THURSDAY, SEPTEMBER 27, 1888.

THE FAUNA OF BRITISH INDIA.

The Fauna of British India, including Ceylon and Burma. "Mammalia." By W. T. Blanford, F.R.S. Part I. Published under the authority of the Secretary of State for India in Council. (London: Taylor and Francis, 1888.)

AMONG the various methods which may be adopted in the composition of zoological monographs, the two most prevalent are those in which either the natural group or the geographical region is taken as the basis. A particular section of the animal kingdom may be selected, and the structure, history, affinities, varieties, and distribution of its members worked out, or a particular region of the earth's surface may be taken, and the whole of its varied inhabitants described.

Monographs of groups and of fauna both have their value, and the success obtained in undertaking one or the other will depend much upon the special facilities of the investigator. From a strictly scientific point of view the former generally produce the best result. There is more cohesion, or naturalness, so to speak, in such a group, whether genus, family, or order; and anyone seriously endeavouring to trace the modifications of its members through all known forms, especially if the extinct can be united with the existing, has a better chance of getting a complete comprehension of the relations of all the parts of his subject than one who has to deal with the disjointed fragments of a large number of groups, brought by various circumstances together upon one part of the earth's surface—work, moreover, in many parts of which he must necessarily be largely dependent upon the labours of others.

On the other hand, for practical convenience, faunistic works are in greater demand than monographs on groups, especially if they treat of regions so important to the educated and civilized world as British India. We may even, in such a case, allow the weight of social and political rather than purely scientific boundaries in defining the range of the territory comprehended in the work. There is a very natural and laudable desire on the part of the large and continually increasing number of residents and travellers in our Indian Empire to obtain some definite knowledge of the varied and interesting forms of animal life by which they are surrounded, and it is gratifying to see that the Government of that great dependency has recognized its responsibility in this matter, and has given its authority to the preparation of a series of descriptive manuals on Indian zoology. The limits adopted for the fauna are those of the dependencies of India, with the addition of Ceylon, which, although British, is not under the Indian Government. Within the limits thus defined are comprised all India proper and the Himalayas, the Punjab, Sind, Baluchistan, all the Kashmir territories, with Gilgit, Ladák, &c., Nepal, Sikhim, Bhutan, and other Cis-Himalayan States, Assam, the countries between Assam and Burma, such as the Khási and Naga Hills and Manipur, the whole of Burma, with Karennee and Tenasserim, and the Mergui Archipelago, and, lastly, the Andaman and the Nicobar Islands. Afghanistan, Kashgaria, Tibet, Yunnan, Siam, and the Malay Peninsula south of Tenasserim are excluded. A few States, such as Nepal and Bhutan, at present not accessible to Europeans, are comprised, because it would be difficult to leave them out: scarcely an animal occurs in either not found also in British territories or in protected States such as Sikhim.

For the present it is proposed to restrict the publication to the Vertebrata, and to complete the work in seven volumes of about 500 pages each. One of these volumes will contain the Mammals, three will be required for the Birds, one for the Reptiles and Batrachians, and two for the Fishes. The authorship of the volumes on Fishes has been undertaken by Mr. F. Day, C.I.E.; the Reptilia and Batrachia will be described by Mr. G. A. Boulenger; whilst the Birds will, it is hoped, be taken in hand by Mr. E. W. Oates, author of the "Birds of British India." The editorship of the whole has been intrusted to Mr. W. T. Blanford, F.R.S., than whom few men could be found better qualified for such an undertaking. Long-continued employment in connection with the Geological Survey of India has made him familiar with the natural features of every part of the country; his qualifications as a field naturalist have been abundantly displayed in the published results of his scientific excursions to Persia and Abyssinia; and he has had recently, during several years' residence in London, ample opportunity of examining and comparing all that bears upon the subject, which is gathered together or recorded in our national collections and libraries at home.

Mr. Blanford has himself undertaken the volume describing the Mammals, and has now given us the first part as an instalment, consisting of 250 pages, and containing the orders Primates, Carnivora, and Insectivora. Notwithstanding the great advance that this work shows over that of Jerdon, published twenty-one years ago, especially in scientific method, critical discrimination of specific distinctions, and attention to the rules of nomenclature, in all of which it leaves nothing to be desired, it is still interesting to observe how much remains to be done, even in such a comparatively well-worn field as the Mammals of India, and how insufficient even our largest collections still are for perfecting such a work. For instance, the materials for a critical and exhaustive examination of the interesting genus of monkeys, *Semnopithecus*, are obviously wanting at present. Fourteen species of the genus are assigned by the author to British India, but doubts are expressed as to the real distinction of several of them, the characters of which are taken from an extremely limited number of examples, and it is stated that very little is known of their breeding habits and life-history in general. The variations, habits, and geographical distribution of the smaller *Felidæ* and *Viverridæ* offer an interesting field for future investigators, though Mr. Blanford has done much to clear away the confusion in which the synonymy of these groups had been involved by previous and less careful and conscientious workers. The account of the Insectivora has been derived largely from Mr. Dobson's excellent monograph of that order, the concluding still unpublished part of which, containing the *Soricidæ*, has been placed by the author at Mr. Blanford's disposal for the purpose.

The complete though condensed accounts of the habits of the animals described, whenever they are known on good authority, will make the work popular even with not strictly scientific readers; but all padding made up of ill-authenticated, fanciful, or exaggerated stories, or of personal narratives of sport and adventure, has been carefully excluded, as becomes the character of such a work as this is intended to be.

One of the most difficult questions that always arises in editing a work on natural history is that relating to the number and nature of the illustrations most suitable for its purpose. Figures are, without doubt, a great help to all classes of readers, and, other things being equal, the more numerous and better they are the more useful the book. But then comes in the question of cost, the bearings of which have carefully to be considered from a business point of view. A book that is intended to have a fairly extensive distribution must not be overweighted in this respect, or much of its utility will be lost. Mr. Blanford has evidently considered it best to sacrifice something of artistic effect and uniformity of character in his illustrations, for the sake of increasing their number and keeping the work within moderate compass as to price. With regard to the spirited little sketches of the external forms of animals, many of which are taken from the unpublished drawings of Colonel Tickell and Mr. Hodgson in the possession of the Zoological Society, the work of the Typographic Etching Company answers its purpose sufficiently well; but we cannot say the same of the figures of the skulls, which compare badly with woodcuts, of which a sufficient number (mostly, if not all, borrowed from other works) are introduced to make the contrast somewhat striking. These, however, are minor blemishes, which, we trust, are compensated by economy in production, and consequent advantage to the purchaser of the work; but the absence of scale to the figures, which is sometimes embarrassing, is an omission which might easily have been rectified.

The general form and typography of the work are all that can be desired, and we cordially welcome it as an instalment of what promises to be not only a most valuable aid to the knowledge of the natural history of one of the most important portions of our Empire, but also a standard contribution to zoological science in general.

W. H. F.

OUR BOOK SHELF.

Flora of the North-East of Ireland. By S. A. Stewart and the late T. H. Corry. Pp. 331. (Cambridge: Macmillan and Bowes, 1888).

LOCAL "Floras" have not been produced at the same rate in Ireland as in England, but Irish botanists are beginning to exercise more activity in this direction. It is true that there previously existed a catalogue of the plants of this region, together with localities of the rarer ones, in Dickie's "Flora of Ulster" (1864); and the twelfth district of Moore and More's "Contributions towards a Cybele Hibernica" (1866) is conterminous with the area of the book under consideration; but both of these works are incomplete, and imperfect in regard to what are termed "critical species."

The present book, we are informed in the preface, is an attempt to give a full and trustworthy account of the native vegetation of the counties of Down, Antrim, and Derry; an undertaking that was projected some years since by the late T. H. Corry, M.A., and the surviving editor. The lamentable and premature death of Mr. Corry by drowning, together with his friend and companion Mr. Dickson, in Lough Gill, on a botanizing excursion in 1883, will be remembered by most botanists. This sad event considerably retarded the appearance of the work, as Mr. Stewart's duties as Curator of the Belfast Museum left him little time for the task.

A brief history of botanical discovery, and the bibliography of what has been published, precede equally short paragraphs on the geography, geology, climate, &c., of the country. Then follows the enumeration, which includes 803 flowering plants and ferns, 293 mosses, and 73 liverworts. Babington's "Manual of British Botany," which contains 1524 vascular plants in the entire British flora, has been taken as the standard of the "Flora of the North-East of Ireland," though deviations in nomenclature have been made—in accordance with the rules of priority, Mr. Stewart explains.

The volume is a small and handy one, not overladen with localities, which is a distinct advantage over many similar works; but it has also certain defects, which, if pointed out, may possibly be remedied in a later edition. In the first place, there is no map of the country, a serious curtailment of its possible usefulness. Another defect, only the initial letter of the generic name is carried forward from page to page, though there is invariably ample space to repeat the name in full; therefore it is necessary to turn back to the beginning of the genus to ascertain what is intended. The same thing is noticeable in the index.

With regard to the purely literary part of the work, more particularly that relating to the priority and authorship of names, it would obviously have been better had the author adhered strictly to the last edition of Babington's "Manual" or the last edition of the "London Catalogue." for this part of the subject is just now in a transitional stage, and without a very complete botanical library it is impossible to do more than add to the existing confusion. We have no sympathy with those who adhere strictly to the "law of priority," because it entails endless changes of familiar names, and sacrifices convenience without any corresponding advantage. The fall of one genus often carries several others with it, and until the whole of the literature of binominal botany has been thoroughly examined there is no saying where the changes will stop. At the same time, if it is to be done, it should be done thoroughly, once for all.

Having turned up at random about half-a-dozen names concerning which there was some ambiguity, we found that the author was wrong in each instance. Thus, "*Nasturtium palustre* (Willd.), D C.," should be *N. terrestre*, B. Br.; "*Lepidium Smithii* (Linn.), Hooker," = *L. heterophyllum*, Benth.; "*Hypericum tetrapterum*, Fries," = *H. quadratum*, Stokes; "*Lotus pilosus*, Beeke (*L. major*, Sm)," = *L. uliginosus*, Schkuhr, and so on to the end. Whether the older names here cited are the oldest of all for the plants in question under the accepted genera is uncertain. Somebody some day may find names for some of these plants a week or two older, and then comes another change!

More interesting are some of the local names cited by Stewart, such as Tormenting Root (*Potentilla Tormentilla*), Mashcorns (*Potentilla Anserina*), Rose-noble (*Scrophularia nodosa*), and Well-ink (*Veronica Beccabunga*). Britten and Holland have all these names, or nearly the same. Thus, mascorns, and other variations, for the same plant in Scotland

W. B. H.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Electric Fishes.

WHILE I was fishing for cod the other day off Walmer, I took up in my hand a small whiting pout that was flopping about in the bottom of the boat, when I received what appeared to me a slight though distinct electric shock in the palm of my hand, which made me exclaim at once, "That fish has given me an electric shock." On asking the fisherman (seventy years of age) if he had known of such a thing occurring before, he said that he had "heard tell of it," and on inquiring further I found that he was referring to whiting pout and not to any other fish. He had never, however, noticed anything of the kind himself.

It will be interesting to know if any of your correspondents can confirm the observation. W. H. CORFIELD.

Savile Row, W., September 22.

Sonorous Sands.

THE communication of Mr. Cecil Carus-Wilson in NATURE of August 30 (p. 415), induces us to state that we are rapidly bringing to completion, and preparing for publication, an exhaustive study of "Sea, Lake, River, and Desert Sands" in their geological, physical, and chemical aspects. Our researches have extended over a period of six years, and are based on studies made in the field, in the laboratory, and with the microscope, and will be found to embrace many novel facts and original views. We have collected in person, by correspondence, and with the aid of the Life Saving Service of the United States, and of the Smithsonian Institution, several hundred specimens of sands and silts from localities in America, Europe, Africa, and Asia: these we have subjected to systematic examination and have tabulated the results.

The interesting phenomena of "musical sands," so called, have also been made special objects of our investigations, resulting in the discovery of many new localities, and of novel properties, as well as of the circumstances connected with the origin, production, and extinction of the sonorous qualities from which these sands receive their name. Furthermore, we have traced the history of musical sands through the literature of many centuries, and have brought together from widely scattered sources memoirs and notices of both scientific and popular interest. Throughout our work the bibliography of the subject has not been neglected, and we have availed ourselves of the photographic art for the purposes of illustration. We beg leave to make this preliminary announcement because our researches have been lengthened far beyond our expectations, and their publication (save in a few abstracts in the Proceedings of the American Association for the Advancement of Science) unavoidably delayed.

With regard to the occurrence of musical sand in Europe, the existence of which is unknown to Mr. Carus-Wilson, we may add that we have specimens from various localities, and the literature of the subject is accessible to everyone.

H. CARRINGTON BOLTON.
ALEXIS A. JULIEN.

London and New York, September 1.

YOUR correspondent in NATURE of the 30th ult. (p. 415), mentions a sea-beach in Dorsetshire as the only place in the Kingdom, besides the Island of Eigg, where "musical" sand is known to occur. This summer I found the sand in Lunan Bay (Forfarshire) to be distinctly sonorous. The sound occurred on moving the foot across the sand, or moving a walking-stick or the finger. The sound was little inferior to that in Eigg. The attention of a fisherman having been directed to the circumstance, he informed me they were quite aware of the occurrence, and that the sound was frequently much louder than on the day I was there; depending, I presume, on the state of the sand and of the atmosphere. He also mentioned that the sound occurs in the sand of Montrose Bay. I observed that the best result was got where the sand was moderately dry, and that little or no effect was produced with such a greater degree of moisture as gave a good result in Eigg. The form and composition of the sand-grains differ considerably in the two localities. It seems probable that sand of this character occurs in more localities than hitherto supposed. K.

Torquay, September 8.

THE LATE ARTHUR BUCHHEIM.

I HAVE been requested, and feel it a melancholy satisfaction, to notice in the columns of NATURE the premature decease on the 9th inst., at the age of twenty-nine, of Mr. Arthur Buchheim, for many years Mathematical Master at the Manchester Grammar School.

He was educated at the City of London School, whence he proceeded to Oxford, and gained an open Scholarship at New College there. He was a favourite pupil of the late Henry Smith, my distinguished predecessor in the Savilian Professorship of Geometry, who always spoke of him as the most promising young mathematician that had appeared in the University of Oxford for a long series of years. I am not able to speak of his earlier work as an original investigator, but know and value highly his contributions to the great subject which engaged the principal part of my own attention during the transition period between my residence in Baltimore and at Oxford, and to which I have given the name of Universal Algebra. He was a man of singular modesty and goodness of heart, which made him beloved by all who were brought into connection with him. Had his life been spared, I think we may safely say of him what Newton said of Horrocks, that "we should have known something" of what may now probably remain long unknown.

His life, it is to be feared, may have been shortened by his intense application to study, as after the arduous labour of the day he would sit up at night to study languages such as Sanskrit, Persian, Chinese, and Russian, almost any one of which was sufficient in itself to occupy his undivided attention.

After leaving Oxford he studied for some time under Prof. Klein at Leipzig. This episode in his life no doubt contributed to widening his intellectual horizon, but at the same time had the unfortunate effect of getting him out of the style of ordinary English University Examinations, in consequence of which he abstained, although strongly pressed by the authorities to do so, from offering himself as a candidate for a vacant Fellowship at the College of which he was a Scholar.

He comes of an intellectual stock, his father being the well-known Prof. C. A. Buchheim, of King's College, London.

Up to the last, after he had been obliged from ill health to resign his appointment at Manchester, he continued in harness, and made a communication to the London Mathematical Society at the monthly meeting in May or June last.

I have been furnished with a list of his published papers, fourteen in number, up to the year 1885 (exclusive), of which four appeared in the Proceedings of the London Mathematical Society, eight in the Cambridge *Messenger of Mathematics*, one in the *American Journal of Mathematics*, and one (November 1884) in the *Philosophical Magazine*. This last was entitled, "On Prof. Sylvester's Third Law of Motion," with which, I regret to say, I was previously unacquainted.

"The three laws of motion" of which it forms one were formulated by me in one of the Johns Hopkins Circulars, and it is a proof of the keenness of his research, that the subject of this notice (probably the only mathematician in Europe) should have made himself so well acquainted with them as to be able to write an independent paper on the subject. They have no direct connection (except in a Hegelian[1] sense) with mechanical principles, but are

[1] By which I mean that sense according to which motion in space is to be regarded as only a particular (visualized) instance of change *in actu*.

three cardinal principles in my Theory of Universal Algebra, between which and Newton's Three Laws of Motion I considered that I had succeeded in establishing a one-to-one correspondence. J. J. SYLVESTER.

Athenæum Club, September 22.

THE BRITISH ASSOCIATION.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY LIEUTENANT-GENERAL PITT-RIVERS, D.C.L., F.R.S., F.G.S., F.S.A., PRESIDENT OF THE SECTION.

I.

HAVING been much occupied up to within the last week in my own special branch of anthropology, and in bringing out the second volume of my excavations in Dorsetshire, which I wished to have ready for those who are interested in the subject on the occasion of this meeting, I regret that I have been unable to prepare an address upon a general subject as I could have wished to do, and am compelled to limit my remarks to matters on which I have been recently engaged. Also, I wish to make a few observations on the means to be taken to promulgate anthropological knowledge and render it available for the education of the masses.

Taking the last-mentioned subject first, I will commence with anthropological museums, to which I have given attention for many years. In my judgment, an institution that is dedicated to the Muses should be something more than a store, it should have some backbone in it. It should be in itself a means of conveying knowledge, and not a mere repository of objects from which knowledge can be culled by those who know where to look for it. A national museum, created and maintained at the public expense, should be available for public instruction, and not solely a place of reference for *savants*.

I do not deny the necessity that exists for museum stores for the use of students, but I maintain that, side by side with such stores, there should in these days exist museums instructively arranged for the benefit of those who have no time to study, and for whom the practical results of anthropological and other, scientific investigations are quite as important as for *savants*.

The one great feature which it is desirable to emphasize in connection with the exhibition of archæological and ethnological specimens is evolution. To impress upon the mind the continuity and historical sequence of the arts of life, is, without doubt, one of the most important lessons to be inculcated. It is only of late years that the development of social institutions has at all entered into the design of educational histories. And the arts of life, so far as I am aware, have never formed part of any educational series. Yet as a study of evolution they are the most important of all, because in them the connecting links between the various phases of development can be better displayed.

The relative value of any subject for this purpose is not in proportion to the interest which attaches to the subject in the abstract. Laws, customs, and institutions may perhaps be regarded as of greater importance than the arts of life, but for anthropological purposes they are of less value, because in them, previously to the introduction of writing, the different phases of development, as soon as they are superseded by new ideas, are entirely lost and cannot be reproduced except in imagination. Whereas in the arts of life, in which ideas are embodied in material forms, the connecting links are in many cases preserved, and can be replaced in their proper sequence by means of antiquities.

For this reason the study of the arts of life ought always to precede the study of social evolution, in order that the student may learn to make allowance for missing links, and to avoid sophisms and the supposition of laws and tendencies which have no existence in reality.

To ascertain the true causes for all the phenomena of human life is the main object of anthropological research, and it is obvious that this is better done in those branches in which the continuity is best preserved.

In the study of natural history, existing animals are regarded as present phases in the development of species, and their value to the biological student depends, not so much on their being of the highest organism, as on the palæontological sequence by which their history is capable of being established. In the same way existing laws, institutions, and arts, wherever they are found in their respective stages of perfection, are to be regarded simply as existing strata in the development of human life, and their value from an anthropological point of view depends on the facilities they afford for studying their history.

If I am right in this view of the matter, it is evident that the arts of life are of paramount importance, because they admit of being arranged in cases by means of antiquities in the order in which they actually occurred, and by that means they serve to illustrate the development of other branches which cannot be so arranged, and the continuity of which is therefore not open to visual demonstration for the benefit of the unlearned.

It is now considerably over thirty years since I first began to pay attention to this subject. Having been employed in experimenting with new inventions in fire-arms, submitted to H.M. Government in 1852–53, I drew up in 1858 a paper which was published in the *United Service Journal*, showing the continuity observable in the various ideas submitted for adoption in the army at that time.

Later, in 1867–68–69, I published three papers, which, in order to adapt them to the institution at which they were read, I called "Lectures on Primitive Warfare," but which, in reality, were treatises on the development of primitive weapons, in which it was shown how the earliest weapons of savages arose from the selection of natural forms of sticks and stones, and were developed gradually into the forms in which they are now used. I also traced the development of the forms of implements of the Bronze Age and their transition into those of the Iron Age. These papers were followed by others on the same subject read at the Royal Institution and elsewhere, relating to the development of special branches, such as early modes of navigation, forms of ornament, primitive locks and keys, the distribution of the bow, and its development into what I termed the *composite* bow in Asia and America, and other subjects.

Meanwhile I had formed a museum, in which the objects to which the papers related were arranged in developmental order. This was exhibited by the Science and Art Department at Bethnal Green from 1874 to 1878, and at South Kensington from that date to 1885; and a catalogue *raisonné* was published by the Department, which went through two editions. After that, wishing to find a permanent home for it, where it would increase and multiply, I presented it to the University of Oxford, the University having granted £10,000 to build a museum to contain it. It is there known as the "Pitt-Rivers Collection," and is arranged in the same order as at South Kensington. Prof. Moseley has devoted much attention to the removal and re-arrangement of it up to the time of his recent, but I trust only temporary, illness, which has been so great a loss to the University, and which has been felt by no one connected with it more than by myself, for whilst his great experience as a traveller and anthropologist enabled him to improve and add to it, he has at the same time always shown every disposition to do justice to the original collection. Since Prof. Moseley's illness it has been in the charge of Mr. H. Balfour, who, I am sure, will follow in the steps of his predecessor and former chief, and will do his best to enlarge and improve it. He has already added a new series in relation to the ornamentation of arrow stems, which has been published by the Anthropological Institute. It appears, however, desirable that the same system should be established in other places, and with that view I have for some time past been collecting the materials for a new museum, which, if I live long enough to complete it, I shall probably plant elsewhere.

Before presenting the collection to Oxford I had offered it to the Government, in the hope that it might form the nucleus of a large educational museum arranged upon the system of development which I had adopted. A very competent Committee was appointed to consider the offer, which recommended that it should be accepted, but the Government declined to do so; one of the reasons assigned being that some of the authorities of the British Museum thought it undesirable that two ethnographical museums should exist in London at the same time; this, however, entirely waives the question of the totally different objects that the two museums (at least that part of them which relates to ethnographical specimens) are intended to serve.

The British Museum, with its enormous treasures of art, is itself only in a molluscous and invertebrate condition of development. For the education of the masses it is of no use whatever.

It produces nothing but confusion in the minds of those who wander through its long galleries with but little knowledge of the periods to which the objects contained in them relate. The necessity of storing all that can be obtained, and all that is presented to them in the way of specimens, precludes the possibility of a scientific or an educational arrangement.

By the published returns of the Museum it appears that there has been a gradual falling off in the number of visitors since 1882, when the number was 767,873, to 1887, when it had declined to 501,256. This may be partly owing to the increased claims of bands and switchbacks upon public attention, but it cannot be owing to the removal of the Natural History Museum to South Kensington, as has been suggested, because the space formerly occupied by those collections at Bloomsbury has been since filled with objects of greater general interest, and the galleries have been considerably enlarged.

The Science and Art Department at South Kensington has done much for higher education, but for the education of the masses it is of no more use than the British Museum, for the same reason, that its collections are not arranged in sequence, and its galleries are not properly adapted for such an arrangement. Besides these establishments, annual exhibitions on a prodigious scale have been held in London for many years, at an enormous cost, but at the present time not the slightest trace of these remain, and I am not aware of any permanent good that has resulted from them. If one-tenth of the cost of these temporary exhibitions had been devoted to permanent collections, we should by this time have the finest industrial museum in the world. Throughout the whole series of these annual temporary collections, only one, viz. the American department of the Fisheries Exhibition, was arranged upon scientific principles, and that was arranged upon the plan adopted by the National Museum at Washington. It appears probable from the experience of the present year that these annual exhibitions are on the decline. Large iron buildings have been erected in different places, some of which would meet all the requirements of a permanent museum. The Olympia occupies 3½ acres, the Italian Exhibition as much as 7 acres. There can be little doubt, I think, that the long avenues of potted meats and other articles of commonplace merchandise, which now constitute the chief part of the objects exhibited in these places, must before long cease to be attractive, and must be replaced by something else, and in view of such a change I venture to put in a plea for a National Anthropological Museum upon a large scale, using the term in its broadest sense, arranged stratigraphically in concentric rings. It is a large proposal, no doubt, but one which, considering the number of years I have devoted to the subject, I hope I shall not be thought presumptuous in submitting for the consideration of the Anthropological Section of this Association.

The Palæolithic period being the earliest, would occupy the central ring, and having fewer varieties of form would require the smallest space. Next to it the Neolithic and Bronze Ages would be arranged in two concentric rings, and would contain, besides the relics of those periods, models of prehistoric monuments, bone caves, and other places interesting on account of the prehistoric finds that have been made in them. After that, in expanding order, would come Egyptian, Greek, Assyrian, and Roman antiquities, to be followed by objects of the Anglo-Saxon, Frankish, and Merovingian periods; these again in developmental outward expansion would be surrounded by mediæval antiquities, and the outer rings of all might then be devoted to showing the evolution of such modern arts as could be placed in continuity with those of antiquity.

In order that the best objects might be selected to represent the different periods and keep up the succession of forms which would constitute the chief object of the Museum, I would confine the exhibition chiefly to casts, reproductions, and models, the latter being, in my opinion, a means of representing primitive arts, which has not yet been sufficiently made use of, but which in my own small local museum at Farnham, Dorsetshire, I have employed to a considerable extent, having as many as twenty-three models, similar to those now exhibited, of places in which things have been found within an area of two miles.

The several sections and rings would be superintended by directors and assistants, whose function it would be to obtain reproductions and models of the objects best adapted to display the continuity of their several arts and periods; and the arts selected for representation should be those in which this continuity could be most persistently adhered to. Amongst these the following might be named: pottery, architecture, house furniture, modes of navigation, tools, weapons, weaving apparatus, painting, sculpture, modes of land transport and horse furniture, ornamentation, personal ornament, hunting and fishing apparatus, machinery, fortification, modes of burial, agriculture, ancient monuments, domestication of animals, toys, means of heating and of providing light, the use of food, narcotics, and so forth.

Miscellaneous collections calculated to confuse the several series, and having no bearing on development, should be avoided, but physical anthropology, relating to man as an animal, might find its place in the several sections.

I have purposely avoided in my brief sketch of this scheme giving unnecessary details. Any cut-and-dried plan would have to be greatly altered, according to the possibilities of the case, when the time for action arrived. My object is to ventilate the general idea of a large Anthropological Rotunda, which I have always thought would be the final outcome of the activity which has shown itself in this branch of science during the last few years, and which I have reason to believe is destined to come into being before long. In such an institution the position of each phase of art development shows itself at once by its distance from the centre of the space, and the collateral branches would be arranged to merge into each other according to their geographical positions.

The advantages of such an institution would be appreciated, not by anthropologists and archæologists only. It would adapt itself more especially to the limited time for study at the disposal of the working classes, for whose education it is unnecessary to say that at the present time we are all most deeply concerned. Although it is customary to speak of working men as uneducated, education is a relative term, and it is well to remember that in all that relates to the material arts they have, in the way of technical skill and handicraft, a better groundwork for appreciating what is put before them than the upper classes. That they are able to educate themselves by means of a well-arranged Museum, my own experience, even with the imperfect arrangements that have been at my command, enables me to testify. Anything which tends to impress the mind with the slow growth and stability of human institutions and industries, and their dependence upon antiquity, must, I think, contribute to check revolutionary ideas, and the tendency which now exists, and which is encouraged by some who should know better, to break drastically with the past, and must help to inculcate Conservative principles, which are urgently needed at the present time, if the civilization that we enjoy is to be maintained and to be permitted to develop itself.

The next subject to which I would draw your attention is the present working of the Act for the Preservation of Ancient Monuments, with the carrying out of which I have been intrusted during the last five years.

It is unnecessary to speak of the measures that have been taken in other countries which have preceded us in the work of protecting ancient monuments. Their system of land tenure and division of property is different from ours, and the same measures are not equally applicable.

In 1882 a Bill was passed through Parliament known as the Ancient Monuments Act, to enable those who desired to do so, to place the ancient monuments belonging to them under the protection of the Government, and to make it illegal for future owners or others to destroy them: also to enable local magistrates to punish summarily, with a fine of £5 or imprisonment for one month, offences committed under the Act. No power is taken to compel any owner to place his monument under the Act, but provision is made for a small annual expenditure in order to preserve the monuments offered voluntarily by their owners. A schedule of certain monuments was attached to the Act, without the consent of the owners, merely to indicate the monuments to which the Act applied, but these, like any others, had to be voluntarily offered before the Government could accept them. Any other monuments not in the schedule could be accepted, but only after the offer of them had been laid forty days before Parliament, in order, I presume, that the country might not become charged with the preservation of monuments that were unworthy of protection.

In November 1882, I was asked by Lord Stalbridge, in a complimentary letter, written by desire of the Prime Minister, to undertake the office of Inspector, intimating at the same time that my position as landowner would place me in a favourable position for dealing with other landowners to whom the monuments belonged, and I accepted the post, hoping to render a public service, not, perhaps, sufficiently considering the difficulties that I should have to encounter, and the amount of time that would have to be devoted to it.

A permissive Act naturally implies that there is some one in the country who desires to make use of it; whereas, as a fact, no owner has voluntarily offered any monument to be put under the Act, except one to whom I shall refer again presently: all have had to be sought out and asked to accept the Act, and of the owners of scheduled monuments the larger number have refused.

Sir John Lubbock was chiefly instrumental in passing the Bill through Parliament, although in the condition in which it actually passed it was not his Bill. He had proposed to make the Act compulsory in the case of some of the more important monuments, but the proposal had been overruled on the ground of its being an improper interference with private ownership.

Being a member of the Liberty and Property Defence League, I have lately received a list of fifty-five measures which have been brought before Parliament in the session of 1888, which that body have thought it desirable to oppose on account of their interference with private property, nearly every one of which would have dealt more hardly with the owners of property than the Ancient Monuments Act would have done had it been made compulsory. But all these measures have been proposed by members of Parliament with the view of catching the votes of particular constituencies, whereas the ancient monuments have no votes to give and very few people to vote for them. Sir John Lubbock, finding that the Act in its approved stage was purely permissive, and not believing, as he told me at the time, that anyone would voluntarily make use of it, naturally being unwilling to put his own property at a disadvantage, by being the only person to come under it, at first refused to include his own monuments, and it was only after I had obtained others, and success appeared probable, that he consented to put Silbury Hill under the Act.

Finding myself involved in the matter, I have done what I could to work it out, and with some success.

(*To be continued.*)

THE INTERNATIONAL GEOLOGICAL CONGRESS.

THE fourth session of the International Geological Congress began on Monday evening, September 17, in the theatre of the University of London, Burlington Gardens; meetings were held throughout the week, and the session was formally closed on Saturday, September 22. In another form and in different places the Congress may be regarded as continuing throughout this week, for five excursions have been organized to various parts of England; those to North Wales and the Isle of Wight are largely attended, whilst smaller numbers have gone to East Anglia, to East Yorkshire, and to West Yorkshire.

At the opening meeting on Monday evening the Council was chosen as follows:- Hon. President: T. H. Huxley. President: J. Prestwich. Past Presidents: G. Capellini, E. Beyrich. Vice-Presidents: Germany, K. von Zittel; Australia, *F. Liversidge; Austria, M. Neumayr; Belgium, G. Dewalque; Canada, T. Sterry Hunt; Denmark, *M. Johnstrup; Spain, J. Vilanova-y-Piera; United States, P. Frazer; France, A. de Lapparent; Great Britain, W. T. Blanford, A. Geikie, *T. McK. Hughes; Hungary, J. von Szabó; India, *H. B. Medlicott; Italy, F. Giordano; Norway, H. Reusch; Holland, K. Martin; Portugal, J. F. N. Delgado; Roumania, G. Stefanescu; Russia, A. Inostranzeff; Sweden, O. Torell; Switzerland, E. Renevier. General Secretaries: J. W. Hulke, W. Topley. Secretaries: C. Barrois, C. Fornasini, C. Le Neve Foster, C. Gottsche, A. Renard, G. H. Williams. Treasurer: F. W. Rudler. Other Members of the Council: T. G. Bonney, A. Briart, E. Cohen, *H. Credner, *E. Dupont, J. Evans, W. H. Flower, A. Gaudry, J. Gosselet, M. von Hantken, W. Hauchecorne, A. Heim, *J. Hooker, A. Issel, J. W. Judd, *R. Lepsius, C. Lory, *A. Michel-Lévy, T. Macfarlane, O. C. Marsh, E. von Mojsisovics, J. S. Newberry, S. Nikitin, *R. Owen, A. Pilar, F. von Richthofen, T. Schmidt, D. Stur, T. Tschernicheff, E. Van den Broeck, C. D. Walcott. (Those marked * were not present at the meeting.)

The President then delivered his address in French. An English translation of this has already appeared in NATURE. The meetings commenced each morning at 10.30, and lasted till about 1 o'clock. Meetings of the Council were held each morning at 9.30. The *procès-verbal* of each meeting both of Council and Congress was printed, and was placed in the hands of members at the opening of the succeeding meeting. At various times meetings of the International Commissions on Nomenclature, and the Geological Map of Europe, and of various Committees appointed by the Council, were also held.

In the afternoons there were visits to the British Museum, in Bloomsbury, and to the Natural History Museum, South Kensington; also to Kew, Windsor and Eton, Erith and Crayford. In the evenings there were three receptions: on Monday, by the President of the Congress, in the library of the University, fitted up as a temporary Geological Museum; on Wednesday, by the Director-General of the Geological Survey, in the Museum of Geology, Jermyn Street; on Friday, by the President of the Geological Society, in the rooms of that Society.

Three invitations for the fifth meeting of the Congress in 1891 were received from America—from Philadelphia, New York, and Washington. Philadelphia was chosen. A Committee of American geologists was appointed to take such steps as it thought necessary to make the arrangements for this meeting. The Committee consisted of Messrs. J. Hall, Dana, Newberry, Frazer, Gilbert, Hunt, Marsh, and Walcott.

The general opinion is that the Congress was a complete success. So far as members go, this is evidently the case, as it was more largely attended than any previous meeting, both by home and by foreign geologists. As regards the number of members inscribed from the country in which the Congress meets, it is not easy to make comparisons, because many join as members who have no claim to be considered geologists. No doubt this was more largely the case in London than at any previous meeting. But the numbers of foreign visitors may fairly be compared, and may be taken as affording a sufficient gauge of the relative importance of each meeting. These stand as follows: Paris (1878), 110; Bologna (1881), 75; Berlin (1885), 92; London (1888), 142.

The success of such a gathering may, however, be reckoned on other lines, and here opinions on the subject may differ. Those who hold that the first duty of such a Congress is to formulate rules and to fix nomenclature may well feel some disappointment; for although excellent discussions took place, and the general feeling was often evident, no formal vote on any such subject was taken. It was generally felt that votes from such mixed assemblages have no value. They can only carry weight when taken on some fixed principle, not dependent upon the accidents of place and nationalities which vary from time to time as the Congress meets in different countries. A Committee was formed to consider this matter. To its report, and to the general results of the Congress, we shall refer again next week. But upon one point there can be no difference of opinion: that is, the immense advantage resulting from the meeting together of men from different nations, engaged in similar pursuits, who can personally discuss subjects upon which they are at work. The friendships thus formed bear fruit long after the dicussions and votes of the formal meetings are forgotten.

ON CRYSTALLINE SCHISTS.[1]

I.

§ 1. AS a preliminary to the study of the schistose or stratiform crystalline rocks, it is desirable to consider the wider question of the origin of crystalline rocks in general, which are often named *Primary* or *Primitive Rocks* to distinguish them from those derived therefrom by mechanical or chemical disintegration. The designation of "crystalline rocks" is defective, inasmuch as we find, associated with masses having a right to this title, and geologically confounded with them, other rocks, such as serpentine, obsidian, perlite, and others, which are not crystalline in character, but colloidal, or, to use the designation of Breithaupt, porodic. The primary rocks, then, including both crystalline and porodic masses, may be divided geologically into three categories:—

(1) Masses more or less distinctly stratiform, including the fundamental granite, gneisses, micaceous and hornblendic schists, and all others formed, according to the views of the Wernerian school, by slow deposition in an aqueous liquid at the earth's surface. These we call INDIGENOUS ROCKS. (2) Masses which have strong mineralogic resemblances with the last, but appear to have been formed by slow deposition among pre-existing rocks, in which they occur as veins or secondary masses, and which we have consequently designated ENDOGENOUS ROCKS. (3) Masses which have resemblances, both mineralogic and geognostic, with the endogenous rocks, but are distinguished therefrom by the fact that they appear to have attained their present position not by slow deposition, but as the result of displacements which took place while they were in a more or less liquid or plastic state. These masses, which we designate EXOTIC ROCKS, are, as we shall endeavour to show, to be regarded (whatever their geological age) either as more or less modified portions of the original plutonic material of the globe, or as displaced portions of indigenous or endogenous rocks, and thus in either case belong to the primary class.

§ 2. Without taking into account those who, like Lehmann in the last century, maintained that the indigenous crystalline masses, which he called primitive rocks, were created as we now see them, we may say that the geologists of our own time are divided into two classes: those who admit for the indigenous rocks (1) an igneous or plutonic origin, (2) an aqueous or neptunian origin. Among the plutonists properly so called there are, moreover, two schools, one of which regards the foliated structure which characterizes the crystalline schists as due to the lamination of an igneous mass exposed to strong pressure during its extrusion through the already solidified terrestrial crust. For this school, in fact, the crystalline schists, not less than the granites, the trachytes, and the basalts, are eruptive rocks. This manner of explaining the origin of the crystalline schists, advanced by Poulett Scrope in 1825, and since frequently resuscitated, we have named the *exoplutonic* or volcanic hypothesis. For the other plutonist school, these same crystalline schists are the products of the consolidation, beneath a crust already formed by superficial cooling, of the igneous matter of the globe; the schistose structure being the result either of currents established in the still liquid and heterogenous material, or of a segregation therein during crystallization. To the views of this second plutonist school we have given the name of the *endoplutonic* hypothesis.

§ 3. The neptunists are also divided into several schools. Werner and his disciples believed that the crystalline rocks, both granitic and schistose, had been successively deposited from a universal ocean, which they imagined to have been a chaotic liquid holding in solution the elements of all the primitive rocks. We have called this derivation by slow crystallization from a primordial liquid chaos, the *chaotic* hypothesis. In this purely neptunian hypothesis, the action of a heated interior of the earth did not enter, but certain plutonists, admitting this notion, have imagined a *thermochaotic* hypothesis. This was advanced by Poulett Scrope, in 1825, as the complement of his exoplutonic hypothesis, and subsequently sustained by De la Beche and Daubrée.

Another neptunist school, which also held plutonic views, was that of Hutton, who supposed that the crystalline rocks now known to us have been formed by the consolidation and crystallization, through the agency of internal heat, of sediments arranged by water at the bottom of the seas, these sediments being the detritus either of endoplutonic or of exoplutonic rocks. The defect of this explanation, which we call the *metamorphic* hypothesis, is that it does not take into account the chemical changes suffered by most silicated mineral species during the process of disintegration of the crystalline rocks and their conversion into sands and clays. The production of species such as the feldspars, the micas, hornblende, &c., as the result of a recrystallization of sediments which do not contain the elements of these minerals, demands the additional supposition of chemical changes brought about either by substitution or by simple addition. In this manner, attempts have been made to explain supposed transformations, often very surprising, among which may be noted, not only the conversion of siliceous and argillaceous sediments into feldspathic and hornblendic rocks, but that of limestones into gneiss and other feldspathic and siliceous rocks, and also the conversion of these, as well as of diabases and diorites, into serpentine, or into crystalline limestone. This view, which we have called the *metasomatic* hypothesis, is, in the minds of many geologists, confounded with the metamorphic hypothesis of Hutton, of which it is, to a certain extent, the indispensable complement.

§ 4. Of all these hypotheses, that of Werner, which considered the primæval chaos as a watery liquid holding in solution the materials necessary for the formation of all the crystalline rocks, appears to us the one nearest the truth. It is certain, however, that in the present state of our chemical knowledge we cannot admit the simultaneous existence of all these materials in solution, even at the elevated temperature supposed by the thermochaotic hypothesis. We have, however, endeavoured to reconcile with known facts the view that a great part of all the primary rocks, including both the granites and the crystalline schists, have at one time been in the state of aqueous solution, through the action of processes which have operated without cessation from the Primary period. This explanation, which we have elsewhere set forth in detail, after a critical examination of the other hypotheses already mentioned, we have named the *crenitic* hypothesis, from the Greek κρήνη, fountain or spring.

Starting from the conception of a liquid globe of igneous origin, the solidification of which commenced at the centre, we find in its exterior portion—the last to solidify—the source of all the known terrestrial rocks; in other words, the veritable mineral protoplasm. This material we suppose to have been, from the time of its superficial cooling, exposed to the action of water and the atmospheric gases, while it was at the same time heated from below by the internal warmth, and penetrated to a greater or less depth by watery solutions. These, under the influence of the existing thermal differences, must have established a circulation between the surface and the deeper portions of the protoplasmic mass, which, as the result of crystallization and cooling, had already become porous. From the abundant outflow of thermal waters thus produced is derived the name "crenitic," given alike to the mineral deposits formed by them and to the present hypothesis. The action of these waters, removing from the protoplasmic material silica, alumina, and potash, and bringing to it at the same time lime, magnesia, and soda, must have necessarily altered by degrees the composition of this porous mass, heated from below, penetrated by aqueous solutions, and rendered more or less plastic in parts. In the changing mass, moreover, took place processes of crystallization, followed by partial separations determined by differences in specific gravity between the species thus formed. In this way were produced various types of plutonic rocks, which may justly be called Primary, since they are more or less modified portions of the original protoplasmic material.

§ 5. The dissolving action of the circulating waters continued without interruption from a very remote period in the history of the globe, and, extending eventually to depths equal to very many kilometres, while giving rise to the immense thickness of crenitic rocks which cover the surface of the protoplasmic mass, must necessarily have effected a great diminution therein. This decrease of volume beneath the crenitic covering must have resulted in movements giving rise to the more or less marked corrugations everywhere met with in the earlier layers of the crenitic envelope—movements which have continued, though with decreasing force, through all geological periods. Moreover, the accumulated weight, alike of crenitic deposits and of mechanical sediments, would bring about at length the displacement, in a plastic state, of portions of the primitive mass, as well

[1] Translated by the author from his essay on "Les Schistes Cristallins," presented to, and published in French by, the International Geological Congress in London, 1888. The footnote to § 5 has been added in translating.

as of parts of the crenitic layers themselves, in the form of eruptive rocks, forming not only *plutonic* masses, but those which we have designated as *pseudoplutonic*—that is to say, masses of crenitic origin which present the geognostic characters of plutonic rocks. Such are apparently the trachytes and the truly eruptive granites. Eruptions of these two classes of rocks seem to have been rare in the more ancient periods, but in later times they have played an important part in the transfer of mineral matters from the depths to the surface of the globe, while at the same time the crenitic activity has progressively decreased. Without questioning the effect of the slow contraction through the secular cooling of the heated anhydrous and solid nucleus of the globe, we believe that the diminution of volume of its more superficial and hydrated portions by the crenitic process, as well as by plutonic eruptions, has played a very important part in geological dynamics.[1]

§ 6. According to the hypothesis just set forth, it follows that the production alike of the crenitic and the plutonic rocks, as the result of the transformations of a primitive material presumed to be of igneous origin, has been subjected to constant, regular, and definite laws. It shows, in fact, a mineralogical evolution which has determined the order, the composition, and the succession of the crenitic masses of the terrestrial crust, as well as the composition of the plutonic masses of the various geological periods. In the study of the successive groups of crenitic rocks we must take into account the intervention in the crenitic process alike of the soluble and the insoluble products of the aërial decomposition both of more ancient crenitic rocks and of plutonic masses, as well as the effects, both direct and indirect, of the products of organized beings. It results from the influence of all these secondary agencies which have intervened in the course of the crenitic process, that the fundamental granite, as the most ancient crenitic rock, presents characters of uniformity and of universality which do not reappear in the less ancient crenitic terranes. These, in fact, already begin to show indications of a passage to the new order of things, and were thus, in the language of the Wernerian school, called Transition rocks.

As a farther result of this mineralogical evolution in the history of the crenitic rocks, we find that certain aluminiferous silicates rarely met with at a given period, at length become more abundant and finally predominate. For this reason it follows that in the mineral kingdom, as in the organic kingdoms, generalizations which have for their object chronological classifications, should be founded upon the character of a group taken in its integrity, and not upon the characters of exceptional species. For the rest, it is to be remarked that non-aluminiferous species, such as the protoxyd-silicates, quartz, carbonate of lime, and oxides of iron are found, with small variations, in the crenitic masses, whether indigenous or endogenous, alike of earlier and of later periods.

It is evident that the operations of solution and of aqueous deposition, as well as those of decomposition and sub-aërial decay, went on in the Primary and Transition periods under geographical conditions which did not differ greatly from those of the Secondary and Tertiary periods. The marks of erosion, of contemporaneous movements, and of deposition in discordant stratification are met with at different horizons in the indigenous terranes of the Primary as well as in those of the Secondary ages; offering in both cases local and accidental interruptions of the normal order of mineralogical development.

§ 7. The various granitic, quartzose, and calcareous veinstones, including metalliferous lodes, not less than the veins and geodes of zeolitic minerals, are examples of endogenous masses formed by the crenitic process. The production of zeolites and of other silicates by the action of thermal waters, and the formation of zeolitic species in the deep-sea ooze, are examples of the same crenitic action continued to our own time. As is shown by the studies of the action of our modern thermal springs, the surrounding solid matters co-operate with those in solution in the production of new mineral species. We must not overlook the part which is often played by infiltrating waters in producing local transformations in sediments, thereby giving rise to the production of crystalline species in the midst of detrital rocks. Pressure alone appears in certain cases to produce similar results, all of which cases are often insisted upon in support of the application of the metamorphic and metasomatic hypotheses to the origin of the primary rocks.

[1] Besides the removal of all the silica and alumina found in the crenitic rocks must be added the diminution of porosity in the protoplasmic mass and the probable formation of more condensed species than those originally contained therein.

The granitic veins, composed essentially of orthoclase and quartz, which are found not only among gneisses and mica-schists, but among basic plutonic rocks alike of Palæozoic and of Mesozoic age,[1] help us to understand the conditions which in times of greater crenitic activity gave rise to the production of the gneisses and the fundamental granite, both of which, according to our hypothesis, are essentially neptunian and crenitic in their origin. These same indigenous and endogenous crenitic rocks have furnished the greater part of the materials for the Secondary rocks. We have already indicated concisely, in § 4, our explanation of the origin of the true plutonic rocks, as the result of modifications which have taken place in the midst of the protoplasmic mass.

§ 8. We must not lose sight of the important part played by water in plutonic and volcanic phenomena, nor the fact that it can exist under strong pressure, at high temperatures, in combination with silicated rocks. From this union there result hydrated compounds, which are more fusible than the anhydrous rocks, and which are decomposed in the transformations that take place during the cooling, with diminution of pressure, which accompanies the eruption of these materials. The water thus set at liberty may be disengaged in the form of vapour, and with it certain other volatile matters which are met with in volcanic emanations. In other cases, however, under a high pressure still maintained, and at a temperature above the critical point of vaporization, the water may be liberated in the state of a dense polymeric vapour, holding in solution, in accordance with late observations, mineral matters, which, through cooling, are at length deposited either from the vapour itself or from the liquid resulting from its condensation, in the form of crystalline species. Superheated aqueous vapours may thus play a part closely akin to that of thermal waters, and one which must be regarded as itself belonging to the crenitic process.

The greater part of the questions here noticed have already been discussed in detail by the author in his volume entitled "Mineral Physiology and Physiography" (Boston, 1886), especially in the three chapters on the Origin, the Genetic History, and the Decay of Crystalline Rocks (pp. 68–277.)

II.

§ 9. In another chapter of the volume just mentioned the author treats of the History of Pre-Cambrian Rocks (pp. 402–25), and endeavours to resume in a few pages the results of his attempts through a period of forty years to arrive at a subdivision and a nomenclature of these terranes, which comprise both the Primary and the Transition systems of Werner. It must suffice for the present to indicate in a succinct manner the conclusions already reached.

I. Laurentian.—Under this name, proposed and adopted by the author in 1854, is included the ancient gneissic terrane met with in the Laurentide and the Adirondack Mountains, as well as in parts of the great Atlantic belt, and in the Rocky Mountains in central North America. To this same series the author has also referred the similar gneisses of Great Britain and of Scandinavia, as well as the ancient or central gneiss of the Alps. Beginning with our first studies in Canada in 1847, we indicated the existence in this ancient gneissic system of two subdivisions, the lower being described as consisting of granitoid gneiss (to be confounded with the fundamental granite), to which succeeds (in discordant stratification) another gneissic series, also granitoid, and frequently hornblendic, with which are intercalated quartzites and crystalline limestones, often with serpentine. These two subdivisions, which we may provisionally call Lower Laurentian and Upper Laurentian, have been described respectively as the Ottawa gneiss and the Grenville series. To prevent any misconception, it should be noted that the name of Upper Laurentian was for a time given by Logan to the terrane subsequently designated Labradorian, and afterwards Norian. It is therefore by a mistake that some have wished to retain as the designation of the upper division of the Laurentian terrane, the name of Middle Laurentian.

[1] We have elsewhere described the granitic veins inclosed in the diabases which themselves traverse the Ordovician limestones of Montreal in Canada. These veins, having sometimes a thickness of three decimetres, are coarsely crystalline and drusy, and, besides quartz and orthoclase, contain, as accidental minerals, sodalite, nephelite, cancrinite, amphibole, acmite, biotite, and magnetite. Veins composed essentially of pink orthoclase and quartz, often accompanied by zeolitic minerals, are found in similar conditions inclosed in the diabases which are contemporaneous with the Mesozoic sandstones of Hoboken, near New York. In both cases the endogenous and crenitic origin of the granitic veins does not admit of any doubt. See for details the author's "Mineral Physiology and Physiography" (Boston, 1886), pp. 121–37.

II. NORIAN.—The terrane thus designated by the author in 1870 is composed in great part of those straliform rocks having a base of anorthic feldspars, to which has been given the name of norite. This terrane, however, includes intercalated strata of gneiss, of quartzite, and of crystalline limestone, all of which resemble closely those of the Upper Laurentian. These norite rocks, which are sometimes called gabbros, are not to be confounded with the very distinct gabbros of the Huronian terrane, nor yet with certain plutonic rocks having with them certain mineralogical resemblances. The facies of the norites serves to distinguish them.

III. ARVONIAN.—This terrane is composed in great part of petrosiliceous rocks, which pass into quartziferous porphyries. With them, however, are intercalated certain hornblendic rocks, sericitic schists, quartzites, oxides of iron, and, more rarely, crystalline limestone. This terrane, indicated for the first time as distinct by Dr. Henry Hicks, in Wales, in 1878, and named by him, is regarded by Mr. Charles Hitchcock as constituting in North America the lower portion of the Huronian.

IV. HURONIAN.—This name was given by the author in 1855 to a terrane already recognized in North America, where it rests in discordant stratification either upon the Laurentian gneiss or upon the Arvonian petrosilex. It includes, besides quartzose, epidotic, chloritic, and calcareous schists, masses of serpentine, and of lherzolite, together with euphotides, which represent herein the norites of the Norian terrane, often confounded with them under the common name of gabbro. This Huronian terrane is greatly developed in the Alps, where it constitutes the series of the greenstones or *pietre verdi*.

V. MONTALBAN.—The studies of von Hauer in the Eastern Alps, published in 1868, and those of Gerlach on the Western Alps, published in the year following, agree in recognizing in these regions two gneissic terranes—namely, an older or ancient central gneiss, and a younger or recent gneiss; this last, which is petrographically very distinct from the old gneiss, being accompanied by micaceous and hornblendic schists. The studies of Gastaldi, published in 1871, and those of Neri, in 1874, while confirming the results of von Hauer and of Gerlach, furnish us with further details respecting these terranes and their lithological characters. It should here be remarked that all of these observers appear to agree in placing the horizon of the *pietre verdi* (Huronian) between the ancient gneiss (Laurentian) and the recent gneiss.

Before becoming acquainted with the first results of these observers, the writer, from his own studies in North America, was led to precisely similar conclusions, and in 1870 announced the existence of a series of younger gneisses very distinct from the old Laurentian gneisses, and accompanied by crystalline limestones and by micaceous and hornblendic schists. To this younger terrane, on account of its great development in the White Mountains of New Hampshire, he gave in 1871 the name of Montalban. This series appears to be identical with the younger gneiss of the Alps; the so-called Hercynian gneisses and mica-schists of Bavaria; the granulites, with dichroite-gneiss, mica-schists, and lherzolite of the Erzgebirge in Saxony; and similar rocks in the Scottish Highlands. The Montalban terrane in North America contains not only crystalline limestones, but beds of lherzolite and of serpentine, resembling in this respect the Huronian and the Laurentian. It is in this series, in North America at least, that are found the chief part of the veins or endogenous masses of granite, which carry beryl, tourmaline, and the ores of tin, of uranium, of tantalum, and of niobium.

Gastaldi, in an essay published in 1874, declares that "the *pietre verdi* properly so called" is found between "the ancient porphyroid and fundamental gneiss" and "the recent gneiss, which latter is finer-grained and more quartzose than the other." This younger gneiss he also describes as a gneissic mica-schist, and as a very micaceous gneiss passing into mica-schist, and often hornblendic; the two gneissic series being, according to him, easily distinguished the one from the other. To these two divisions, superior to the ancient gneiss—that is to say, the true *pietre verdi* and the younger gneiss—Gastaldi adds a third division, still more recent. This highest division contains considerable masses of strata called by him argillaceous schists, and otherwise lustrous, talcose, micaceous, and sericitic schists. Associated with these are also found quartzites, statuary and cipolin marbles, with dolomite, karstenite, and sometimes hornblendic rocks and serpentines, the presence of which in this division, and also among the recent gneisses, as well as "in the *pietre verdi* proper," was regarded by Gastaldi as justifying the name of "the *pietre verdi* zone," often given by him to the whole of this triple group of crystalline schists, which he recognized as younger than the central gneiss.[1]

VI. TACONIAN.—This third division, to which Gastaldi did not give a distinctive name, has, as is well known, a very interesting history in Italian geology. A terrane having the same horizon and the same mineralogical characters is found developed on a grand scale in North America, where it includes quartzites, often schistose, and sometimes flexible and elastic, with crystalline limestones yielding both statuary and cipolin marbles. It also contains deposits of magnetite and of hematite, as well as important masses of limonite, which is epigenic in some cases of pyrites, and in others of chalybite, two species which form, by themselves, large masses in the undecayed strata. This same terrane contains, moreover, roofing-slates, as well as lustrous unctuous schists, ordinarily holding damourite, sericite, or pyrophyllite, but including, occasionally, chlorite, steatite, and hornblendic rocks with serpentine and ophicalcite. We also find among these schists, which are met with at several horizons in the terrane, layers which are visibly feldspathic, with others of ill-defined character, which, however, are converted into kaolin by sub-aërial decay. These same schists furnish remarkable crystals of rutile, and also tourmaline, cyanite, staurolite, garnet, and pyroxene. This terrane, which, moreover, appears to be diamond-bearing, was described in 1859 by the late Oscar Lieber, under the name of the Itacolumitic group. Eaton already, in 1832, had placed the quartzites and the limestones, which form the lower members of this group, in the Primitive division. The argillites in the upper part of the group were regarded as the inferior member of his Transition division, and were, according to him, overlain unconformably by the fossiliferous graywacke (First Graywacke), made the upper member of this same Transition division. In 1842, Ebenezer Emmons included in what he then named the Taconic system the whole of this crystalline series, to which he added the graywacke; but in 1844 he separated this latter, in which he had meanwhile found a trilobitic fauna, and gave it the name of Upper Taconic; the inferior and crystalline portions being the Lower Taconic. Many years of study have shown me that this upper division is entirely independent of the Lower Taconic, with which the fossiliferous graywacke series is found in contact only in certain localities, while in many others it rests directly upon more ancient crystalline terranes. Seeing, morever, that the Lower Taconic is found without this graywacke, in a great number of localities, from the Gulf of St. Lawrence as far as Alabama to the south, and as far as Lake Superior to the west; and recognizing also the fact that the Upper Taconic is really a part of the Cambrian (as was avowed by Emmons himself in 1860), the author proposed in 1878 to limit the use of the term Taconic to the crystalline infra-Cambrian series which forms the Lower Taconic of Emmons and the Itacolumitic group of Lieber, and to call it the Taconian terrane.

The history of the various attempts made by the partisans of the metamorphic school to establish a more recent origin for the Taconian is a curious one. Various American geologists, adopting for the most part stratigraphical arguments, have successively referred it to the Cambrian, Ordovician, Silurian, Carboniferous, and Triassic horizons. It is, however, to be noted that these same geologists have also maintained the Palæozoic age of the greater part of the other crystalline terranes of North America, comprising the Montalban, the Huronian, the Arvonian, and a part of the Laurentian itself. The want of any conception of the principle of mineralogical development in the history of the crystalline schists, conjoined with the difficulties arising from the stratigraphical complications met with at many points along the eastern border of the great North American Palæozoic basin, has helped to confirm the belief of many American geologists in the hypotheses of the metamorphic and metasomatic schools.[2]

§ 10. The mineralogical resemblances which exist between the various crystalline terranes above mentioned are easily recognized.

[1] This question is discussed at length by the writer ("Mineral Physiology and Physiography," pp. 457–96) in a study of the geology of the Alps and the Apennines, and of the serpentines of Italy. See also his paper on "Gastaldi and Italian Geology," containing a hitherto unpublished letter from Gastaldi, in the *Geological Magazine* for December 1887.

[2] The reader who wishes to follow this question will find it discussed with much detail in the volume already cited "Mineral Physiology and Physiography" (pp. 517–686) under the title of "The Taconic Question in Geology." It is also treated, with some new facts, in the *American Naturalist* for February, March, and April, 1887, in an article entitled "The Taconic Question Restated."

The type of rocks characterized by orthoclase, appearing in the fundamental granite and the granitoid gneisses of the Laurentian, is again found in the quartziferous porphyries of the Arvonian, in the Montalban gneisses, and, though less distinctly, in the feldspathic rocks of the Taconian. The non-magnesian micas, rare in the fundamental granite and the Laurentian gneisses, appear abundantly in the Montalban gneisses and mica-schists, as well as in the lustrous schists which are found in the Huronian and the Taconian, and which predominate in the latter. It is further to be remarked that the simple silicates of alumina, such as andalusite, cyanite, fibrolite, and pyrophyllite, as yet unknown in the more ancient terranes, are abundant in the Montalban, and are also found in the Taconian. At the same time, crystalline limestones, oxides of iron, and calcareous and magnesian silicates, are met with in every terrane above the fundamental granite.

The chemical and mineralogical differences between these various terranes are more remarkable than the resemblances, a fact which, however, has not prevented some observers from confounding the younger with the older gneisses. Again, the resemblances between the Huronian and Taconian terranes led the late Prof. Kerr, in North Carolina to refer the latter terrane to the Huronian. Moreover, in the vicinity of the Lakes Superior and Huron, where we find alike Laurentian, Norian, Huronian, Montalban, and Taconian, the outcrops of this last were confounded with the Huronian by Murray and by other observers. In 1873, however, the author, distinguishing between the two, gave to the Taconian in this region the provisional name of the Animikie series. It was not until later that he recognized the fact that this series, which is here found in certain localities resting unconformably upon the Huronian, is no other than the Taconian. Emmons, on the contrary, who had long known the existence in this region of what he called the Lower Taconic, believed that the terrane to which the author gave, in 1855, the name of Huronian, was identical with this same Lower Taconic or Taconian. The differences between these two terranes in the basin of Lake Superior, first noted by Logan and later by the author, are clearly brought out by the recent studies of Rominger.

Upon all these different terranes, including the Taconian, there rests in discordant stratification in this region a vast series of sandstones and conglomerates, with contemporary basic plutonic rocks, the whole remarkable by the presence of metallic copper. This series, which had been alternately confounded with the Huronian and the Taconian on the one hand, and with the trilobitic sandstones of the Cambrian on the other, was for the first time separated by the author in 1873, under the name of the Keweenaw group, a term changed by him in 1876 to that of the Keweenian terrane. It still remains to be decided whether this series, upon which rest unconformably these same trilobitic sandstones, should form a part of the Cambrian, or should constitute a distinct terrane between the Taconian and the Cambrian.

§ 11. In submitting to his colleagues of the International Geological Congress this summary of his conclusions, based on over forty years of study, the author takes the liberty to state that the notions here advanced as to the origin, the chemical and mineralogical history, the subdivision, and the nomenclature of crystalline rocks, are for the most part the generalizations of a single observer. He now offers them as a first attempt at a classification of the indigenous rocks, and at the same time as an exposition of his crenitic hypothesis, and of the mineralogical evolution of the globe, which he conceives to have determined the succession and the chemical nature of the masses which he has named crenitic, as well as those of plutonic masses. He feels at the same time that his work is far from complete, and that to others must now be left the task of correcting and finishing it.

As a large part of these results, so far as regards geognostic classification, appeared for the first time in the Reports of the Geological Survey of Canada, the author may be permitted to say, in closing, that the first publications made by that Geological Survey on the crystalline rocks of Canada—that is to say, the reports of progress for the years 1845 and 1846, were prepared by him, and published in 1847, from the notes and the collections made by Logan and by Murray in the two years previous. Moreover, all the statements relating to the mineralogy, the lithology, or the chemical composition of the rocks of Canada, which are found in the official reports from 1847 to 1872, when the author resigned his position as a member of the Geological Survey of Canada, were written by him or under his personal direction.

T. STERRY HUNT.

SOME QUESTIONS CONNECTED WITH THE PROBLEM PRESENTED BY THE CRYSTALLINE SCHISTS, TOGETHER WITH CONTRIBUTIONS TO THEIR SOLUTION FROM THE PALÆOZOIC FORMATIONS.[1]

THE question of the "crystalline schists" still presents so many unsolved difficulties, and the views of contemporaneous fellow-workers diverge herein so widely, that an attempt at unanimous agreement on the points at issue must at present be regarded as premature. This assuredly does not prevent our taking counsel together, interchanging observations, and endeavouring to gain solid ground, whence a future solution can be aimed at. Each geologist will approach such a consultation in a way differing in accordance with his own experience.

I can only contribute experience gained by the study of the metamorphic crystalline schists, belonging to the *Palæozoic* formations, that have been proved to have resulted from the action of contact or dynamic metamorphism on eruptive or stratified rocks, the latter including the tuffs. The *direct* application of this experience to *all* Archæan crystalline schists appears to me premature—*i.e.* rather a *thema probandum* than *probatum*. Doubtless there are cases—as, for instance, in the so-called flasergabbros or zobtenites, which, apparently, must be regarded as quite analogous to the alteration of the diabases in the Palæozoic formations. Indeed, the same essential features which Lehmann has described in the development of the Saxon "flasergabbros" have been demonstrated by Teall in the Lizard gabbros, G. H. Williams in the Baltimore gabbros, and Hans H. Reusch in Norway. But Hans H. Reusch also mentions *bedded gabbros*[2] as well as eruptive flasergabbros, differing thus from Lehmann; while Credner and Roth appear by no means willing to concede all that is contained in Lehmann's book. This fundamental difference must, however, be noticed: Lehmann holds the Archæan schists half for metamorphosed sediments, half for interbedded or injected eruptive rocks; and although I cannot agree with or follow Lehmann in every detail (and, above all, lay more stress upon the altered tuffs), still on the whole I can but support him in this view. Roth, on the other hand, holds all the Archæan crystalline schists—limestones, quartzite, gneiss, mica-schist, amphibolite, &c.—for *schistose, plutonic* (only in form not eruptive) rocks (*Erstarrungskruste*); finally, Credner holds the majority of the crystalline schists, including granite-gneiss and flaser-gabbro, for the *normal stratified* sediments of a primæval ocean, their crystalline nature being essentially not due to metamorphism.

I have dwelt thus at length on this point in order to demonstrate that there exist numerous controversies even on those questions that admit of solution by reason of the *most undoubted pseudomorphic changes* (hornblende after diallage, hypersthene, augite; zoisite, epidote, actinolite, quartz, albite after lime-soda feldspar), and by reason of the presence of the *original eruptive structure*.

My stand-point is identical with that expressed by Carl Friedrich Naumann in the following words: *My task above all else is to study the metamorphism, with respect both to substance and to structure, of the fossiliferous sediments and the eruptive rocks, together with the tuffs intercalated therein.* Much has already been done, especially with respect to contact-metamorphism, which is more sharply defined than regional or dynamic metamorphism. There remains, however, much to answer, especially as the primary structures of original schistose eruptive rocks and the structure and substance of certain very common sedimentary rocks (as, for instance, the greywackes, the so-called greywacke-schists, or the majority of the tuffs) are still too little known to afford a firm basis for the study of metamorphic processes.

Still the detailed solution of the following question would be of no little value for the study of the Archæan schists:—

(1) What material agreement or difference exists between the

[1] "Einige Fragen zur Lösung des Problems der krystallinischen Schiefer, nebst Beiträgen zu deren Beantwortung aus dem Paläozoicum," von Prof. Dr. K. A. Lossen. "Etudes sur les Schistes cristallins," 1888. Published by the International Geological Congress in London, 1888. (Translated from the German by Dr. F. H. Hatch.)

[2] Giving a somewhat wide meaning to the word "gabbro"; he now says, "dioritic rock," "altered gabbro and diabase." In the Hartz the interesting gabbro-district of Hartzburg presents, among numerous other varieties, some which show layers alternately richer in plagioclase and diallage (bronzite) or present *flaser*-structure with biotite, and possess thus a *bedded-like* but not a true *bedded* parallel structure. These rocks are true eruptive gabbros.

results of metamorphism due to the contact of granite with fossiliferous sediments and the eruptive rocks intercalated therein, on the one hand, and the Archæan schists on the other?

For such a comparison useful data are furnished by the *Hartz.* These mountains, consisting of fossiliferous sediments and the most diversified eruptive rocks, already plicated at the Coal-measure period, represent a fairly average section of the earth's crust, *i.e.* although there is no axis of crystalline schists, the strata, together with diabases, keratophyres, and the accompanying tuffs, are considerably depressed between highly elevated plutonic rocks (granite, gabbro, &c.).

The *contact-zones around the gabbro and granite* present the following authigenic *minerals:* quartz, orthoclase, albite, plagioclase, biotite, muscovite, hornblende, actinolite, augite, bronzite, chlorite, epidote, garnet, vesuvian, tourmaline, axinite, wollastonite, cordierite, sphene, spinel, andalusite, rutile, magnetite, hematite, titaniferous iron ore, magnetic pyrites (pyrrothine), and other sulphur ores, calcite, fluorite, apatite; and continued investigations will easily add others to the list, as, for instance, anatase, zoisite, lithionite, lepidolite, corundum, sillimanite, cyanite, graphite—indeed, the four last-mentioned minerals have already been detected in certain mineral aggregations in post-granitic dykes of the Hartz, that probably are to be referred to metamorphic influence. But not only *do these minerals show great resemblance to those which are most frequently present in Archæan crystalline schists; their combination to definite mineral aggregates and rocks also makes the analogy even more complete.* In the *normal gneisses,* which are derived, with great diversity of structure, from the *culm-greywackes and the greywacke schists of the Oberhartz,* in contact with granite and gabbro, are intercalated *cordierite- and garnet-gneisses and augite- (or bronzite-) bearing gneisses,* which are produced by the alteration of schistose and calcareous sediments. *Saccharoidal quartzites* are clearly produced by the recrystallization of *Carboniferous* or *Devonian lydites* (*Kieselschiefer*); and it is very difficult to distinguish these from rocks produced by the contact-metamorphism of nearly pure quartz-sandstone (*Quarzitsandsteine*). *Hornstones* (*cornéenne*), which contain garnet, amphibole, augite (or bronzite), schorl, andalusite, apatite, as well as orthoclase and plagioclase, are found replacing *mica-schists* and phyllites. The thin *limestone-seams in the Lower Devonian* (*Hercynian*), Upper Devonian, and the Culm-measures, are partly metamorphosed to compact or phanero-crystalline "*lime-silicate-hornstones,*" containing garnet or other allied silicates—vesuvian, epidote, malacolite, cordierite, amphibole, sphene, &c., in places also fluorite or axinite, and corresponding to the garnet-rocks, epidote-rocks, pyroxenites, ecklogites, &c., of the Archæan formation.

In part, however, they have undergone *marmorosis,* while being impregnated with garnet or other silicates and locally with ores; even anthraconite is not altogether absent from these marbles. Amphibolites are in part also derived from *calcareous sediments;* those, however, that contain felspar (plagioclase) in any essential quantity can be demonstrated to result from the *contact-metamorphism of pre-granitic, Devonian, and Carboniferous diabases that have been plicated and metamorphosed in common with the strata.* Further, there are, in the granite and gabbro contact-zones, *alteration products of the diabase* that are *rich in biotite;* and other pre-granitic eruptive masses, such as the *augite-keratophyres* and the *augite-orthophyres,* show a great abundance of biotite, which is associated with a recrystallization of the orthoclase and of a part of the augite. This biotite is certainly developed at the expense of chlorite derived from augite or primary hornblende.

Schistose rocks with more abundant biotite, that are locally present among the more dominant massive rocks, bear the strongest resemblance to garnetiferous *mica-schists.* In the *porphyroids of the Hartz,* which occur both within and without the contact-zones, we mainly find *sericitic muscovite;* beyond the contact-zone it occurs in such abundance as to produce very schistose sericite rocks, which, on the other hand, are here also derived directly from the porphyritic massive rocks. These porphyroids I regard, from my present stand-point, as the metamorphosed pre-granitic tuffs of quartz-keratophyres and quartz-porphyries. To these tuffs are perhaps related certain *hornstones, very rich in orthoclase,* which occur in the granite contact-zone with Devonian and Carboniferous siliceous schists (equivalents of Adinole?).

Other questions are:—

(2) What differences exist in the *order of crystallization* of the minerals which compose granites, quartz-diorites, gabbros, diabases, in short holo- and phanero-crystalline eruptive rocks, and that of the secondary minerals produced in the contact-metamorphism of these eruptive rocks?

This question must be the more carefully answered, as, in spite of the rich material so excellently collected and cleverly arranged for the use of science by H. Rosenbusch, the order of crystallization of the eruptive rocks is not yet firmly established. A certain degree of regularity is undeniable; but, on the one hand, the chemical law is, as Lagorio has demonstrated, more intricate than that formulated by Rosenbusch; and on the other, the order varies quite unaccountably with alterations in the physical conditions of consolidation (compare granite and pegmatite).

(3) Is the ophitic (diabase-) structure under all circumstances the structure of an eruptive rock, or are there undoubted sedimentary rocks possessing a similar structure?

(4) It has been proved that graphic granite, as micro- and macro-pegmatite, forms an integral part of true eruptive rocks, especially of granite and its porphyritic modification. Since graphic granite is very common among the gneisses, the question arises whether it is to be regarded as a true eruptive rock, or whether such occurrences can be proved to have been produced by thermal action, or even lateral secretion, in the sense of a partial solution of the neighbouring rocks.

Even if it be admitted that all minerals can be produced, by a suitable variation of the conditions, either by consolidation, by separation from aqueous solutions, or by sublimation, still it does not follow, to my mind, that all the structures that combine minerals to regular aggregates, can be produced in like manner in these three modes of formation. It seems to me that such structures—as, for instance, the ophitic (diabasic) or the pegmatitic (to say nothing of the structures which are developed in rocks containing glass or other base)—that have been demonstrated to be *characteristic of rocks of undoubted eruptive origin, must rather be regarded as indicating an origin by consolidation from a magmatic condition, so long as contrary proofs are not forthcoming.* No one, to my knowledge, has ever maintained that the ophitic or diabasic structure can be of sedimentary origin; but gabbros have been claimed—wrongly, as I believe—as sediments, in spite of the close relation of their structure to that of the diabases.

As regards *graphic granite* (or *macro-pegmatite*), the case is somewhat different.

The frequent occurrence of such masses in gneiss has created the notion that they are integral components of the *sedimentary* gneisses. And this view is maintained, although a considerable portion of these pegmatitic masses can be clearly seen filling vein-like cavities, while another part make up lenticular patches that follow, more or less, the dip and strike of the schists. The occurrence of simple aggregates of quartz and feldspar, that are of *thermal* origin, must, then, in accordance with one's experience of regional and contact-metamorphism, be unconditionally conceded; while the absence of such aggregates in the greywackes appears to me to absolutely disprove a development by lateral secretions. It is therefore not inconceivable that the pegmatitic aggregates represent, so to speak, the quintessence of the gneiss, exuded into primary cracks. At the same time, great caution is to be recommended; for, since the introduction of the microscope, *micropegmatite* has, little by little, been recognized as an essential constituent of numerous acid and basic (with SiO_2 per cent. as low as 48) rocks. The veins of graphic granite in the Hartzburg gabbro have been held by some for segregation-veins. They are, however, demonstrably apophyses of the eruptive granite; indeed, the principal mass of granite in the Brocken *massif* is, in the main, micropegmatitic. The *banded structure, with bilateral symmetry,* of many pegmatites, which has been compared to that of many mineral veins, is no proof of their non-eruptive nature. The augites, felspars and other minerals of lavas present banded structures with variable chemical composition: banded structure with a chemical composition varying from that of diabase to granite-porphyry, is shown by compound eruptive dykes, as has lately been well shown by Bücking, in the Thüringerwald ("Jahrb. d. kgl. preuss. Geol. Landesanst. f. 1887," p. 110, *et seq.*). Even the *drusy character and the richness in minerals presented by the central portion of many pegmatite-dykes* finds its analogy in the external shells of true eruptive granites, which may, however, be complicated by the influence of thermal actions, accompanying, or subsequent to, eruption. *Giant spherulites,* of a decimetre diameter, *composed of macropegmatite, enveloping a porphyritic Carlsbad twin of potash-feldspar* (orthoclase or microcline), that occur in the granite of the Riesengebirge, repeat, on the large scale, the microscopic characters of the micropegmatite of certain quartz- and granite porphyries (the granophyre of Rosenbusch). *All these phenomena compel the assumption that at least a part of the pegmatites are of indubitably eruptive origin*, and arouse in us the question whether this structure is not to be brought into connection with the origin of the gneisses.

(5) What are the differences between the *primary structures* (*due to consolidation*) of the plutonic and volcanic rocks and the *structures* of (*a*) the crystalline sediments, (*b*) the metamorphic rocks in contact with granite, (*c*) the crystalline schists?

(6) What *reliable* characters have we, to distinguish crystalline grains developed *in situ* from clastic grains, in cases where they occur, side by side, in one and the same rock?

The answer to this question has already frequently been attempted, among others in the most praiseworthy manner by A. Wichmann. It requires, however, a fresh solution based on the latest experiences. The safest test of the authigenic, non-clastic nature of a grain is doubtless the presence in it of enclosures of minerals that are also present in the rock as authigenic constituents. External form and internal molecular relations, in consequence of pressure-phenomena, can, however, be very misleading. Hard minerals, especially, occur in clastic sand in very sharp crystals (quartz, tourmaline, zircon, &c.).

(7) Are the views of those authors justifiable, who conceive certain gneisses or porphyroid crystalline schists to have been produced by the injection of a granitic magma, *in discontinuo*, between the schists (*Schiefer*)?

(8) If the views expressed in the preceding question are justifiable, how are the gneisses and porphyroids, produced by the addition of granite *in discontinuo* to slaty sediments, to be distinguished (*a*) from true eruptive granite or its porphyritic modification, both having, under the influence of pressure, undergone a "phyllitic" modification; (*b*) from slaty sediments in which aggregates or crystals of silicates have been deposited from water (quartz and feldspar)?

(9) What differences can be established in mineral composition and structure between a true eruptive granite and an indubitably stratified (not simply jointed or cleaved) so-called "*Lagergranit*" or granite-gneiss?

An *amalgamation of eruptive granite* with *the mineral aggregates of the rocks in contact* has, according to my experience taken place in some cases; but I have not yet observed an undoubted discontinuity in such granitic material. It is much to be desired that the French geologists (for instance, Michel-Lévy and Charles Barrois), who defend the views formulated in Questions 7 and 8, would enlighten us by good drawings of macro- or microscopic sections, as to how far in this difficult question *an incontestable separation of injected eruptive granite from metamorphic gneiss* is possible. This would, without doubt, facilitate the solution of Question 9. Unanimity on this point will scarcely be obtained without a careful structural diagnosis, which, of course, must be supported by serviceable material, self-collected in the field.

(10) Are there any *absolute* material and structural differences between metamorphic rocks of the granite contact-zone (hornstones, cornéenne, &c., cp. Question 1) and rocks affected by regional or dynamic (*Dislocations-*) metamorphism? or are such differences only *relative*, and what are they?

The exact solution of this question requires, above all, the assumption that only such occurrences shall be submitted to consideration that are unmistakably connected with *visible* eruptive rocks. It should also not be forgotten that rocks which have originally undergone contact-metamorphism have, in some cases, *subsequently* lost their peculiar characteristics in consequence of the influence of regional metamorphism. With this qualification I am personally inclined to concede only a *relative* and not absolute differences. I am guided in this, not only by my experience in the Hartz, which has made me acquainted with the remarkable variation of the metamorphic rocks in contact with granite, according as they occur just outside the contact-zone or in its outer, middle, or inner division; or again according as they belong to the unpenetrated but eroded mantle of the eruptive cores, or to masses, of greater or smaller extent, that have sunk deep in between the eruptive masses and have been covered up by them. The rocks occurring thus differently vary between a phyllitic clay-slate and gneiss, while the main mass of the slate- and grauwacke-hornstones present little resemblance to the crystalline schists. In the classic region of the Erzgebirge, however, there occur, according to the careful investigation of our Saxon colleagues, *compact hornstone-like or even conglomeratic greywacke gneisses* (the mica-trap of older writers) that present this analogy in a complete degree. The same analogy is presented by Gosselet's Lower Devonian "*cornéite*" (to be distinguished from *cornéenne*, the product of contact-metamorphism) from the regionally metamorphic Ardennes of Belgium. Again, *the Lower Devonian fossiliferous sediments* of the Ardennes, *containing garnets, hornblende, and graphite*, that are so well known through A. Renard's admirable descriptions and drawings, remind one of hornstone, although no contact with eruptive rocks has been observed affecting either them or the *Cambrian garnetiferous* "*Wetzschiefer*" of Vielsalme. The association of such hornstone-like rocks with those of the usual phyllitic type of regional metamorphism recalls the occurrence of lime-silicate-hornstones in the outermost zone (beyond the zone of the "Knotenschiefer" around the granite of the Rammberg. Whatever explanation of these phenomena may be given—Gosselet is decidedly in favour of dynamic metamorphism as opposed to a latent contact-metamorphism—at least this is evident, that important contributions to the question, here formulated, can be furnished by the Ardennes.

ON THE CLASSIFICATION OF THE CRYSTALLINE SCHISTS.[1]

THE most important constituent of the earth's crust—the crystalline schists—has remained, with respect to their field-relations and their origin, the most shrouded in darkness. The difficulties that bar the way are quite exceptional. We have frequently to deal with rocks that have undergone subsequent alteration, without being able to determine their original constitution, and without being able to explain the nature of the change. We have, as it were, to deal with an equation with two unknowns—we cannot solve it.

At the present time we meet with a number of attempts to classify the crystalline schists, mainly according to petrological characters, in stratigraphical groups. I regard these attempts as premature, for this reason: microscopists are unfortunately very behindhand in the exact investigation of the crystalline schists, and of the half-clastic, half-crystalline sediments. The purpose of these lines is to direct attention to another difficulty which has not yet received sufficient consideration, but which bars the way to every attempt of that kind—namely, *the mechanical metamorphism during mountain-formation.*

That, by the plication of the Alps, the constitution of the rocks has been completely changed, is most directly proved by an examination of the sedimentary rocks; because the latter can be also studied in an unaltered condition in adjacent localities. The commonest changes met with here in connection with folding are:—

Deformation of fossils, pebbles, or crystals (compression in one direction, extension in another).

Cleavage (*Transversalschieferung*).

Cleavage with linear extension.

Puckering.

Internal formation of breccias and cementing of the same by secretions.

Internal formation of innumerable slickensides, so as to change the whole structure.

Scaly structure, produced by the compression of oolitic structure.

Alteration of hematite and limonite into magnetite, in connection with cleavage.

Marmorosis of the limestones.

Formation of confusedly "kneaded" structures (*Knetstructuren*).

Development of new minerals (garnet, staurolite, mica) in places that have undergone crushing.

Now, sedimentary rocks, metamorphosed in the above way, are frequently found in extremely narrow synclinal zones, nipped in between rocks belonging to the crystalline schists. The

[1] "Zur Klassification der krystallinischen Schiefer," von Prof. Dr. Albert Heim. "Etudes sur les Schistes Cristallins." Published by the International Geological Congress in London, 1888. (Translated from the German by Dr. F. H. Hatch.)

Alpine zones, which consist mainly of crystalline schists, are termed *central massifs*. Such intercalations of mechanically metamorphosed sediments with the crystalline schists are very frequently to be observed at the ends of the strike of the *central massifs*, and between the *central massifs*; they are not rare even in the interior of the *central massifs*. The crystalline schists and metamorphosed sediments not only present the same stratigraphical position, but also similar characters in other respects. The cleavage of the sedimentary rocks may be continued in the same direction into the crystalline schists; and similar contortions may traverse both: in the latter, as in the former, a marked linear extension in the same or but slightly deviating direction may be present: calcareous patches in the crystalline schists are crystalline and granular, and contain layers of mica-scales which have undergone extension, precisely as in the neighbouring Jurassic limestones, &c., &c. From these facts we see that in these crystalline schists we have not to deal with rocks of original constitution, but that both these rocks and the sediments have undergone similar mechanical metamorphism. The only difficulty in dealing with the schists is contained in the fact that we are never in a position to describe the original appearance of the rock before it underwent the mechanical metamorphism.

Now it is in the crystalline schists that the plications of the earth's crust are most potently developed. The isoclinal and fan-shaped folds, the wedging and "kneading together" at the contact with the sediments—in short, all these high forms of dislocation, which are the earliest to modify the inner structure of rocks, are to be found in the crystalline zones of the Alps. They are most highly developed in the northern series of the *central massifs* (Mont Blanc, Aiguille Range, Finsteraar-*massif*, Gotthard-*massif*, Silvretta-*massif*, &c.).

At first sight it appears as if the crystalline schists and the true sediments, in the Alps, were separated by a constant unconformity; but frequently even recent sediments are found folded in, parallel with the crystalline schists. Again the sediments often take the position of a *central massif*; indeed, it seems as if a great part of several of the *central massifs* consisted of Palæozoic sediments. On the other hand, in the southern *central massifs* of the Central Alps, we see the crystalline schists lying in all respects like the sediments.

Those who have worked in these parts of the Alps will have remarked how often the mechanical crushing undergone by the rocks obliterates the limits of stratigraphical and petrographical characters, and how many rocks have become confused thereby in their development (*Ausbildungsweise*). Such changes can sometimes be directly proved to be the result of local crushing; sometimes, however, they are regional, and then passages into the unaltered rock are difficult to trace. All degrees of change by earth-movements are to be found, from a slight alteration of the structure up to complete metamorphism. In hundreds of places one does not know whether one has to deal with the residual traces of original bedding or with a cleavage (*Transversalschieferung*, *Quetschungsschieferung*) that has completely obliterated the original structures. In many cases it is impossible to distinguish between a schistose structure (*Schieferung*), superinduced by earth-movements, and one that is original. Schistose structures which cross one another are by no means rare. Whether the more pronounced or the less definite one is then the original is often not to be decided. Even an exact microscopical examination will often not suffice to distinguish between structures resulting from crushing and lateral deformation, and the fluxion-structure of an eruptive rock. It is certain that a structural modification by earth-movements has everywhere taken place where linear extension abounds. The latter is never original. In such crystalline schists with linear-parallel structure there are often elongated, ragged mica-scales. The linear extension can go as far as the development of rod-like separation (*stenglige Absonderung*).

Are there any rocks left in the *central massifs* of the Alps which have undergone no change in structure during the orogenetic processes?

The metamorphism can penetrate still deeper.

Enormous zones, for instance, in the interior of the Finsteraar-*massif*, that were formerly held to be true crystalline schists, prove to be originally clastic rocks of the Carboniferous period that have been squeezed into schists, and pervaded by secondary mica. Conglomeratic rocks of the Verrucano group, and clay-slates, nipped into the *central massif*, have become crystalline, schistose, and even gneissose. They can scarcely be distinguished, in the field and in the hand-specimen, from crushed gneisses pervaded by sericite. Granites can be proved, locally and perhaps also regionally, to have been compressed into gneisses. Gneisses, having a different position relatively to the pressure, have locally become granitoid. Massive eruptive felsite-porphyries have become felsite-schists. Mica-schists have been dragged out; their quartz grains ground down; and the whole converted into a rock that one would be inclined to describe as a sandy clay-slate. Even Liassic slates with fossils have been converted into garnetiferous mica-schists, staurolite-schists, &c. The boundary between the old crystalline schists and real sediments in the Alps has, by such processes of dynamic metamorphism, been obliterated, and the proper character of the rock so altered as to render recognition impossible. When we see, in true sediments, new minerals developed by the progress of the mechanical metamorphism (magnetite in the crushed Oolitic ironstone of the Windgälle, garnet in the Belemnite-slates of Scopi), the question arises, for the crystalline schists of this and neighbouring regions—Which minerals are original, and which have been produced subsequently, by orogenetic processes?

We arrive at this conclusion:—*The constitution of the crystalline schists in the Alps has been much changed by the orogenetic process* (dynamic metamorphism). *Original material and material mechanically produced at a later period, are often not to be separated from one another.*

Besides these, the Alps present other difficulties that stand in the way of the recognition of a stratigraphical grouping of the crystalline schists. The field-relations are frequently so intricate, that often it is very difficult to decide what originally lay under and what above; and whether the enormous thickness, for instance, of many gneiss-complexes, is real, or merely produced by repetitions of the folding, the folds being concealed by cleavage.

It follows that, if, on the basis of petrographical relations, a general stratigraphy of the crystalline schists is to be attempted, *this must never take place as the result of observations made in plicated regions of the earth's crust: districts must rather be chosen which are not influenced by disturbances of the Alpine character.* In the question of the stratigraphy of true crystalline schists, the Alpine geologist is not in the position to furnish material of essential value; he must rather wait for the results of the workers in other regions, in order to be able to apply them to his own district. The dislocations of fractured regions have, in the main, left unaltered the constitution of the rocks. There, then, the crystalline schists can be studied in their unaltered condition. There also they lie in flatter and more regular bedding; and a stratigraphical sequence is sooner to be found than in the Alps.

ON THE ORIGIN OF THE PRIMITIVE CRYSTALLINE ROCKS.[1]

IN this paper the author briefly summarizes the ideas prevailing on the origin of the crystalline schists, and throws a doubt on the current opinion that the primitive rocks have been formed by the *direct* crystallization of their constituents. He divides his treatise into two parts: (1) stratigraphical considerations; (2) the mode of association of the component minerals.

(1) *Stratigraphical Considerations.*—The primitive crystalline rocks form the fundamental floor upon which lie the earlier detrital deposits, their schistosity being often parallel to the stratification of the latter.

Although composed mainly of acid gneisses, the primitive rocks present countless variations in chemical and mineralogical composition; they include very basic representatives, such as the amphibolites, pyroxenites, peridotites, cipolines, and dolomites, &c. These intercalations are always parallel to the schistosity: they form elongated lenticular patches, of which the greater axis is in the direction of the general banding.

At the same time, their relative homogeneity in composition is shown by comparison of sequences established, not only in Europe, but also in the United States and the rest of the world. Acid gneisses predominate at the base; then come frequent intercalations of mica-schists and leptynites, with which are

[1] "Sur l'Origine des Terrains Cristallins Primitifs," by M. A. Michel-Lévy, Bull. Soc. Géol. France. 3e série, t. xvi. p. 102, 1888. Published by the International Geological Congress in London, 1888. (Abstracted from the French by Dr. F. H. Hatch.)

associated amphibolites and cipolines. Above this first division chloritic and sericitic mica-schists are developed, alternating occasionally with amphibolitic layers. This second stage is succeeded by a series which also comprises hornblendic and augitic (*cornes vertes*) schists, but includes, further, the first detrital deposits. At every horizon there is a gradual passage from the one stage to the other. The first detrital deposits alternate with sericitic and chloritic schists; and even as far up as in the Cambrian, large bands of felspathic schists, which can scarcely be distinguished from the more ancient gneisses, are developed in connection with the intrusion of granite.

The primitive rocks are, as first pointed out by the author, injected and penetrated by ancient eruptive rocks. This phenomenon is al o to be observed in the earlier detrital schists.

Rolled pebbles and fragments of gneiss, mica-schist, &c., have been repeatedly found in the granitic and granulitic gneisses of various localities. The author's own observations lead him to compare these phenomena with those in which rounded balls have been inclosed in a truly eruptive granite. In numerous cases, in which fragments of gneisses have been enclosed in other gneisses, he has always been able to prove that the enclosing rock is much more felspathic than the inclosed fragments.

These facts cannot, therefore, be advanced in support of the detrital origin of true gneisses.

(2) *Mode of Association of the Component Minerals.*—The mineralogical composition of the gneisses and of the schistose basic rocks associated with them, is nearly identical with that of the granular eruptive rocks; and all the types of the older eruptive rocks have their representatives in the schistose series.

A great analogy therefore exists between the natural forces instrumental in the production of the two series.

Speaking generally, the older eruptive rocks are rigorously homogeneous over vast areas: fragments of these rocks are everywhere comparable to one another. This homogeneity is reproduced in the schistose series; but it is, so to speak, periodic, and one must first know the orientation before comparing fragments taken from a distance.

The structure of the gneisses presents a series of successive crystallizations, accompanied by mechanical phenomena and a cementing of the dislocated components. The author, while seeing in these phenomena the traces of a series of metamorphic actions, followed by the injection of foreign material, does not wish to deny the additional intervention of secondary mechanical actions. But, whatever theoretic explanation be adopted, the facts are well established, and irreconcilable with the assumption of a preliminary mixing of the magma of the schistose rocks, and therefore with the hypothesis of a primordial origin.

The author then proceeds to demonstrate at some length that the intimate structure of the gneisses is identical with that of sedimentary schists modified by contact metamorphism, and finally injected by eruptive rocks.

Microscopic studies have disclosed the minute liquid inclusions contained by the quartz of the gneisses. Zirkel and Kalkowsky have made the interesting observation that the streams of inclusions are restricted to the central portions of the quartz-grains and are not prolonged to the periphery; and De Lapparent adduces this fact as a proof that the grains have not been derived from a pre-existing rock. But this argument is overthrown by the fact that the quartz-grains in the Cambrian micaceous schists, which are of indisputably detrital origin, present exactly the same phenomenon. It admits, moreover, of a very simple explanation. These quartz-grains, of clastic origin, have undergone subsequent enlargement by the assimilation of secondary quartz, which tends also to give them an exterior crystalline form. This secondary quartz is poor in liquid inclusions, and encloses scales of black mica and other minerals.

General Considerations and Hypotheses on the Origin of the Primitive Rocks.—Among the hypotheses advanced to explain the origin of gneiss, the author discusses the two that have found the most general acceptance. The first, which is now somewhat abandoned but has the merit of perfect clearness, makes the gneisses the result of a kind of conflict between water and the primary molten magma of the earth. The other explanation, which is more vague, accords to the gneisses a sedimentary origin. They are the deposits of a kind of supersaturated sea, which precipitated on to its floor the successive crystalline bands which characterize the gneisses. Note that this hypothesis presupposes a floor—an unknown substratum.

(1) Geologists originally supposed that the first substratum was formed by the granites which are found cropping out over such vast areas. Detailed studies have shown, however, that the granites are younger than the gneisses which they traverse, inject, and displace. Even the most ancient among them are at least younger than the first detrital schists.

It is therefore to the gneisses, distinctly banded and alternating in their lower beds with mica-schists, that this mixed origin—this *rôle d'écumes primordiales*—must be attributed.

Has this substratum of the terrestrial crust ever been seen in the most disturbed regions?

Cordier supposed that terrestrial refrigeration was constantly increasing, in the downward direction, the thickness of the first solid crust. If we could descend through the earth's crust, we should pass successively through rocks of increasing basicity until we should find, enveloping the still incandescent nucleus of impure iron, a rock analogous to lherzolite.

A serious objection to this is the fact that a descending order of basicity is not borne out by the stratigraphical relations of the gneisses. Lherzolite is found erupted through the primitive rocks; and the basic peridotites are intercalated moderately high up in the gneissic series.

From the purely speculative point of view it is improbable that the first products of consolidation did not receive a thorough mixing, rendering the rock homogeneous, and preventing the formation of those numerous micaceous membranes so characteristic of the primitive rocks. If these first products were acid, as there is reason to suppose, the first substratum must have constituted a massive and homogeneous granite. It is on a floor of this kind that the precipitation of the atmospheric waters must have prepared the elements of the first detrital rocks—the first arkoses.

(2) The second explanation—the successive crystallization of bands of gneiss from the waters of a universal sea—encounters similar difficulties. It appears to the author irreconcilable with the structure of the gneissic rocks. The continuous membranes of mica, and the almost vein-like appearance of the quartz and felspar, do not accord with the notion of concretionary deposits that this hypothesis requires, supposing the supersaturated liquid to have been in a state of perfect tranquillity. If, on the other hand, we suppose that there existed local agitations due to the unequal distribution of high temperatures, the remarkable periodic homogeneity of the gneisses becomes inexplicable.

From a consideration of these facts and hypotheses, the author arrives at the conclusion that the veritable and primary substratum of the terrestrial crust is not visible; that this substratum has undergone much alteration; finally, that the so-called primitive rocks are a complex of eruptive rocks, later than the gneisses, and of rocks which are really detrital, but which have undergone excessive metamorphism.

The eruptive rocks, by which the primitive rocks have been injected, are later than the beginning of the Cambrian. They were produced in extraordinary abundance in the later portion of this period: granites, diabases, diorites, norites, and lherzolites.

In discussing the primary causes of the eruption of these rocks, the author mentions that Lehmann and others of the German school, are inclined to seek them in the partial transmutation into heat of the mechanical work performed during the intense periods of contortion undergone by the earth's crust. The author himself refers them to manifestations of the internal heat of the globe, the great earth-movements having simply effected the ascension and injection of the eruptive magmas.

NOTES.

By the death of Mr. Jameson on the Upper Congo, science has lost a most promising young naturalist. The collections made by him some years ago in Borneo were never described, but we believe that in that island Mr. Jameson met with many species of birds since obtained by other travellers. His expedition to Mashoona Land resulted in the discovery of some interesting new species of birds, and an elaborate paper was written on his collection by Captain Shelley in the *Ibis* for 1882. A small number of birds has also been sent by him from the Aruwimi River to his friend Mr. Bowdler Sharpe, who has been waiting for further collections before writing an account of them. We do not know whether any further consignments are on their way from the district where Mr. Jameson was stationed for many months with the late Major Barttelot. He described the country as a disappointing locality for the collector, the few birds obtained by him being merely the ordinary Congo species.

We regret also to have to record the death of Mr. T. H. Potts, a well-known New Zealand ornithologist. Mr. Potts's name has been connected with the natural history of New Zealand for a number of years, and his observations on the nesting and life-history of the birds of his native country are among the most interesting contributions to the Transactions of the New Zealand Institute.

We have received a communication from Herr Gamél, of Copenhagen, the equipper of the Norwegian Expedition to Greenland, in which he informs us that if the undertaking has been successfully accomplished the members of the Expedition should be on board the sailing-ship *Peru*, which was to leave Disco Bay on September 16, and is due in Copenhagen in the middle of October. If not on board this vessel, the Expedition will have to remain in Greenland until next spring, as this is the last ship leaving, and no news will be obtainable from Greenland till then.

We learn from the *Scotsman* that the fishery cruiser H.M.S. *Jackal* lately left Granton on a scientific expedition, which will include a cruise of several weeks in the North Sea and a visit to the Baltic. The chief object in view is to collect data likely to throw more light on various questions which, when solved, will admit of a better understanding of the movements of the edible fishes and of the myriads of minute organisms on which they feed. The Expedition is under the direction of Dr. John Gibson, of the University of Edinburgh Chemical Laboratory, who is accompanied by Dr. Hunter Stewart and Mr. Maitland Gibson, also from the University of Edinburgh.

Many students of science will regret to learn that the *Naturforscher* has ceased to appear. The last number is dated September 23.

Preparations have been made for effecting the proposed connection between the Observatory of Paris and Greenwich. It is expected that this will lead in the end to the acceptance of the Greenwich meridian by French astronomers.

The General Omnibus Company in Paris has introduced into its service the electricity supplied by the Electric Storage Company. The carriages run from the Arc de Triomphe to Courbevoie, a distance of about two miles. Each of the two fore-wheels is put into rotation by a separate dynamo, over which the driver exerts control. The velocity is somewhat greater than that obtained with horses.

Three new sulpho-chlorides of mercury have been isolated by Drs. Poleck and Goercki, of Breslau. Every student of chemical analysis is familiar with the peculiar changes of colour which occur when a solution of mercuric chloride is precipitated by sulphuretted hydrogen gas; how that the precipitate at first is perfectly white, shortly passes to a yellow, and then rapidly darkens, becoming orange, brownish-red, and finally, when excess of the gas has been led through the solution, perfectly black. The white compound first formed was shown so long ago as 1828 by Rose to consist of a sulpho-chloride of the composition $2HgS . HgCl_2$; but the further changes appear never to have been hitherto thoroughly investigated. The Breslau chemists, after fully confirming the composition of the white substance, now show that the darkening is due to the formation of successive higher sulpho-chlorides, $3HgS . HgCl_2$, $4HgS . HgCl_2$, $5HgS . HgCl_2$; the final product being, of course, the sulphide of mercury, HgS, itself. This has long been supposed to be the case, and it is very satisfactory to have these various sulpho-chlorides at last isolated. It may readily be seen, however, that by simply passing the current of sulphuretted hydrogen until the precipitate became of any particular tint, one would never be able to isolate these higher compounds, the mixture becoming more complicated every minute. The method adopted, after many fruitless attempts, consisted in completely precipitating in various experiments quantities of mercuric chloride corresponding to three, four, and five molecules respectively; the precipitates were in each case transferred to a flask fitted with inverted condenser, and digested for some time with a fresh quantity of the chloride corresponding to another molecule. The first product, $3HgS . HgCl_2$, possessed a brownish colour, and the two higher ones more and more nearly approximated to the black of the pure sulphide of mercury. In each case the filtrate was found to be free from quicksilver and chlorine, proving that the extra molecule of the chloride had in each case combined, and analysis showed that the precipitates really possessed the compositions above indicated. These sulpho-chlorides, moreover, are very stable; they are almost perfectly insoluble in water, and may be digested with water in sealed tubes at 200° C. without undergoing any change. They are also insoluble in both hydrochloric and nitric acids, but dissolve in the mixture of the two known as aqua regia. They were finally shown to be distinct chemical compounds, and no mere mechanical mixtures of sulphide and chloride, by the peculiar action of potassium iodide upon them. It may therefore be considered that the question of the action of sulphuretted hydrogen upon mercuric chloride has now been definitely settled.

Prof. H. A. Hazen, of the Signal Service, Washington, has compiled a "Hand-book of Meteorological Tables," containing in a convenient form all the reductions needed for current work, omitting those not now generally used, such as Reaumur temperatures, &c. Several of the tables are new, or re-computed in their present form after some years' experience of the author in their use. The table for reduction of barometrical observations to sea-level has been extended to 8000 feet. Among the useful additions we may mention formulæ and tables for the determination of mean wind direction, and for the conversion of wind velocities from miles per hour to metres per second, and *vice versâ*. The latest determination of the metre is used in all linear tables.

On the night of September 5 a brilliant meteor was seen at Bolmen, in Småland, in Sweden. It first went in a straight line from east to west, when it suddenly altered its course, falling to the earth with a dull report. Its colour was bluish-white.

Snow and frost are reported from several parts of Sweden, whilst flocks of birds have been seen migrating southwards.

The preservation of the eider on the south coast of Sweden has had the most beneficial results, considerable flocks of these birds being now often seen.

Two runic stones have been discovered at Sorunda, in Sweden.

The Swedish Consul at Eskefjord, in Iceland, writing at the end of August, states that although the fjord was free from ice there were still large masses of drift-ice along the east and north coasts, which were practically unapproachable for vessels. There was also much drift-ice in Denmark Sound. The cod and herring fisheries had been good.

A Norwegian naturalist, Herr L. Ucherman, draws attention to the peculiarly green waters of certain rivers in Norway, emanating from those snow-fields which never melt, and describes the colour as due to certain green Algæ on old snow. In support of this he mentions that, when walking across old snow in the highest parts of Norway this summer, he noticed that footprints a sumed a greenish hue, which was not the case with new snow. It has generally been assumed that the snow Algæ, so well known in higher latitudes, did not as a rule flourish on snow in Norway.

THE Society for Promoting Christian Knowledge will publish shortly a "Star Atlas," containing maps of all stars from 1 to 6·5 mag. between the North Pole and 34° south declination, and of all nebulæ and star clusters in the same region which are visible in telescopes of moderate powers. The explanatory text, by Dr. Hermann J. Klein, has been translated and adapted for English readers by Mr. Edmund McClure.

MESSRS. CROSBY LOCKWOOD AND SON will publish during the ensuing season the following works bearing on science:—"The Metallurgy of Gold," a practical treatise on the metallurgical treatment of gold-bearing ores, including the processes of concentration and chlorination, and the assaying and refining of gold, by M. Eissler, formerly Assistant Assayer of the United States Mint, San Francisco; with 90 illustrations. "Practical Surveying," a text-book for students prepa'ing for examinations or the colonies, by George W. Usill, A.M.I.C.E.; with upwards of 330 illustrations. "Tables, Memoranda, and Calculated Results for Farmers, Agricultural Students, Graziers, Surveyors, Land Agents, Auctioneers, &c.," with a new system of farm book-keeping, selected and arranged by Sidney Francis; waistcoat pocket size. Also the following new volumes in Lockwood's series of "Handy-books for Handicrafts":—"The Model Engineer's Handy-book," a practical manual, embracing information on the tools, materials, appliances, and processes employed in constructing model steam-engines, by P. N. Hasluck; with about one hundred illustrations and working drawings (in the press). "The Clock Jobber's Handy-book," a practical manual, embracing information on the tools, materials, appliances, and processes employed in cleaning, adjusting, and repairing clocks, by P. N. Hasluck; with about one hundred illustrations. "The Cabinet Worker's Handy-book," a practical manual embracing information on the tools, materials, appliances, and processes employed in cabinet work, by P. N. Hasluck; with about one hundred illustrations.

IN an interes'ing paper presenting a concise history of the acclimatization of the Salmonidæ in Tasmania, Mr. P. S. Seager claims that success has been secured in the thorough and unquestioned establishment of salmon trout and brown trout, both of which species are now abundant in Tasmania. The establishment of the true salmon is still to some extent a matter of uncertainty. "It must, however, be borne in mind," says Mr. Seager, "that more than one specimen submitted for scientific examination to Dr. Günther and others have been pronounced *S. salar*, and that Sir Thomas Brady has publicly stated his belief that specimens shown to him are of the same species. In speaking of them commercially, Sir Thomas states that such specimens in a salmon-producing country would be accepted as salmon without a doubt." This being so, Mr. Seager is of opinion that the establishment of *S. salar* in Tasmania may almost be regarded as an accomplished fact.

ADVICES from the Philippine Islands, *viâ* Hong Kong and Yokohama, received at Queenstown from New York on Saturday morning last, state that over 300 lives were lost in those islands by the eruption of an old volcano, named Mayon, at the latter end of July. Several hundreds of houses were also destroyed by the lava and ashes, and the natives were in a state of panic. Volcanoes in the islands of the Bissayar group were also in a violent state of eruption, and it is thought there has been a terrible loss of life.

THE Artisans' Classes at the Royal Victoria Hall will reopen on Monday, October 1. Among the subjects taught will be arithmetic, physiology, physiography, shorthand, chemistry astronomy, mechanics, machine drawing, and electricity. Many of the classes are in connection with the Science and Art Department.

THE additions to the Zoological Society's Gardens during the past week include two Vulpine Phalangers (*Phalangista vulpina* ♀ ♀) from Australia, presented by Mr. J. M. Kirby; a Suricate (*Suricata tetradactyla*) from South Africa, presented by Lieut. Lionel de Latour Wells, R.N.; a Common Teal (*Querquedula crecca* ♀), British, presented by Mr. Bergman; an European Pond Tortoise (*Emys europæa*), European, presented by Master William Reed; a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; an Ourang-outang (*Simia satyrus* ♀) from Borneo, a Ruffed Lemur (*Lemur varius*) from Madagascar, a Larger Hill Mynah (*Gracula intermedia*) from India, two —— Tree Ducks (*Dendrocygna* ——) from the Celebes? deposited; a —— Capuchin (*Cebus* —— ♀) from Brazil, two Brush-tailed Kangaroos (*Petrogale penicillata* ♂ ♀) from Australia, purchased; a Chinese Goose (*Anser cygnoides* ♀) from China, received in exchange.

OUR ASTRONOMICAL COLUMN.

COMET 1888 *e* (BARNARD).—The comet discovered by Barnard on September 2 is increasing in brightness, but is still a faint object. M. Bigourdan describes it on September 5 as showing a round nebulosity from 1′ to 1′·5 in diameter, with a fairly stellar nucleus, of magnitude 11½ or 12. The nebulosity was not quite symmetrical with regard to the nucleus, but was most developed in the direction of position-angle 20°. The following elements are by Dr. A. Berberich from observations made at Strassburg, September 4 and 8, and Dresden, September 13 (*Astr. Nach.*, No. 2858):—

$$T = 1889 \text{ January } 29{\cdot}0959, \text{ Berlin M.T.}$$

$$\left.\begin{aligned} \omega &= 341^\circ\ 43'\ 27''{\cdot}9 \\ \Omega &= 358\ \ 6\ \ 20{\cdot}8 \\ \iota &= 166\ \ 20\ \ 28{\cdot}2 \end{aligned}\right\} \text{ Mean Eq. } 1888{\cdot}0.$$

$$\log q = 0{\cdot}252291$$

Error of middle place (O – C). $\Delta\lambda = -2''$; $\Delta\beta = 0''$.

Ephemeris for Berlin Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Sept. 30	6 40 1	8 37·7 N.	0·3694	0·3326	2·27
Oct. 2	6 37 54	8 23·4			
4	6 35 33	8 8·4	0·3637	0·3081	2·59
6	6 32 56	7 52·6			
8	6 30 2	7 35·9	0·3580	0·2822	3·00
10	6 26 48	7 18·2			
12	6 23 14	6 59·5 N.	0·3523	0·2550	3·51

The brightness on September 2 has been taken as unity.

Prof. Krueger has deduced very similar elements to the above, using an observation made at Hamburg on September 13 instead of that made at Dresden.

COMETS BROOKS AND FAYE.—The following ephemerides for these two comets are in continuation of those given in NATURE for September 20 (p. 503), and are by Dr. H. Kreutz:—

1888.	Comet 1888 *c* (Brooks). R.A. h. m. s.	Decl. ° ′	Comet 1888 *d* (Faye). R.A. h. m. s.	Decl. ° ′
Sept. 30	15 30 41	14 47·4 N.	7 6 24	14 3 N.
Oct. 2	15 37 15	13 28·2	7 10 19	13 41
4	15 43 35	12 11·6	7 14 8	13 19
6	15 49 42	10 57·6	7 17 51	12 56
8	15 55 35	9 46·3	7 21 29	12 33
10	16 1 16	8 37·7	7 25 0	12 10
12	16 6 46	7 31·7	7 28 25	11 47
14	16 12 6	6 28·2	7 31 43	11 23
16	16 17 18	5 27·1 N.	7 34 55	10 59 N.

Comet Brooks is slowly decreasing in brightness, but Comet Faye is brightening.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *SEPTEMBER* 30—*OCTOBER* 6.

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

At Greenwich on September 30

Sun rises, 6h. 1m. ; souths, 11h. 49m. 46·2s. ; sets, 17h. 38m. : right asc. on meridian, 12h. 28·4m. ; decl. 3° 4′ S. Sidereal Time at Sunset, 18h. 18m.

Moon (New on October 5, 15h.) rises, 23h. 22m.* ; souths, 7h. 30m. ; sets, 15h. 32m. : right asc. on meridian, 8h. 8·0m. ; decl. 20° 22′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	8 25	13 16	18 7	13 55·2	14 2 S.
Venus.....	8 5	13 12	18 19	13 50·4	10 55 S.
Mars......	12 20	16 9	19 58	16 47·9	24 1 S.
Jupiter....	11 11	15 25	19 39	16 4·1	20 13 S.
Saturn....	1 10	8 41	16 12	9 19·2	16 28 N.
Uranus...	6 53	12 25	17 57	13 3·6	6 7 S.
Neptune..	19 37*	3 24	11 11	4 1·7	18 56 N.

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

Occultation of Planet and Star by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
1	Saturn	—	15 59	16 49	110 288
3	*l* Leonis	5	3 0	3 52	55 199

Oct. h.
1 ... 15 ... Saturn in conjunction with and 0° 55′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Oct. 1,	4 13	*m*
			,, 6,	3 52	*m*
S Arietis	1 58·6	11 59 N.	,, 4,		*M*
Algol	3 0·9	40 31 N.	,, 2,	4 20	*m*
			,, 5,	1 8	*m*
R Persei	3 22·9	35 17 N.	Sept. 30,		*M*
ζ Geminorum	6 57·5	20 44 N.	Oct. 4,	4 0	*M*
R Cancri	8 10·4	12 4 N.	,, 2,		*M*
S Ursæ Majoris	12 39·1	61 42 N.	,, 1,		*m*
U Ophiuchi	17 10·9	1 20 N.	Sept. 30,	21 20	*m*
			Oct. 5,	22 6	*m*
W Sagittarii	17 57·9	29 35 S.	Sept. 30,	19 0	*m*
β Lyræ	18 46·0	33 14 N.	Oct. 3,	19 0	m_2
R Lyræ	18 51·9	43 48 N.	,, 3,		*m*
S Sagittæ	19 50·9	16 20 N.	Sept. 30,	21 0	*M*
X Cygni	20 39·0	35 11 N.	Oct. 3,	5 0	*M*
T Vulpeculæ	20 46·7	27 50 N.	,, 3,	5 0	*M*
Y Cygni	20 47·6	34 14 N.	,, 2,	3 0	*m*
			,, 5,	3 0	*m*
δ Cephei	22 25·0	57 51 N.	Sept. 30,	21 0	*m*

M signifies maximum ; *m* minimum ; m_2 secondary minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near η Aurigæ	75	41 N.	Swift ; streaks. October 2.
	225	52 N.	Bright ; slow. October 2.

GEOGRAPHICAL NOTES.

LIEUT. WISSMANN, who is to command the German Emin Pasha Expedition, has already done much excellent work in Africa, for which he received one of the medals of the Royal Geographical Society a few months ago. In his hands the interests of science are sure to be attended to. The Expedition will consist of two contingents, which will proceed through German East Africa by the south shore of Victoria Nyanza to the region between that lake and the Albert Nyanza. That the Expedition is sure to meet with difficulties is evident from the telegrams which are almost daily appearing from Berlin and from Zanzibar. The whole coast region is rising against the Germans, and it is to be feared that Lieut. Wissmann will have to proceed through a practically hostile country all the way to Wadelai. It is a pity that in the matter of Emin Pasha, which interests all Europe, Germany and England could not work hand in hand.

THE new American Geographical Society recently founded at Washington, and including the most eminent geologists and geographers of the United States, has already held several meetings, and begun work in earnest. It has been resolved that the Society will undertake the task of bringing out a new physical atlas of the United States, and for this purpose it has appointed a committee of specialists to proceed with the undertaking.

IT is to be regretted that Dr. Meyer's Expedition to Kilimanjaro has met with opposition in traversing Usambara, and has been compelled to return to the coast. Dr. Meyer, who was accompanied by Lenz's former companion, Dr. Baumann, intended to make a thorough survey of the whole region around Kilimanjaro, which, it will be remembered, he recently scaled to within a few hundred feet of the summit. The Chief Semboja, who is reported to have attacked the Expedition, has hitherto been on friendly terms with the whites. He is a great friend of the missionaries of the Church Missionary Society, and Mr. H. H. Johnston, in his book on Kilimanjaro, speaks in highly favourable terms of him, and was indeed indebted to him for many friendly services. It is to be feared, therefore, that the Germans have shown some want of tact in dealing with the Usambara people. It is to be hoped that Dr. Meyer may be able to resume his journey, and carry out the objects of his expedition.

THE last number of the *Izvestia* of the Russian Geographical Society will be welcome to geographers, as it contains a chapter from the work of Przevalsky, now in print, about his fourth journey to Central Asia. All discoveries made during that journey are summed up in this chapter, and the relations of the mountain ridges, mapped by the Russian traveller to other hilly tracts, formerly known, or explored by Mr. Carey, are shown. We hope that this chapter, the chief one of the whole work, will soon be translated into English. After giving a general sketch of the Kuen-lun Mountains, M. Przevalsky describes his journey along the Zaisan-saitu River, the ridges of Tsaidam, "Columbus" and "Moscow," the Lake Unfreezing, Przevalsky's ridge, and the "Windy Valley," which offers an advantageous route to China ; as also the return journey, the excursion to the Khatyn-zan River, the passage across the Altyn-tagh, and the return to Lake Lob-nor. The forty pages covered by the article are a rich mine of geographical information. The same number of the *Izvestia* contains an abstract from A. D. Carey's "Journey to East Turkistan," with a map.

THE remarkable facts communicated by M. Yadrintseff as to the drying up of lakes in Siberia have induced the Russian Geographical Society to take decisive steps for the exploration of the lakes of the Empire. A great number of copies of an instruction by Dr. Forel, of Lausanne, have been sent out to correspondents of the Society, as also a programme for collecting data on the subject, and it is hoped that in a year or two most valuable data will thus be gathered.

IN the last number of *Petermann's Mitteilungen*, Herr J. Menges raises once more the question of the possibility of utilizing the African elephant. Herr Menges points out that there is strong evidence that the elephant was made use of in ancient times in Africa, and asserts that no serious attempt has been made in modern times to subdue it to the uses of humanity. He maintains that it is quite as docile as the Indian elephant, and much stronger, and that if it could be really tamed and trained to work, it would be of immense utility in the opening up of Africa. But, unless some protection is accorded to the African elephant, Herr Menges believes that by the end of next century it will be quite extinct. We are therefore glad to notice that the British East African Company will take special means for the protection of the animal, and they might very well make some attempt to prove whether or not it is capable of being tamed.

NOTES ON METEORITES.[1]

III.

Identity of Origin of Meteorites, Luminous Meteors, and Falling Stars.

It is very fortunate for science that many of the meteorites so carefully preserved in our museums *have been seen to fall*. This being so we possess full accounts of the accompanying phenomena and effects.

These comprise the most vivid luminosity; visible and audible explosions, in some cases heard over thousands of square miles of country, and at times a long train in the sky indicating the meteor path, which sometimes remains visible for hours.

Now precisely similar effects have been noted when nothing has reached the earth's surface; and in the thousands of records of the phenomena presented by luminous meteors, fire-balls, bolides, or shooting or falling stars as they have been variously called, we have the links which connect in the most complete manner the falls of actual irons and stones from heaven with the tiniest trail of a shooting or falling star, *une étoile qui file, qui file, et disparait*.

The heavy masses fall by virtue of their substance resisting the friction of the air, the grains are at once burnt up and fill the upper regions of the earth's atmosphere with meteoric dust.

As we have seen, the weights of meteorites which have actually fallen vary between many tons and a few ounces, the latter being, in all probability, fragments shattered by the explosion. In the case of some shooting-stars, the actual weight involved has been estimated by Prof. Herschel as low as *two grains*, not one out of twenty estimated by him exceeding a pound.

It may appear impossible that such atoms should produce the brilliant effects observed, but Prof. Herschel has calculated that a single grain moving at the rate of 30 miles a second represents a dynamical energy of 55,675 foot-pounds. This energy is converted by the resistance of our grosser air into heat, as the motion of a projectile is converted into heat by its impact on the target;[2] and hence the combustion of the matter of the meteorite, and perhaps even the incandescence of the air through which it rushes with such lightning velocity. This luminosity commences often at a height of 80 miles, and sometimes even higher, in regions where the atmosphere must be excessively rare.

Could these little bodies pierce our envelope as readily as do their larger cousins, the meteoric stones and meteoric irons, we should certainly have the advantage of placing them in our museums; but, on the other hand, the bombardment—the *feu-de ciel*—might be one to which the *feu-d'enfer* of all terrestrial artillery would be, in the gross total of results, as mere child's play.

But the identity of such phenomena as these is by no means the only line of evidence demonstrating the connection now in question.

Proof from the Chemistry of Fire-balls.

The spectral appearances observed with meteors, fire-balls, and shooting-stars, which explode and produce luminous effects, are entirely in harmony with those observations on the spectra of meteorites to which I have referred.

The observations, so far as they have gone, have given decided indications of magnesium, sodium, lithium, potassium, and of the carbon flutings seen in comets.

Prof. Herschel and Herr Konkoly have both noticed that in the generality of cases the lines of magnesium (one of the constituents of the olivine) show themselves first in the ordinary meteor or falling star, and the beautiful green light which is so often associated with these falling bodies is due to the incandescence of the vapour of magnesium.

The following quotations from Konkoly and Prof. Herschel are among the authorities which may be cited for the above statement:—

"On August 12, 13, and 14 I observed a number of meteors with the spectroscope; amongst others, on the 12th, a yellow fire-ball with a fine train, which came directly from the Perseid radiant. In the head of this meteor the lines of lithium were clearly seen by the side of the sodium line. On August 13, at 10h. 46m. 10s., I observed in the north-east a magnificent fire-ball of emerald-green colour, as bright as Jupiter, with a very slow motion. The nucleus at the first moment only showed a very bright continuous spectrum with the sodium line; but a second after I perceived the magnesium line, and I think I am not mistaken in saying those of copper also. Besides that, the spectrum showed two very faint red lines."[1]

"A few of the green 'Leonid' streaks were noticed in November (1886) to be, to all appearances, monochromatic, or quite undispersed by vision through the refracting prisms; from which we may at least very probably infer (by later discoveries with the meteor-spectroscope) that the prominent green line of magnesium forms the principal constituent element of their greenish light."[2]

Again, later on in the same letter, Prof. Herschel mentions Konkoly's observation of the bright *b* line of magnesium, in addition to the yellow sodium line, in a meteor on July 26, 1873. I again quote from Prof. Herschel:—

"On the morning of October 13 in the same year, Herr von Konkoly again observed with Browning's meteor-spectroscope the long-enduring streak of a large fire-ball, which was visible to the north-east of O'Gyalla. It exhibited the yellow sodium line and the green line of magnesium very finely, besides other spectral lines in the red and green. Examining these latter lines closely with a star-spectroscope attached to an equatorial telescope, Herr von Konkoly succeeded in identifying them by direct comparison with the lines in an electric Geissler-tube of marsh-gas. They were visible in the star-spectroscope for eleven minutes, after which the sodium and magnesium lines still continued to be very brightly observable through the meteor spectroscope."[3]

Another series of observations[4] gives continuous spectra for the nucleus, and two trains with sodium, and a third with sodium and a predominant green band, which was doubtless *b* of magnesium, the meteor itself being of emerald-green colour.

In cases where the temperature has been higher, the bright line spectrum of iron has been associated with the bright lines of magnesium in the spectrum of the falling star, so that the two substances which are among the chief constituents of stones and irons—precisely the two substances which we should expect to find—are actually those which have been observed.

The two lines which Konkoly supposes are probably due to copper will, I expect, be found to be iron lines when other observations are made of the spectra of meteors.

These spectral appearances are naturally associated with colours, and again we find that the colours of the trail, when meteorites have fallen, closely resemble those observed when no fall has been observed.

Green is a tolerably common colour, especially in slow-moving fire-balls about equal to Venus in lustre. These generally leave a short trail of red sparks.

About 10 per cent. of all shooting-stars show a distinct colour, the most usual being orange or red, the slowly-moving ones generally being red. The larger ones, or those with the longest trails, often turn from orange to bluish white, like burning magnesium. Sometimes the change is very sudden and startling.[5]

A purple or mauve tint, like that given by copper, is sometimes seen.

Proof from the Aurora.

When we come to consider the number of meteorites which fall upon the earth daily we shall find that it is enormous; and this being so, if we can trace this dust in the air, or after it has fallen, or both, if chemical examination shows it to be identical with that of meteorites, we shall be supplied with another argument which can be used in support of the fact that the bodies which produce the dust are meteoric in their origin.

One must suppose that these meteors in their passage through the air break into numerous fragments, that incandescent particles of their constituents, including nickel, iron, manganese, and the various silicates of iron, are thrown off, and that these or the products of their combustion eventually fall to the surface as almost impalpable dust, among which must be magnetic oxide of iron more or less completely fused. The luminous trains of falling stars are probably due to the combustion of these innumerable particles, resembling the sparks which fly from a ribbon of iron burnt in oxygen, or the particles of the same metal

[1] Continued from p. 458.

[2] The particles of iron in a large projectile, after impact, which is accompanied by a flash of light, are usually brought to a dark blue colour, which would correspond to about 555° F., but the momentary heat imparted is certainly greater than this.

[1] Konkoly, *Observatory*, vol. iii. 157.

[2] Herschel, letter to Nature, vol. xxiv. p. 507.

[3] *Ibid.* See also *Astr. Nach.*, No. 2014.

[4] *Monthly Notices*, vol. xxxiii. p. 575.

[5] Corder, *Monthly Notices*, vol. xl. p. 133

thrown off when striking a flint. It is known that such particles in burning take a spherical form, and are surrounded by a layer of black magnetic oxide.

How are we to trace this dust in the air? It is well known that at times the air is electrically illuminated, not only by the flashes of lightning which pass along its lower levels, but by so-called "auroral" displays in its higher reaches.

It is now many years since the idea was first thrown out that the aurora was in some way connected with shoooting-stars.

M. Zenger has prepared a catalogue of auroræ observed from 1800 to 1877, in which he shows an apparent connection between the brightest displays and the appearance of large numbers of shooting-stars.

M. Denza noted the same connection on November 27, 1872, and remarked that he had noticed it before.

Admiral Wrangel, as quoted by Humboldt, observed that in the auroras so constantly seen on the Siberian coast, the passage of a meteor never failed to extend the luminosity to parts of the sky previously dark.[1]

It is clear that in such a case as this the spectroscope is the only chemical aid applicable, and it has long been recognized that the spectrum observed is *not* the spectrum of the constituents of the atmosphere, as we can study it in our laboratories.

The spectrum, however, strictly resembles that seen in the "glows," to which reference has been made; if the factors present in both cases are meteor dust, low pressure, and feeble electric currents, the resulting phenomena should not be dissimilar.

The results of recent inquiries certainly justify us, therefore, in concluding that the upper reaches of the atmosphere contain particles giving us the spectra of magnesium, manganese, iron, and carbon.

The natural origin is the dust of those bodies which are continually entering those regions, and hence the proof afforded by the spectroscopic observation of shooting-stars, that they are identical in chemical composition with meteorites, is strengthened by these auroral observations, while, on the other hand, the origin of the auroral spectrum is placed beyond all doubt.

Proof from the Fallen Dust.

It is universally recognized that the atmosphere holds in suspension an immense number of very minute particles of organic and inorganic origin. These must be either dust taken up by aërial currents from the ground—the result of volcanic action—or extra-terrestrial bodies. Many scientific men, among whom we may mention Ehrenberg, Daubrée, Reichenbach, Nordenskjöld, Tissandier, Murray, and Renard, have studied this problem. Dust collected in various places at different times has been examined with a view of determining whether its origin was meteoric. In many cases, in which chiefly definite iron chondroi have been observed, the evidence has seemed very strong in favour of the view.

It is at once obvious that the detection of such dust which falls on the general surface of the land is hopeless, and that that which is collected on snow in inhabited countries containing foundries and the like is doubtful.

But a considerable advance of this question has recently been made in studying the deep-sea deposits collected by the *Challenger* Expedition. Messrs. Murray and Renard,[1] in giving the results of their researches, point out that at the greatest depths of the ocean furthest from land, the sea bottom is very different from that nearer the coast lines.

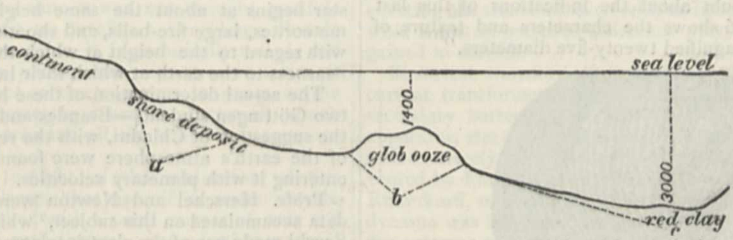

FIG. 3.—Section of ocean showing red clays at depths of 3000 fathoms (18,000 feet).

Under these necessary conditions of exceeding slow deposition and absence from ordinary sources of contamination, it is clear that the problem can be attacked under the best conditions.

We read:—"The considerable distance from land at which we find cosmic particles in greatest abundance in deep-sea deposits, eliminates at once objections which might be raised with respect to metallic particles found in the neighbourhood of inhabited countries. On the other hand, the form and character of the spherules of extra-terrestrial origin are essentially different from those collected near manufacturing centres. These magnetic spherules have never elongated necks or a cracked surface, like those derived from furnaces, with which we have carefully compared them. Neither are the magnetic spherules with a metallic centre comparable either in their form or structure to those particles of native iron which have been described in the eruptive rocks, especially in the basaltic rocks of the north of Ireland, of Iceland, &c."

Messrs. Murray and Renard then state on what they rely in support of their view that many of the particles thus obtained from great depths are of cosmic origin:—

"If we plunge a magnet into an oceanic deposit, especially a red clay from the central parts of the Pacific, we extract particles, some of which are magnetite from volcanic rocks, and to which vitreous matters are often attached; others again are quite isolated, and differ in most of their properties from the former. The latter are generally round, measuring hardly 0·2 mm.; generally they are smaller, their surface is quite covered with a brilliant black coating, having all the properties of magnetic oxide of iron; often there may be noticed upon them cup-like depressions clearly marked. If we break down these spherules in an agate mortar, the brilliant black coating easily falls away, and reveals white or gray metallic malleable nuclei, which may be beaten out by the pestle into thin lamellæ. This metallic centre, when treated with an acidulated solution of sulphate of copper, immediately assumes a coppery coat, thus showing that it consists of native iron. But there are some malleable metallic nuclei extracted from the spherules which do not give this reaction, they do not take the copper coating. Chemical reaction shows that they contain cobalt and nickel; very probably they constitute an alloy of iron and these two metals, such as is often found in meteorites, and whose presence in large quantities hinders the production of the coppery coating on the iron. G. Rose has shown that this coating of black oxide of

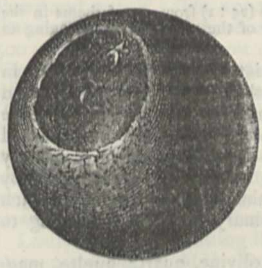

FIG. 4.

FIG. 5.

FIG. 4.—Black spherule with metallic nucleus (60 : 1). This spherule, covered with a coating of black shining magnetite, represents the most frequent shape. The depression here shown is often found at the surface of these spherules. From 2375 fathoms, South Pacific.

FIG. 5.—Black spherule with metallic nucleus (60 : 1). The black external coating of magnetic oxide has been broken away to show the metallic centre, represented by the clear part at the centre. From 3150 fathoms, Atlantic.

[1] "Cosmos" (Otté), vol. i. p. 114.

[1] "On the Microscopic Characters of Volcanic Ashes and Cosmic Dust and their Distribution in Deep-sea Deposits," Proc. R.S.E., and NATURE, vol. xxix. p. 585.

iron is found on the periphery of meteorites of native iron, and its presence is readily understood when we admit their cosmic origin. Indeed these meteoric particles of native iron, in their transit through the air, must undergo combustion, and, like small portions of iron from a smith's anvil, be transformed either entirely or at the surface only into magnetic oxide, and in this latter case the nucleus is protected from further oxidation by the coating which thus covers it."

We are next shown that these metallic chondroi occur with stony chondroi, so that if the interpretation of a cosmic origin for the magnetic spherules with a metallic centre be not considered established in a manner absolutely beyond question, it almost becomes so when we take into account their association with the silicate spherules, never found in rocks of a terrestrial origin. These are thus described:—

"Among the fragments attracted by the magnet in deep-sea deposits we distinguish granules slightly larger than the spherules with the shining black coating above described. These are yellowish-brown, with a bronze-like lustre, and under the microscope it is noticed that the surface, instead of being quite smooth, is grooved by thin lamellæ. In size they never exceed a millimetre, generally they are about 0·5 millimetre in diameter; they are never perfect spheres, as in the case of the black spherules with a metallic centre; and sometimes a depression more or less marked is to be observed in the periphery. When examined by the microscope, we observe that the lamellæ which compose them are applied the one against the other, and have a radial eccentric disposition. It is the leafy radial structure (*radialblättrig*), like that of the *chondres* of bronzite, which predominates in our preparations. We have observed much less rarely the serial structure of the *chondres* with olivine, and indeed there is some doubt about the indications of this last type of structure. Fig. 6 shows the characters and texture of one of these spherules magnified twenty-five diameters."

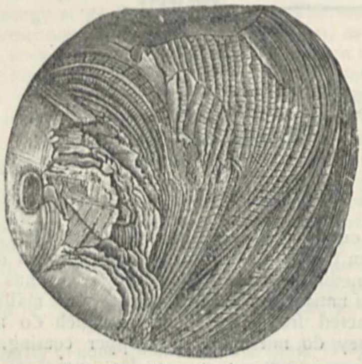

FIG. 6.—Chondros. Spherule of bronzite (25 : 1) from 3500 fathoms in the Central South Pacific, showing many of the peculiarities belonging to *chondres* of bronzite or enstatite.

It is worthy of remark that, associated with these chondroi in the red muds at the greatest depths in the ocean, are found manganese nodules in enormous numbers. If a section be made of one of these, a number of concentric layers will be observed arranged around a central nucleus—the same as in a urinary calculus. When the peroxide of manganese is removed by strong hydrochloric acid, there remains a clayey skeleton which still more strongly resembles a urinary calculus, according to Mr. Murray.

This skeleton contains crystals of olivine, quartz, augite, magnetite, or any other materials which were contained in the clay from which the nodule was taken. In the process of its deposition around a nucleus, the peroxide of manganese has inclosed and incorporated in the nodule the clay and crystals and other materials in which the nucleus was embedded. The clayey skeleton thus varies with the clay or ooze in which it was formed. Those from a fine clay usually adhere well together; those from a globigerina ooze have an areolar appearance; those from a clay with many fine sandy particles usually fall to pieces. Mr. Murray attributes the origin of these nodules entirely to the decomposition of volcanic rocks:—

"Wherever we have pumice containing much magnetite, olivine, augite, or hornblende, and these apparently undergoing decomposition and alteration, or where we have evidence of great showers of volcanic ash, there we find the manganese in greatest abundance. This correspondence between the distribution of the manganese and volcanic *débris* appears to me very significant of the origin of the former. I regard the manganese, as we find it, as one of the secondary products arising from the decomposition of volcanic minerals.

"Manganese is as frequent as iron in lavas, being usually associated with it, though in very much smaller amount. In magnetite and in some varieties of augite and hornblende the protoxide of iron is at times partially replaced by that of manganese.

"In the manganese of these minerals and in the carbonic acid and oxygen of ocean waters we have the requisite conditions for the decomposition of the minerals, the solution of the manganese, and its subsequent deposition as a peroxide."[1]

These nodules have been examined in the same way as the meteoric dust. Naturally the chief manganese fluting (the chief auroral line) has been seen.

The question arises, therefore, whether the origin of these deep-sea concretionary deposits of iron and manganese, which are unrepresented in any deep-sea geological deposit, may not be in part, even if in small part, meteoritic, and represent, like the chondroi, another form of fallen dust.

Proof from Similar Velocities.

Again, the meteorites, as we have seen, enter our atmosphere with very different velocities. The same thing happens with falling stars, which on this account have been divided into three classes as follows:—

Class I. Swift, streak-leaving meteors.
II. Slow, with trains of sparks.
III. Small, quick, short-pathed, sometimes with streaks.

It has also been determined that the luminous effect which is common to the fall of a meteorite or the appearance of a shooting-star begins at about the same height. In fact, we have in meteorites, large fire-balls, and shooting-stars, a progression both with regard to the height at which they become visible and the nearness to the earth at which their luminosity is extinguished.

The actual determination of these heights was commenced by two Göttingen students—Brandes and Benzenberg—in 1798, at the suggestion of Chladni, with the result that the upper reaches of the earth's atmosphere were found to be pierced by bodies entering it with planetary velocities.

Profs. Herschel and Newton were the first to discuss the data accumulated on this subject,[2] while, as early as 1864, Father Secchi made use of the electric telegraph in securing simultaneous observations.[3] The results of these combined inquiries may be thus shown in the case of shooting-stars:—

	Beginning. Height in miles.	End. Height in miles.	Authority.
Europe and America, 1798–1863	70·1	54·2	H.
	73·5	50·6	N.
Italy, 1864	74·6	49·7	S.
Average	72·7	51·5	

In Herschel's values fire-balls are excluded, and hence the limits are narrower.[4] Fire-balls often arrive within 20 miles of the earth's surface, and then the concussion is of nearly the same intensity whether stones fall or not.

Such determinations as these, when the observations can be depended upon, can be made with the greatest nicety and by graphical methods. One of the earliest employed—a description of which will give a fair idea of the investigation—is due to Colonel Laussedat.[5]

The observations stating the path of each meteor among the stars having been obtained, a 12-inch celestial globe is "rectified" in the usual manner for the place and time. In this way we get first the azimuth and altitude of the beginning and end of each trail. This is done for each place at which the same meteor is observed.

The results are then plotted on a large-scale map, on which the altitudes and longitudes of the places of observation and the distances between them can be determined. The scale of the map permits the height of the intersection of the lines of sight to be at once found, and the agreement or disagreement of the observations can be noticed, thus allowing the worst observations to be rejected.

[1] Murray, NATURE, vol. xv. p. 340.
[2] Herschel, B.A. Report, 1863, p. 328; Newton, *Silliman's Journal*, 2nd series, vol xxxvii., July 1864.
[3] *Bull. Meteor.*, vol. iii. p. 67.
[4] *Monthly Notices*, vol. xxv. p. 159.
[5] *Comptes rendus*, vol. lviii. p. 1100, 1864.

By taking such observations as these in different places it is possible not only to determine the height at which they enter but the velocity with which they pass through the upper regions of the air, even supposing they do not eventually get to the bottom. The lowest velocity determined up to the present time is something like 2 miles per second; the maximum is something like 50 miles a second; but we may say that the average rate of movement is 30 miles a second, which is about 150 times faster than a shell leaving one of our most powerful guns.

J. NORMAN LOCKYER.

(To be continued.)

THE ELECTRIC TRANSMISSION OF POWER.[1]

II.

THE next point to consider is the loss of power on the road between the dynamo at the one end and the motor at the other. This problem was perhaps seriously attacked for the first time in the discussion of a paper read by Messrs. Higgs and Brittle at the Institution of Civil Engineers in 1878, and that problem was considered in some detail theoretically and experimentally at the lecture I gave during the meeting of the British Association in Sheffield in the following year. It was then shown that, since the power developed by the generator and motor depended on the product of the current into the electric pressure, while the loss when power was transmitted through a given wire depended on the square of the current and was independent of the electric pressure, the economical transmission of power by electricity on a large scale depended on the use of a very large electric pressure and a small current, just as the economic transmission of much power by water depended on the use of a very large water pressure and a small flow of water. At that time it was not thought possible to construct a small dynamo to develop a very large electric pressure, or potential difference as it is technically called, and therefore it was proposed to join up many dynamos in series at the one end and many lamps or electromotors in series at the other, and to transmit the power by a very small current, which passed through all the dynamos and all the lamps in succession, one after the other.

You have an example to-night of the realization of this principle in the fifteen arc lamps that are all in series outside this Drill Hall, and are worked with a small current of only 6·8 amperes, as indicated in the wall diagram; and a further example in the thirty arc lamps at the Bath Flower Show, which are also all worked in series with the small current passing through them; but it is known now how to produce a large potential difference with a single dynamo, so that a single Thomson-Houston dynamo belonging to Messrs. Laing, Wharton, and Down supplies the current for each of the two circuits.

The electric pressure, or potential difference, between the terminals of any arc lamp is not high, but it is between the main wires near the dynamo as well as between these wires and the ground. How far does this lead to the risk of sparks or unpleasant shocks? That is a point that can be looked at in a variety of ways. First, there is the American view of the matter, which consists in pointing out to people exactly what the danger is, if there be any, and training them to look out for themselves: let ordinary railway trains, say the Americans, run through the streets, and let horses learn to respect the warning bell. Next, there is the semi-paternal English system, which cripples all attempts at street mechanical locomotion, because we are conservative in our use of horses, and horses are conservative in their way of looking at horseless tramcars. Lastly, there is the foreign paternal system, which, carried to its limit, would prohibit the eating of dinners because some people have at some time choked themselves, and would render going to bed a penal offence because it is in bed that most people have died.

We laugh a good deal at the rough-and-ready manner adopted on the other side of the Atlantic. The Americans, no doubt, are very ignorant of the difficulties that properly-minded people would meet with, but it is a blissful ignorance where it is folly to be wise. Every English electrician who has travelled in America comes back fully impressed with their enterprise and their happy-go-lucky success. They have twenty-two electric tramways, carrying some 4,000,000 passengers annually, to our four electric tramways at Portrush, Blackpool, Brighton, and Bessbrook. Why, New York city alone, Mr. Rechenzaun tells me, possesses 300 miles of ordinary tramway track, and Philadelphia 430 miles, so there is more tramway line in these two cities than in the whole of the United Kingdom put together. Now there would be no difficulty in proving, to anyone unfamiliar with railway travelling, that to go at 50 miles an hour round a curve with only a bit of iron between him and eternity would be far too risky to be even contemplated. And yet we do go in express trains, and even 80 miles an hour is beginning to be considered not to put too great a demand on the funds of life insurance companies. The American plan of basing a conclusion on experience rather than on anticipations is not a bad one; and if we follow that plan, then, taking into account that there are 75,000 arc lights alight every night on the Thomson-Houston high-potential circuits throughout the world, and the comparatively small number of people that have suffered in consequence (not a single person, I am assured, outside the companies' staffs) we are compelled to conclude that high potential now is what 30 miles an hour was half a century ago—uncanny rather than dangerous.

But it is possible to use a very large potential difference between the main wires by means of which the electric power is economically conveyed a considerable distance, and transformed into a very small potential difference in the houses where it is utilized. An electric transformer is equivalent to a lever, or wheel and axle, or any other of the so-called mechanical powers. You know that a large weight moving through a small distance can raise a small weight through a large distance; there is no gain in the amount of work, but only a transformation of the way in which the work is done. A large weight moving through a small distance is analogous with a high potential difference and a small current, while a small weight moving through a large distance is analogous with a small potential difference, and a large current, and an electric transformer is for the purpose of effecting the transformation with as little loss as possible, so that what is lost in potential difference may, as far as possible, be all gained in current.

Electrical transformation may be effected by (1) alternate current transformers, (2) motor-dynamos, (3) accumulators, or secondary batteries, (4) direct-current transformers. Of these apparatus, the eldest by far is the alternate-current transformer, as it is merely the development of the classical apparatus invented by Faraday in 1831, and familiar to many of you as the Ruhmkorff, or induction-coil. A combination of a motor and dynamo was suggested by Gramme in 1874. Accumulators are the outcome of Planté's work, while direct-current transformers are quite modern, and not yet out of the experimental stage.

After studying the literature on this subject, it appears, as far as I have been able to judge, that the first definite proposal to use a high potential difference in the street mains, and transform down to a low potential difference in the houses, was made in the lecture given by me at the meeting of the British Association in Sheffield in 1879, on which occasion I explained and showed in action the motor-dynamo principle suggested by Prof. Perry and myself. The apparatus on the platform is not unlike that shown on the former occasion: an Immisch motor working at 500 volts, and with a current of 6·8 amperes, is geared direct to a Victoria Brush dynamo giving five times that current, and we will now use this larger current to produce an electric fire. [Experiment shown.] Messrs. Paris and Scott have combined the motor and dynamo into one machine, which they have kindly lent me, and by means of which we are now transforming about 700 volts and 6·8 amperes into 100 volts and about 40 amperes used to light that group of sunbeam incandescent lamps or work these motors. [Experiment shown.]

Lastly, here is a working illustration of the double transformation proposed by MM. Deprez and Carpentier in 1881, by means of which—while the potential difference between the mains may be 2000 or 10,000 volts, if you like—not merely is the potential difference in the house so low that you could hardly feel anything if you touched the wires, but, in addition, there is the same security against shocks in the dynamo-room. This alternate-current machine is producing about 50 volts, which is transformed up to 2000 volts by means of this transformer. At the other end of the platform, by means of a similar transformer, the 2000 volts is transformed down again to 50 volts, employed to light that cluster of low-voltage incandescent lamps. [Experiment shown.] For the use of this apparatus I am indebted to the kindness of the Anglo-American Brush Company.

In this experiment there is, as a matter of fact, still more transformation than that I have yet mentioned, because, whereas in actual practice the alternate-current dynamo, as well as the small dynamo used to produce the current for magnetizing the electro-magnets in the alternate-current dynamo, would be worked by

[1] Lecture delivered by Prof. Ayrton, F.R.S., at the Drill Hall, Bath, on Friday, September 7, 1888. Continued from p. 511.

a steam, gas, or water engine, I am working them both by electromotors, since a steam-engine or a water-wheel would be an unsuitable occupant of the Drill Hall. Practically, then, a steam-engine on the land belonging to the Midland Railway Company, on the other side of the Lower Bristol Road, is driving a Thomson-Houston dynamo; this is sending a small current working these high-voltage constant-current Immisch motors. The motors being geared with low-voltage dynamos the potential difference is transformed down, the first alternate-current transformer transforms it up again, and the second alternate-current transformer transforms it down again, so that there are in fact three transformations taking place in this experiment on the platform before you. For the benefit of the electricians present, I may mention that the two motors are running in series, and that their speed is kept constant by means of a centrifugal governor which automatically varies the number of the convolutions of the field magnet that are being utilized at any moment. In fact, since the dynamo maintains the current constant that is passing through each motor, the function of the governor may be regarded as that of proportioning the potential difference maintained at the terminals of either motor to the load on the motor at any moment.

A vast district in London, extending from Regent's Park on the north to the Thames on the south, from the Law Courts on the east to Hyde Park on the west, has over 20,000 incandescent lamps scattered over it all worked from the Grosvenor Gallery in Bond Street by means of alternate-current transformers which convert the 2000 volts maintained between the street mains into 100 volts in the houses, and this London Electric Supply Company have arranged for a vast extension of this system to be worked from Deptford.

In America, alternate-current transformers are, due to the remarkable enterprise of Mr. Westinghouse, used to light 120,000 incandescent lamps in sixty-eight towns. In fact the electric lighting of a whole town from a central station begins to excite less astonishment than the electric lighting of a single house did ten years ago.

The efficiency of a well-made alternate-current transformer is very high, being no less than 96·2 per cent. when the transformer is doing its full work, and 89·5 per cent. when it is doing one-quarter of its full work, according to the experiments made by our students. It certainly does seem most remarkable, and it reflects the highest praise on the constructors of electrical machinery, that motive power can be converted into electrical power, electrical power at low pressure into electrical power at high pressure, or electrical power at high pressure into electrical power at low pressure, or, lastly, electrical power into motive power, in each case with an efficiency of not less than 94 per cent.

As a further illustration of the commercial importance of this electric transformation I will show you some experiments on electric welding, one of the latest developments in electrical engineering. To weld a bar of iron one square inch in section requires a gigantic current of some 13,000 amperes. To convey this current even a few yards would be attended with a great waste of power; consequently, while an enormous current is passed through the iron to be welded, only a comparatively small current is transmitted along the circuit from the dynamo to the welding apparatus. Mr. Fish, the representative of Prof. Elihu Thomson, of America, to whom this apparatus is due, will be so kind as to first show us the welding together of two bars of square tool steel, the edge of each bar being ¾ of an inch, and the operation is, as you see, entirely completed in some fifteen seconds. For this experiment an alternate current of 20 amperes will be produced by the dynamo at the other side of the Lower Bristol Road, and this current will be converted by the transformer on the platform into one of 9000 amperes, large enough for 12,000 of these incandescent lamps if they were placed in parallel and the current divided among them. He will next try welding some thicker bars, and lastly he proposes welding together two pieces of aluminium which it is extremely difficult, if not impossible, to weld in any other way. The bars, as you see, are in each case pressed together end on, and, in consequence of the electric resistance of the very small gap between the bars being much higher than that of the bars themselves, the current makes the ends of the bars plastic long before it even warms the whole bar, so that I can, as you see, hold the bar at a distance of three or four inches from where the weld has been made without experiencing any marked sense of warmth. The heat is, in fact, applied exactly where we require it, the temperature can be adjusted with the greatest nicety so as not to burn the steel, and the softening of the bar is effected throughout its entire cross-section. Hence a very good weld indeed can be made by end pressure. We have to thank Mr. Fish, not merely for showing us these most interesting experiments on electric welding, but for supplying the electric power for many of the experiments I have been showing you, and for the electric lighting of the Drill Hall.

To Mr. Snell, the representative of Mr. Immisch, our best thanks are due for his having devoted several days in arranging the two high-voltage, constant-current motors, to drive the dynamos with that constancy of speed which you observe. This ingenious telpher model, to which I shall refer presently, is the handiwork of Mr. Bourne, and considering that it has had to be hastily taken to pieces, and hastily put together again, it is surprising that it works as well as it does. An ordinary watch is a very trustworthy, steady-going machine, but if one had to take it to pieces hastily, and as hastily to put it together again, one might expect it to lose. Indeed, if you or I had to do it, we should not be surprised if it did not go at all, and so was only right twice every twenty-four hours.

For the arrangements of the models and the smaller experiments, as well as for the admirable execution of many of the diagrams, our best thanks are due to Mr. Raine.

Did time allow I should like to describe to you to what perfection the system of economical distribution with accumulators, originally proposed by Sir William Thomson in 1881 and shown in its very simplest form in the wall diagram, has been brought by Mr. King, the engineer to the Electrical Power Storage Company; how the cells when they are fully charged are automatically disconnected from the charging circuit, and electrically connected with the discharging circuit; how the electric pressure on the discharging or house mains is automatically kept constant, so that the brightness of the lamps is unaffected by the number turned on; and how cells that are too energetic have their ardour automatically handicapped, and not allowed to give more current than is being supplied by the less active ones.

During the last few months fierce has been the battle raging among the electricians, the war-cry being "alternate-current transformers *versus* accumulators," while the lookers-on, with that better view of the contest that they are proverbially said to possess, have decided that the battle is a drawn one. Neither system is the better under all circumstances: if the district to be lighted be a very scattered one, use alternate-current transformers by all means; but if the houses to be lighted are clustered together at a distance from the supply of power, then the storing property possessed by accumulators, which enables the supply of electric power to far exceed the capacity of the dynamos and engines in the busiest part of the twenty-four hours, will win the battle for accumulators. Any direct-current system of distribution such as is furnished by accumulators has also the very great advantage that it lends itself to the use of the very efficient electromotors which I have been using this evening. Alternate-current motors do exist, but they are still in the experimental stage, and are not yet articles of commerce.

Secondary batteries have caused much heart-burning, for their users, from the apparent fickleness of their complex chemical action, yet but imperfectly understood. But we have at length been taught what is good and what is bad treatment for them; and after years of brave persevering application on the part of the Electrical Power Storage Company, that forlorn hope the secondary battery has become one of the most useful tools of the electrical engineers; and secondary cells, some of which, thanks to the kindness of that Company, I am using here to-night to supply power for lamps and motors, may now be trusted to have a vigorous long life. That Company, I learn, undertake henceforth to keep their cells in order, when used for central station work, for 12½ per cent. per annum, and I understand that they have such confidence in them that they anticipate making no little money by incurring this insurance office responsibility. It is not, then, surprising that the Chelsea Supply Company have decided to use secondary batteries on a large scale for the economical distribution of light and power in their district.

Oliver Goldsmith said, more than a hundred years ago, in his "Life of Richard Nash, Esquire": "People of fashion at Bath, . . . when so disposed, attend lectures on the arts and sciences, which are frequently taught in a pretty superficial manner, so as not to tease the understanding, while they afford the imagination some amusement." I want not to be superficial, yet I must not tease your understanding, and so we will not lose ourselves in technical details. If, however, my remarks have led you to appreciate the vast economical importance of using very large electric pressures, and to grasp that, by substituting 2000 volts for 50 volts, when transmitting a certain amount of electric power, the current can be reduced to the one-fortieth part, and the waste of power, when transmitted along a given length of a given wire to the one-fortieth of the one-fortieth—that is, to the one sixteenth-hundredth part—your imagination will have been kindled as well as amused.

With a loss on the road of only 11 per cent., M. Deprez has, by using 6000 volts, transmitted 52 horse-power over a distance of about 37 miles through a copper wire only one-fifth of an inch in diameter. A piece of the actual conductor he employed I hold in my hand: the copper wire is coated with an insulated material, and then with a leaden tubing, so that the outside may be touched with perfect impunity, in spite of the high potential difference employed. M. Deprez's dynamo and motor were not nearly as efficient as he could make them now, so that his terminal losses were unnecessarily great, and the efficiency of the whole arrangement, wonderful as it was, was not so startling as it would otherwise have been. I have told you that the loss in dynamo and motor has actually been reduced to only 12½ per cent.; so that, if a dynamo and motor of this efficiency had been used by M. Deprez, the total loss in the whole transmission over 37 miles would have been under 25 per cent. Indeed, by using only 1250 volts, Mr. Brown has succeeded in transmitting 50 horse-power supplied by falling water at Kriegstetten to Solothun, in Switzerland, five miles away, with an entire loss in the dynamo, motor, and the five miles of going and returning wire of only 25 per cent.; so that three-quarters of the total power supplied by the water at Kriegstetten was actually delivered to machinery at Solothun, five miles away.

In less than twenty years, then, from Gramme's practical realization of Pacinotti's invention, we have power transmitted over considerable distances by electricity with only a total loss of 25 per cent., whereas the combined loss in an air-pump and air-motor or in a water-pump and water-motor is 40 per cent., irrespective of the additional loss by friction or leakage that occurs *en route*. We cannot help feeling that we are rapidly arriving at a new era, and that it will not merely be for the inauguration of the quick transmission of our bodies by steam, or the quick transmission of our thought by telegraph, but for the economical transmission of power by electricity, that the Victorian age will be remembered.

I showed you a little while ago an electric fire. Was that a mere toy, or had it any commercial importance? To burn coal, to work dynamos, and to use the electric current to light your houses and your streets is clean and commercial; to use the current to warm your rooms clean but wasteful, on account of the inefficiency of the steam-engine. But when the dynamos are turned by water power which would otherwise be wasted, the electric current may be economically used, not merely to give light, but also to give heat. And when the electric transmission of power becomes still more perfect than at present, even to burn coal at the pit's mouth where it is worth a shilling a ton may, in spite of the efficiency of the steam-engine being only one-tenth, be the most economical way of warming distant towns where coal would cost 20*s.* a ton. Think what that would mean!—no smoke, no dust, a reform effected commercially which the laws of the land on smoke prevention are powerless to bring about, a reform effected without the intervention of the State, and therefore dear to the hearts of Englishmen.

I am aware that this idea of burning coal at the pit's mouth and electrically transmitting its power has quite recently been stated to be commercially impracticable. But is that quite so certain?—for in 1878 it was stated that, although telephones might do very well for America, they certainly would never be introduced into Great Britain, as we had plenty of boys who were willing to act as messengers for a few shillings a week. The phonograph was also declared to be worked by a ventriloquist, and electric lighting on a large scale was proved to be too expensive a luxury to be ever carried out. Putting a Conservative drag on the wheels is a very good precaution to take when going down hill, but it is out of place in the up-hill work of progress.

To-day the electric current is used for countless purposes. Not only is it used to weld, but by putting the electric arc inside a closed crucible, smelting can be effected with a rapidity and ease quite unobtainable with the ordinary method of putting the fire outside the crucible. If one had pointed out a few years ago that it was as depressing scientifically to put a fire outside a crucible when you wanted to warm the inside, as Joey Ladle, the cellarman, found it depressing mentally "to take in the wine through the pores of the skin, instead of by the conwivial channel of the throttle," who would have believed that in 1888, a 500 horse-power dynamo would be actually employed to produce an electric arc inside a closed crucible in the manufacture of aluminium bronze.

But, of all the many commercial uses to which the electric current may be put, probably, after the electric light, electric traction has most public interest. The English are a commercial people, but they are also a humane people; and when, as in this case, their pockets and their feelings are alike touched, surely they will be Radicals in welcoming electric traction, whatever may be their political sentiments on other burning topics of the day. It is not a nice thing to feel that you are helping to reduce the life of a pair of poor tramway horses to three or four years: it would be a very nice thing to be carried in a tramcar for even a less fare than at present. Now, while it costs 6*d.* or 7*d.* to run a car one mile with horses, it only costs 3*d.* or 4*d.* to propel it electrically. Indeed, from the very minute details that have recently been published of the four months' expenses of electrically propelling thirty cars at 7½ miles an hour along a 12-miles tramway line in Richmond, Virginia, it would appear that the total cost—inclusive of coal, oil, water, engineers, firemen, electricians, mechanicians, dynamo and motor repairers, inspectors, linemen, cleaners, lighting, depreciation on engine, boiler, cars, dynamos, and line-work—has been only 1¼*d.* per car per mile. This is indeed a low price; let us hope that it is true. The tramway is, no doubt, particularly favourable for propelling cars on the *parallel* system (that is, the system in which the current produced by the dynamo is the sum of the currents going through all the motors on the cars) without a great waste of power being produced by a very large current having to be sent a very long distance, because the tramway track is very curved, and the dynamo is placed at the centre of the curve, with feeding-wires to convey the current from the dynamo to all parts of the track. But even in the case of a straight tramway line with a dynamo only at one end, it is quite possible to obtain the same high economy in working by employing a *large* potential difference and by sending a small current through all the trains in *series*, instead of running the trains in *parallel*, as is done on the Portrush, Blackpool, Brighton, and Bessbrook tramways.

This *series* system of propelling electric trains was oddly enough entirely ignored in all the discussions that have taken place this year at the Institution of Civil Engineers, and at the Institution of Mechanical Engineers, regarding the relative cost of working tramways by horses, by a moving rope, and by electricity; and yet this series system is actually at work in America, as you will see from an instantaneous photograph which I will now project on the screen, of a series electric tramway in Denver, Colorado; and a series electric tramway 12 miles long, on which forty cars are to be run, is in course of construction in Columbus, Ohio. The first track on which electric trams were run in *series* was the experimental *telpher line*, erected in Glynde in 1883 under the superintendence of the late Prof. Fleeming Jenkin, Prof. Perry, and myself, for the automatic electric transport of goods. A photograph of this actual line is now projected on the screen. The large wall diagram shows symbolically, in the crudest form, our plan of series working: the current follows a zigzag path through the contact pieces, and when a train enters any section the contact piece is automatically removed, and the current now passes through the motor on that train, instead of through the contact piece. The Series Electrical Traction Syndicate, whom we have to thank for the model *series* tramway on which the two cars are now running, are now developing our idea, but it has received its greater development in the States, where the Americans are employing it, instead of spending time proving, *a priori*, that the automatic contact arrangements could never work. Mental inertia, like mechanical inertia, may be defined in two ways. Inertia is the resistance to motion—that is the English definition: but inertia is also the resistance to stopping—that is the American definition.

In addition to the small waste of power, and consequent diminished cost of constructing the conductors that lead the current into and out of the passing trains, the series system has another very marked advantage. Some years ago we pointed out that when an electric train was running down hill, or when it was desired to stop the train, there was no necessity to apply a brake and waste the energy of the moving train in friction, because the electric motor could by turning a handle be converted into a dynamo, and the train could be slowed or stopped by its energy being given up to all the other trains running on the same railway, so that the trains going down hill helped the trains going up hill, the stopping trains helped the starting trains. At that time we suggested detailed methods for carrying out this economical mutual aid arrangement whether the trains were running on the parallel or on the series system. But there is this difference, that, whereas on the parallel system it is only when a train is running fairly fast that it can help other trains, the series system has the advantage that, when a motor is temporarily converted into a dynamo by the reversal of the connections of its stationary magnet, the slowing train can help all the other trains even to the very last rotation of its wheels. Brakes that save the power instead of wasting it are of purely English extraction, but their conception has recently come across the Atlantic with such a strong Yankee accent that it might pass for having been born and bred in the States.

Economy is one feature that gives electric traction the right to claim your attention; safety is another. This model telpher line worked on "the post head contact" system is so arranged that no two trains ever run into one another, for, in addition to each of the three trains being provided with an automatic governor which cuts off electric power from a train when that train is going too fast, the line is divided into five sections connected together electrically in such a way that as long as a train is on any section, A, no power is provided to the section B behind, so that if a train comes into section B, it cannot move on as long as the train in front is on section A. [Three trains shown running on a model telpher line with four automatic locks.] Whenever a train—it may be even a runaway electric locomotive—enters a blocked section, it finds all motive power withdrawn from it quite independently of the action of signalmen, guard, or engine-driver, even if either of the latter two men accompanied the train, which they do not in the case of telpherage: no fog, nor colour-blindness, nor different codes of signals on different lines, nor mistakes arising from the exhausted nervous condition of overworked signalmen, can with our system produce a collision. Human fallibility, in fact, is eliminated. While the ordinary system of blocking means merely giving an order to stop—and whether this is understood or intelligently carried out is only settled by the happening or non-happening of a subsequent collision—our automatic block acts as if the steam were automatically cut off; nay, it does more than this: it acts as if the fires were put out in an ordinary locomotive and all the coal taken away, since it is quite out of the power of the engine-driver to re-start the electric train until the one in front is at a safe distance ahead.

The photograph now seen on the screen shows the general appearance of the Glynde telpher line, which has recently been much extended in length by its owners, the Sussex Portland Cement Company; and a telpher line with automatic blocking on the broad principles I have described is about to be constructed between the East Pool tin-mine in Cornwall and the stamps. There will be four trains running, each consisting of thirty-three skeps containing three hundredweight each, so that the load carried by each train will be about five tons.

It may be interesting to mention that the last difficulty in telpherage, which consisted in getting a proper adhesion between the driving-wheels of the locomotive and the wire rope, has now been overcome. The history of telpher locomotives is the history of steam locomotives over again, except that we never tried to fit the electric locomotives with legs, as was proposed in the early days for steam locomotives. It is a tedious discouraging history, but it is so easy to be wise when criticizing the past, so difficult to be wise when prospecting the future. Gripping-wheels of all kinds, even the india-rubber tires used for the last three years, have all been abandoned in favour of simple, slightly loose, cheap iron tires, which wear for a very long time, and give a very perfect grip when the bar supporting the electromotor is so pivoted, pendulum-wise, to the framework of the locomotive that the weight of the motor no longer makes the locomotive jump in passing the posts, as it did until quite recently.

After several years of experimenting, we have in telpherage, I venture to think, at last a perfectly trustworthy, and at the same time a most economical, method of utilizing distant steam- or water-power to automatically transport our goods, and in time it may even be our people, over hills and valleys, without roads or bridges, and without interfering with the crops or the cattle, or the uses to which the land may be put over which the telpher trains pursue their snake-like way: we have, in fact, the luxury of ballooning, without its dangers.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 17.—M. Des Cloizeaux in the chair.—Complement to the theory of overfalls stretching right across the bed of a water-course (weirs, mill-races, and the like), by M. J. Boussinesq. In supplement to the theory worked out in the *Comptes rendus* of July 4, October 10 and 24, 1887, the author here deals with the discharge as influenced by the velocities of the currents at the overfall.—On M. Lévy's recent communication on the subject of Betti's theorem, by M. E. Cesaro. This theorem, which plays an essential part in Betti's "Teoria dell' Elasticità," is practically that of Green, which is capable of such manifold applications, and which M. Lévy has shown to admit of so many interesting corollaries in graphostatics. In the present paper M. Cesaro confines himself to proving that the formula of Laplace, giving the velocity of sound in rectilinear elastic mediums, is itself a consequence of Betti's fruitful theorem.—Compressibility of the gases, by M. E. H. Amagat.—On the chlorides of gallium, and on the value of the elements of the aluminium group, by MM. Nilsson and Otto Pettersson. Here are studied the two different chlorides Ga_2Cl_6 (or $GaCl_3$) and $GaCl_2$, as determined by M. Lecoq de Boisbaudran, the discoverer of gallium. The combinations are also given that are formed with chlorine by the elements of the third group of the natural system, whose chlorides have so far been studied. It is pointed out that aluminium and gallium displace three atoms, indium two, and thallium one of hydrogen of the hydrochloric gas. In this group, with the increase of the atomic weight the elements show an evident tendency to form several combinations with chlorine.—On ferrous chloride and the chlorides of chromium, by MM. Nilsson and Otto Pettersson. The preparation and properties are described of ferrous chloride, and of the two known chromium chlorides—the trichloride, $CrCl_3$, and the bichloride, $CrCl_2$.—Papers were communicated by M. René Chevrel on the great sympathetic nervous system of bony fishes; by M. Alexandre Vitzou on the incomplete intercrossing of the nerve-fibres in the optic chiasma of the dog; and by MM. Raphaël Dubois and Léo Vignon on the physiological action of para- and metaphenylene-diamine.

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