galton, F.R.S.; Prof. Huxley, F.R.S.; Sir John Lubbock, Bart., F.R.S. Director—E. W. Brabrook, F.S.A.—Treasurer—Rev. Dunbar I. Heath, M.A. Council—Dr. John Beddoe, F.R.S.; W. Blackmore; H. G. Bohn, F.R.G.S.; Dr. A. Campbell; Hyde Clarke; Dr. J. Barnard Davis, F.R.S.; W. Boyd Dawkins, F.R.S.; Robert Dunn, F.R.C.S.; David Forbes, F.R.S.; Sir Duncan Gibb, Bart, M.D.; George Harris, F.S.A.; J. Park Harrison, M.A. J. F. McLennan; C. R. Markham, C.B. F.R.S.; Frederic Ouvry, F.S.A.; F. G. H. Price, F.R.G.S.; J. E. Price, F.S.A.; F. W. Rudler, F.G.S.; C. R. Des Ruffières, F.R.S.L.; E. Burnet Tylor, F.R.S.

Edinburgh

Royal Physical Society, Jan 28.—Mr. Scot Skirving, president, in the chair.—The following communications were read: Note on the Crushed Boulders from the Old Red Conglomerate in Kincardineshire, by James C. Howden, M.D., —On Crushed Boulders from Arbroath, and other localities, by Mr. Charles W. Peach.—Report of the Dredging Committee for 1873, by James Middleton, M.B., convener. The meeting of the committee had been held conjointly with the Naturalists' Field Club. In all about 133 species of animals had been obtained, including two new to the Firth of Forth.—Note on the Suspension of Clay in Water, by Mr. William Durham. This research was undertaken in continuance of those recorded in the papers on the same subject read at the last meeting. As the general result of Mr. Durham's elaborate and careful series of experiments, it was found that clay held in suspension by water sinks more quickly if the water is slightly acidulated, and more slowly if a slight amount of an alkali is added, but that the conditions are reversed if a large amount of either substance is mixed with the water.

Manchester

Geological Society, Jan. 27.—Mr. J. Dickenson Hill in the chair.—Mr. J. Aitken exhibited some new fossil fishes from the millstone grit, Yorkshire, and read a paper descriptive of the bed whence they were obtained. He said that evidences of fossils had been brought to the surface during the excavations connected with the scheme for taking water from Widdop colliery to the borough of Halifax by a tunnel cut through Wadsworth Moor, about two miles north of Hebdenbridge. After an examination, by no means exhaustive, there had been discovered seven specimens of *Goniatites*, three of *Nautili*, two of *Orthoceratites*, two of *Avicula pecten*, two of *Posodonia*, one of *Gastropod*, one of *Milamia*, fish remains, &c. The discovery of the most remarkable character was a new species of *Acrolepis* presenting peculiar characteristics. The situation in which these remains occurred was near but somewhat above the middle of the shells which usually divided the third floors from the fourth or undermost grit.

Göttingen

Royal Academy of Sciences, Nov. 1, 1873.—M. Schering communicated a paper on the Hamilton-Jacobi theory for forces whose measurement depends on the motion of bodies.—MM. Wagner, Philippi, and Tollens described some researches on the Allyl group, made with the view of establishing the constitution of allyl alcohol, and of some of its compounds, especially acrylic acid. They find new evidence, in opposition to Wislicenus, that acrylic acid, as well as acetic acid, propionic acid, and all other organic acids, contains the group CO_2H, and may therefore be classed with them.—MM. von Grote and Tollens described an acid obtained from cane sugar by means of dilute sulphuric acid; and M. Tollens gave the first results of an investigation as to combinations of starch with alkali.

Nov. 20.—Prof. Lüroth read a paper on reckoning by projections; and Prof. Hattendorff made some observations on Sturm's theorem.

Dec. 3.—M. Enneper communicated a paper on the general theory of surfaces.

Dec. 10.—The Society celebrated its 121st anniversary. The prizes for competition in the next three years were announced. In the physical section the Society invites experiments on the artificial production of some crystallised minerals, as stephanite, pyrargyrite, grey copper ore, galena, fluor spar; in order to solution of the question how crystallised sulphur and fluor-compounds have arisen in the natural state. In the mathematical section, the Society desires an investigation of current-work, *i.e.* the work done by the electro-motive forces in their action on the current electricity, especially in its relation to the heat produced from the current, and the *vis viva* produced from it immediately in the current electricty, or mediately, in other movable particles in the conductor. Papers on these subjects must be sent before Sept. 1875, in the former case, and Sept. 1876, in the latter. The prizes offered are fifty ducats each.—Prof. Ewald communicated an interesting paper on the so-called rhetorical ornaments of Oriental speech (a subject suggested, apparently, by the late visit of the "king of kings)."—M. Riecke presented a note on the function of leaf-teeth, and the morphological value of some leaf-nectaries. In the bud, the teeth often prevent the hermetical closure of the two folded halves of the leaf; which is perhaps important, that the bud may not suffer from the want of gas. A more evident function consists in the separation of resin or mucilage. *Prunus avium* is taken as a good example; and two other types of structure are also described. The teeth of leaves of *Prunus avium* are closely allied, morphologically, to numerous nectar-secreting organs in these and other kinds of leaf.

Dec. 17.—M. Bjerknes read a paper giving a generalisation of the problem of motions produced in a still inelastic fluid by the motion of an ellipsoid.—M. Wöhler presented a list of the meteorites in the University collection at Göttingen.

Vienna

Imperial Academy of Sciences, Dec. 4, 1873.—Prof. Mach stated that he had made experiments, during the summer, on the time required for rotation of the plane of polarisation by a current—a flint glass disc being rotated between the magnetic poles; but similar experiments by Villari had been described in *Pogg. Ann.* (No. 7, 1873), and the results were almost identical. Villari used a double plate; and Prof. Mach points out another very simple method for such researches, viz., the spectral observation of a sounding glass rod placed between the magnetic poles.—A paper by Dr. Dvorak described some experiments on the velocity of sound in gas-mixtures. If a mixture is made of two different gases, with densities d and d' respectively, and both with an expansive force 1, the velocity of sound V in the mixture $= \sqrt{\frac{2}{d + d'}}$. The author's results show close agreement with the theory. Thus for mixtures of carbonic acid and hydrogen, air and hydrogen, ordinary gas and CO_2, respectively, the observed and calculated numbers for the half wave-length of a given tone were these: 71·5, 71·0; 88, 89·0; 64, 63·3. The author remarks that for a simple gas, as well as for a mixture of gases, the gas theory implies not one velocity, but a graduated series of velocities, of sound; and perhaps the prolongation in sound of a cannon shot heard at a distance may be thus explained.—Dr. Exner communicated a determination of the temperature at which water has a maximum of density. He improved on Rumford's method by using thermo-elements instead of a mercury thermometer. The value obtained was 3·945°.

Paris

Academy of Sciences, Jan. 26.—M. Bertrand in the chair.—The following papers were read: On the various reactions of the compounds of oxygen and nitrogen, by M. Berthelot.—On the production of yeast in a mineral solution containing sugar, by M. Pasteur. The author described the growth of yeast in a solution of inorganic substances such as enter into the composition of its ash added to a solution of sugar. M. Trécul replied at some length to certain of M. Pasteur's remarks.—On the liquefaction and solidification of acetylene by the silent electric discharge, by MM. P. and A. Thenard. The author found that this gas condensed at the rate of four or five cubic centimetres a minute into a solid horny body isomeric with acetylene; by varying the conditions of experiment a liquid isomer was also obtained.—Experimental researches on Newton's rings, by M. P. Desains.—Direct demonstration of the equation $\int \frac{dQ}{T} = 0$ for every closed and reversible cycle, by M. A. Ledieu. This paper formed a sequel to the author's other papers on thermo-dynamics, lately published.—Note on Poncelet's teaching of applied mechanics, by General Morin.—A note from Prof. Nordenskiöld was read; he has detected iron, nickel and cobalt in the carbonaceous dust found in 1870 on the Greenland snow; traces of phosphorus were also found.—Instructions for M. Doûmet-Adanson's travel in Tunis, by M. Cosson. The instructions are issued to M. Adanson, who is about to undertake a botanical exploration of Tunis.—On magnetism, by M. J. M. Gaugain.—New researches on the rejoining end to end of the fibres of sensory with the fibres of motor-nerves, by M. A. Vulpian.—Organogenesis compared with androgenesis in its relation to natural affinities, by Ad. Chatin. This portion of the author's paper deals with the *polygonoid* and *cactoid* plants.—Researches on the silicified plants of Autun; study of the genus *Myelopteris*, by M. B. Renault.—On the presence of a considerable proportion of potassic nitrate in two varieties of *Amaranthus*, by M. A. Boutin. The author found that *A. atropurpureus* contained 22·7 and *A. ruber* 16·0 per cent. of the weight of the dried plant; he suggested a possible future cultivation of the plant on this account.—On the theory of the flight of birds, by MM. H. and L. Planavergne.—On a statistical chart showing the distribution of the population of Paris, by M. Vauthier.—On the geometrical properties of rational fractions, by M. F. Lucas.—On the determination of the pluckerian numbers of envelopes, by M. H. G. Zeuthen.—On the theory of numerical equations, by M. Laguerre.—On the breaking of magnetised needles, by M. Bouty. The author found that if the steel was very brittle and broke like glass the two portions are magnets of the same magnetic moment, but not so if the steel has to be bent backwards and forwards before it breaks.—On certain peculiarities in the efflorescence of the two hydrates of sodic sulphate, by M. D. Gernez.—Researches on the reaction of argentic chloride on phosphoric di-iodide, by M. Arm. Gautier.—On the isomerism of terebenthene and terebene, from a physical point of view, by M. J. Ribau.—On the alterations of the soft matter (of the brain) accompanying the tearing and cutting back of the sciatic nerve in the rabbit, by M. G. Hayem.—On the pluvial *régime* of the torrid zones in the Indian and Pacific Ocean basins, by M. V. Raulin.—Note on Professor Tyndall's experiments on the acoustic transparency of air, by M. W. de Fonvielle.—On the production of crystals of calcic oxalate and ammonic-magnesic phosphate, by M. E. Monier. During the meeting, the Academy elected M. P. Gervais as successor to the late M. Coste, of the section of Anatomy and Zoology.

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