THURSDAY, MAY 17, 1883

THE FISHERIES EXHIBITION

"Do you know me, my lord?
Excellent well; you are a fishmonger."—*Hamlet.*

THE exhibition which was opened last Saturday by the Prince of Wales on behalf of Her Majesty the Queen is the latest of a series of such shows of matters relating to fish and fishing apparatus which was initiated by the French at Arcachon, other exhibitions having followed in subsequent years at Amsterdam, at Norwich, at Berlin, and at Edinburgh. Though in this country the accumulated knowledge and experience of scientific zoologists is not made use of either by the Government, or by local authorities, or by private capitalists in order to render our fisheries more productive, or to prevent the total destruction of some branches of them (except in the case of the salmon fisheries), yet the Fisheries Exhibition will have some interest for scientific men and for the readers of NATURE.

It is true that the present exhibition differs from its continental predecessors in the fact that it is a private undertaking entirely organised by practical men who would disclaim the title of "scientific" for themselves, and who have not largely availed themselves of the services of the professional zoologists of the country in carrying out their enterprise. Nevertheless the exhibition will have a scientific character and importance in consequence of the fact that almost without exception every foreign country which takes part in the exhibition is represented by distinguished zoologists, who have been delegated by the governments of the countries to which they belong, to take charge of and to organise the exhibition of such objects as in their judgment may best serve to illustrate the vast variety of matters of interest and instruction connected with their fisheries. From the republican United States of America, from democratic Norway, from Holland, from Sweden, and from Italy skilled zoologists have been sent by their respective Governments and are at this moment in London in order both to teach and to learn at the fisheries show.

It will be interesting to compare the results which these skilled officials and men of science can produce with those offered by the crowd of independent English exhibitors, manufacturers, fishery owners, fishmongers, and naturalists. Hitherto in the great fishery exhibitions England has been represented at a great disadvantage, for although the Governmental departments of fisheries control and inspection of foreign states have cordially responded to the invitation sent by the committee of the present London exhibition, yet on no occasion has the English Government assisted to place before the public in other countries any of the methods or products of English fishing. On the present occasion, though as hitherto the English Government has no official machinery for representing or dealing with British fisheries generally, and practically takes no part in the affair, yet in consequence of the activity of the large committee of gentlemen who have organised the exhibition, we shall no doubt see a much fuller representation of British fishing enterprise than at any former exhibition.

It is too soon at this moment, when many cases of objects are still unopened and sufficient time for a careful inspection of the exhibits has not elapsed, to offer any detailed remarks on the teaching to be derived from the fisheries show. On Saturday the special exhibit, which cannot be retained permanently during the whole period for which the show is open, was that of the fisherfolk themselves. Amongst the men the East Anglian herring fishers of Yarmouth and Lowestoft carried off the palm by their fine physique, intelligent faces, and sturdy bearing, wonderfully like to their brother Norsemen from the other side of the North Sea. These and the bright fearless faces of the Newhaven fisher-girls as they sat side by side with the strangely capped women from Boulogne and from the Dutch and Belgian coast, who good naturedly took part in the ceremony of Saturday last, were sufficient to demonstrate that whether British fisheries need or do not need to be improved and developed by that scientific supervision which is applied to the harvest of the sea on foreign shores, the race of men and women occupied in carrying on those fisheries bring to their business the fullest measure of intelligence and physical capacity. It is due to the courage, skill, and vigour of these fisherfolk that British fisheries continue to flourish, though their enterprise is unaided by the science of a Government department and their market is systematically injured by the devices of "middle-men."

Possibly the London Fisheries Exhibition of 1883 may have a result in regard to the British interests there represented similar to that which the Great Exhibition of 1851 effected in regard to the various art manufactures of the country. Just as the public demonstration of British inferiority in the matter of artistic workmanship led to the action of the Government in promoting a remedy in the foundation of schools of art and design, so the extraordinary contrast afforded by the British and Foreign exhibits on the present occasion in all that relates to a reasonable use of accurate knowledge (otherwise called science) in dealing with fish, oysters, lobsters, &c., may lead to an effort on the part of the constituted authority to imitate in some way the action of foreign governments (whether popular or paternal) in retaining the services of competent zoologists for the purpose of continually acquiring new knowledge in regard to fishes, and in particular of devising new ways of increasing and protecting the annual yield of fishes in the market.

It is a remarkable fact that for the purpose of dealing with questions and effecting practical objects connected with the economic aspects of the vegetable kingdom, the British Government supports the most efficiently organised botanical institution in the world. The Royal Gardens at Kew are the source of a ceaseless stream of scientific information and advice which is poured by every mail into all parts of the globe where our colonies extend, and it may be truly said that the pecuniary value of the scientific knowledge to British commercial enterprise, which has thus been furnished, is gigantic.

It does not admit of any question, that a parallel, though not in the first instance so vast a service, might be rendered to British industrial and commercial interests by a governmental zoological institution, to the scientific staff of which might be intrusted for study and control, not only matters relating to the sea- and river-fisheries of these islands, our oyster, lobster, and shell-fish fisheries, but also matters concerning the pearl fisheries of India, the sponge-fisheries of the Bahamas, and the possible coral fisheries of the Australian coast. Further, the duties which even among the self-helping inhabitants of the United States are assigned to a State entomologist, might here also be discharged. From the duly established officials of such a state zoological laboratory or institute, the Foreign Office and the Colonial Office could at once obtain full and decisive information enabling them to act intelligently in relation to the importation of the Phylloxera pest, whilst the Home Office might gain courage in the presence of the Colorado beetle. It seems strange that the creation of an official laboratory of economic zoology has been so long delayed.

We shall be able to judge in the case of the present exhibition whether the cooperation of scientific men would have rendered the English department more instructive than it is under the present conditions, as compared with the scientifically organised exhibits of foreign countries. The comparison of the official catalogue of the London Exhibition with that of the Berlin Exhibition will be important in the same direction. With regard to the essays for which the committee has offered prizes, it may at once be stated that unfortunately no steps have been taken to bring the questions concerning which treatises are desired under the notice of the persons most likely to be able to deal with them satisfactorily either in this country or abroad. A series of valuable reports might have been obtained and circulated in connection with the exhibition by a sufficiently public appeal to the zoological world made in due time. It may yet be not too late to take some steps in this matter.

SCIENCE AND ART

NO one will be surprised that Mr. Huxley took advantage of the opportunity afforded him at the Academy dinner to reply to some remarks made by Mr. Matthew Arnold on a like occasion two years ago. Mr. Arnold, we presume, does not claim to possess that amount of knowledge either of art or of science which would render him a prejudiced witness, and, being unprejudiced, he drew a terrible picture of the future of art, not only in this, but in all other countries, unless some very decided steps were taken. Time out of mind, according to Mr. Arnold, art and literature had divided the sweets and beauties of this world between them, but now, in these latter days, that terrible thing science—

"Monstrum horrendum, informe, ingens, cui lumen ademptum,"

was about to bar their future progress, and invade and destroy the fair kingdoms of thought and work gained from the unknown by the labours of both. Hence the necessity of an alliance offensive and defensive against the common enemy; hence the artist and the man of letters were to band themselves together to stamp this new hydra from out the land.

It was not to be expected that such a view as this would be allowed to pass unchallenged by Mr. Huxley. He declined to regard science as an invading and aggressive power, eager to banish all other pursuits from the universe. Putting Mr. Matthew Arnold's view in a more concrete form, he represented it as picturing science rising as a monster from out the troubled waters of the sea of modern thought, intent upon devouring the unprotected Andromeda of Art. For him Literature was Perseus equipped with the swift shoes of the ready writer, and the cap of invisibility of the editorial article, while the death-dealing quality of Medusa's head had a fitting representative in the sting of vituperation. Mr. Huxley's remarks dealt less with Andromeda than with Perseus, to whom he suggested the advisability of thinking twice before trying conclusions with the risen monster. He ended by showing how necessary Art and Science were to each other, how each was strong in the other's strength, and how they were never likely to be sundered, but were certain to twine round each other more closely, and to help each other more as time went on. Agreeing as we do altogether with Mr. Huxley, we think, however, that another view is worthy of consideration. For ourselves, although likening art to fair and chained Andromeda, we cannot admit that science is correctly represented in the form of the monster. Without further considering of whom or of what the monster may be typical, it seems to us perfectly certain that the Perseus of whom the Andromeda of Art stands so much in need is not Literature, but Science, because this Perseus alone can give the help and render the assistance which the maiden needs so sorely at the present moment.

Occasion has been before taken in these columns to point out how one of the greatest revivals of art in the history of the world was contemporaneous with the dawn of one of those sciences which must for ever lie at the base of much work in art: we refer to the science of anatomy; and when one looks round this year's Academy and compares the work based upon this branch of knowledge, the anatomy of form, with that connected with the other branch of knowledge which has to do with the anatomy of light and colour, one cannot but feel that the Andromeda of Art is being sacrificed indeed. Landscape painting has as close a connection with physical science as figure painting has with anatomy, and we cannot help thinking it is because physical science has not been sufficiently taught in our public schools, that our landscape painting is, if we are to judge by this year's pictures, not advancing, but almost retrograding. The man who finds anatomy too difficult for him and rushes into landscape soon discovers that there is something there which he has not learned, but which has to be learned ere he can achieve distinction; and like too many others he has to give up the battle ingloriously. Not for many years has there been such an absence of landscapes of the highest order as in the present Academy; and in order to show, on the one hand, how those artists who have some knowledge of the branches of science which bear upon their work in art have succeeded in filling their canvases with worthy representations of natural effects, and, on the other hand, how those who lacking this knowledge are only successful in producing misrepresentations and distortions of nature, we shall on a subsequent occasion give a series of notes upon those pictures which fall within the reach of our remarks. In some pictures the ignorance of one part of nature has been as great as if a portrait painter had painted a face in which the mouth was represented between the eyes and the nose, or again as if he had painted feet instead of hands.

There is one instance so much in point that we may at once refer to it. One artist, who shall be nameless, has attempted to grace his picture by introducing into it a rainbow. Now if the rainbow had been part of the human form it would have been studied, there would have been books about it, and the artist would have made it as much his own as the student of physical science, since some artists study anatomy as closely as does the man of medicine, but, because the rainbow happens to lie outside that branch of scientific knowledge which is generally supposed to be the only branch to which artists need turn their attention, the painter thinks that he may treat it anyhow. Thus we have had rainbows with the colours—which in nature are absolutely definite in their order and arrangement—painted in reverse order; again, we have had a rainbow, which must always appear to form part of a circle, painted in perspective; but the rainbow fancier of this year has almost transcended the want of observation shown by his predecessors. Possibly ignorant of the fact that all primary rainbows are alike; that the order of colours, from red through orange, yellow, green, blue, indigo, to violet, is dominated by a most rigid law, to which there is, and can be, no exception; the artist has chosen to paint his rainbow with the violet in the middle. This seems to indicate either such looseness of observation or such contempt for nature—and the painter may take his choice between these two alternatives—that we doubt whether side by side with either there can exist that sympathy with nature which must lie at the root of all good work in art. We shall show on a subsequent occasion that this picture is only typical of a good deal of artistic work, which must in the nature of things act like a discord, and put the eye and the heart of the painter out of tune.

Those branches of science to which we have to make reference in these columns have to do of course with the forms and colours of clouds and sky and natural objects generally, and the laws of reflection, and if an artist will paint suns and moons, then with those elementary astronomical principles which have to deal with the appearances of these bodies, and which are not beyond the comprehension of a child in the Fourth Standard of a public elementary school. It is not therefore imposing too much upon an artist that he should know these things, and it is not too much to suppose that one who paints work on which he wishes to build his fortune or his reputation as the case may be, should wish to appeal to a more or less cultured audience. At present, perhaps, it is only a select few who notice and deplore this want of harmony with nature which marks the productions of so many of our artists; but the love of physical science among the great mass of mankind grows stronger and more strong, and the circle of those who can discriminate between fact and fancy as displayed in the works which grace the walls of our picture galleries is daily becoming a wider one. We would therefore utter a word of warning to the artist who allows blunders to creep into his picture because he thinks nobody will find them out. Somebody is sure to find them out.

The opportunities which artists in following their profession have of studying nature in very varied moods enable them to see the actual phenomena, where *a priori* considerations leave a student who lacks such opportunities entirely in the dark. Several very interesting questions are raised by some of the pictures in this year's Academy, and the candid critic must acknowledge that many of them give much food for thought and suggestions for future inquiry and study on his own part.

THE TRANSIT INSTRUMENT

A Treatise on the Transit Instrument as Applied to the Determination of Time; for the Use of Country Gentlemen. By Latimer Clark, M.I.C.E., &c. (Published by the Author.)

IT is something new to have a book on the transit instrument for the use of country gentlemen. It is something still newer to find that book brought out by an eminent engineer. In fact we may regard the publication of such a book, under such conditions, as a sign of the times, and as an indication of the slow but sure way in which science, and even the methods of science, are interesting a gradually increasing number of our educated classes. Mr. Latimer Clark has done his work in a most admirable manner, and no country gentleman who wishes to know a little more than he does at present about the practical working of a most fascinating branch of science, could do better than invest, not only in the book, but in the very satisfactory and handy little instrument which Mr. Clark has been wise enough to produce side by side with it. This transit instrument to which we refer, and which can be obtained of Mr. Coppock of Bond Street, is an excellent one of its kind. It is cheap—costing only about 10*l.*—and it is simple. The many parts of the instrument which form necessary adjuncts to it when used in an observatory are of course suppressed, but nothing is wanting which is really of importance to that public which Mr. Latimer Clark wishes to educate in its use. The author is quite wise in the way he goes to work. We naturally have a description of the instrument, and reference to the way in which it can be most conveniently and satisfactorily employed, nor are those necessary adjustments omitted without which of course the simplest instrument would be of very little use. Full instructions are then given for putting it in position, and Mr. Clark's form of instrument has a cover, by means of which, when once placed in position, say, out on a stone pillar on a lawn with a good north and south line, it can be left out with very little chance of its taking any harm in all weathers. The actual taking of transits, both of the sun and stars, are then dealt with, and we should add here that the transit eyepiece is armed with a system of seven vertical wires, so that the means of several transits over the wires can be taken in the ordinary way. The only objection we have so far found to Mr. Latimer Clark's form, is that there are no means of adjustment for the verticality of the wires. We regard this as a point which should be looked to, in case our author should be fortunate enough to induce a great many people to employ this cheap and simple form.

The corrections for longitude and latitude are next given, and we are glad to see that the book deals with these matters in a way not only far from dry, but so as to introduce a considerable quantity of very useful astronomical knowledge put in a very simple and taking form. Thus, for instance, when Mr. Clark urges, and rightly urges, that the latitude of any particular house where it may be suggested to put up one of these instruments is more likely to be accurately determined by a reference to the Ordnance Map than in any other way, we are not only told that the Ordnance Map may be got for 1*s*., but the ignoramus is not even forgotten, and the way in which longitude is marked on the map is clearly stated. Mr. Clark it will be seen has spared no pains to make everything as clear as possible to everybody. Here, for instance, is what he says on counting time to a beginner in astronomical work:—

"In taking the time of transit by a watch some difficulty will at first be experienced, owing to the fact that watches tick various numbers of beats in a minute, but rarely any direct multiple of the second. Having carefully ascertained the number, the watch is placed to the ear at any even ten or twenty seconds, and the counting is continued by sound, thus: One and, one and, two and, two and, three and, three and, fifteen, fifteen, &c. In recording the transit, the minutes and seconds at the time of starting are noted, and the number of additional ticks is counted by ear, and these are afterwards converted into seconds and added. When a single observation is considered sufficiently accurate for practical purposes, it may be conveniently recorded by a chronograph, which is started in exact accordance with your clock a few minutes before it is required, and is stopped at the exact instant of transit. At night this dispenses with the necessity of referring to a lantern for time. These chronographs may now be obtained at a very moderate price, and when they beat quarter-seconds are very useful for counting, and for carrying the time of a clock or chronometer into the open air, and might be advantageously used on shipboard. The portable American clocks, which are sold everywhere for a few shillings, sometimes beat quarter-seconds, and serve well for counting time. They may be easily converted into rough chronographs by adding a stop movement in the form of a light spring pressing against the balance wheel; the second hands are, however, loose and inaccurate. A servant may be taught to record transits with the assistance of one of these chronographs very correctly.

"The writer, however, employs an arrangement which he considers still preferable. A rough pendulum about ten inches long is constructed out of a wire suspended by a flat spring or by a double loop of wire, and screwed at the bottom through a flat leaden plumb-bob; it is adjusted so as to beat half seconds correctly. A very small glass bead or button is suspended loosely at the lower end of the rod, and strikes against a thin metal plate at each oscillation; it is started by a trigger of wood or wire, supporting the plumb-bob at a definite angle, and insuring uniformity of swing. This is fixed on a board and hung within reach of the instrument. It may be used in several ways; the trigger may be pulled at the instant of transit, the first tick being counted as one, and the counting being continued while the observer rises from his position, and obtains at leisure a coincidence with a given second on his watch, the number of beats being converted into seconds and deducted from the time noted. The observation is noted thus, 46m. 28-21, and is afterwards converted into

$$\begin{array}{rrr} 8^{h} & 46^{m} & 28^{s} \\ & - & 10{\cdot}5 \\ \hline 8 & 46 & 17{\cdot}5 \end{array}$$

"Or the observer may start the pendulum just before the transit occurs, and having obtained a coincidence with his watch may continue to count by ear. The pendulum may, if preferred, be made to tick seconds by muffling one side of the metal strip with a piece of felt, and it may be removed after an observation, leaving the support and striking plate attached to the pillars of the transit. It oscillates on a rod or knife edge, and will continue in motion for two or three minutes."

As an appendix to the book we find transit tables giving the Greenwich mean time transit of the sun and certain stars for every day in the year, computed from the *Nautical Almanac*, and there is also a table for converting intervals of sidereal time into equivalent intervals of mean solar time. More recently the author has published a set of transit tables separately, and we would recommend those who use his book on the transit instrument to obtain them. To all who are not familiar with the use of the tables, clear and simple instructions are given, and we think they will prove of great use to those for whom they are intended.

Mr. Latimer Clark has certainly well deserved the thanks of all interested in astronomy, for the pains he has taken in thus endeavouring to popularise an instrument which, although it is the most important instrument used in astronomy for the determination of position, is at the same time one from which an immense amount of pleasure can be obtained by the merest tyro in science, whilst the great advantage of using such an instrument as this is, that no one can use it without rendering himself thoroughly familiar with some of the most important problems which lie at the root of any useful knowledge touching the stars, or the planet on which we dwell.

LETTERS TO THE EDITOR

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

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

Fossil Algæ

IN a review of Saporta's work on "Fossil Algæ" in NATURE (vol. xxvii. p. 514) there are certain opinions brought forward which ought not to be passed by without some remarks. At first it should be stated that Saporta, while still insisting upon the vegetable nature of his so-called "algæ," does not only defend his views about those doubtful bodies which have been the objects of my criticisms, but also "defends" true algæ, concerning which I have never expressed any doubt. Such are, for example, the Florideæ represented on the first three plates in his work, and by "defending" them he puts me in a somewhat false position, at least in the eyes of the readers who have not studied my work.[1] The real fact is that I have only questioned —and still do so—the vegetable nature of almost all those objects which Schimper in Zittel's "Handbuch der Paleontologie" comprises under the head of "*Algæ incertæ sedis.*" There is consequently some exaggeration in Saporta's statement of my opinion.

Now it is quite clear that fossil trails of animals must occur in most cases in relief on the under sides of the slabs, the tracks in such cases being impressions in the soft sand or mud, which have since been filled by sediment. And it is also quite natural that the trails of animals should especially be found where there are alternating beds of sandstones and shales.

Now it is a fact that the Bilobites, as well as Eophyton, *always* occur in this way, projecting as convex bodies on the under sides

[1] A. G. Nathorst, "Om spår af några evertebrerade djur arbderas paleontologiska betydelse." With a French *résumé*, "Mémoire sur quelques traces d'animaux sans vertèbres et de leur portée paléontologique." (*Svenska Vetenskaps Akademiens Handlingar*, Bd. xviii., No. 7. Stockholm: Norsted och Söner, 1880.)

of the sandstones. Both Dr. Dawson and Dr. Linnarsson therefore long ago expressed the opinion that the Bilobites of Sweden and America must have been trails of some animals. In order to explain this mode of occurrence so that it might not appear as proof against the vegetable nature of the bodies, Saporta takes refuge in a somewhat curious manner of fossilisation described and illustrated by woodcuts in the review referred to. As I feel sure that every one who has made himself acquainted with true modes of fossilisation will immediately be aware that the process adopted by Saporta is indeed most improbable, it will be superfluous to dwell any longer on that question. But even granted that the plants *sometimes* should occur in this way—which statement I, however, think must be due to some confusion as to the real facts—such an occurrence could never be regarded but as a very rare exception to the general rule. And it therefore does not explain why the Bilobites should *only* occur in this, for true plants, exceptional way, (on the under surfaces of the slabs), *never* as true fossils embedded *in* the rock. This mode of occurrence harmonises, on the other hand, perfectly with the view that the Bilobites are trails of some animals, while it *cannot* be explained on the supposition that they are true organic bodies.

One arrives precisely at the same conclusions on studying their external structure, which possesses pretty great analogies as well with the trails of Limulus, long ago described by Dawson, as with those of other Crustaceans, described by myself. It is true that Saporta lays great stress on some superficial markings which are to be seen on some of the French specimens; but those who have studied not only the French Bilobites, but also those from Sweden or America, will soon be aware that the markings referred to are quite accidental. It is indeed surprising that Saporta, while adopting my views concerning Crossochorda, does not see that the Bilobites are somewhat analogous forms, though much larger. There is consequently no reason why they should be regarded as other than the trails of Crustaceans.

As for Eophyton, it is a pity that this should still be mentioned as possibly of organic origin. It occurs precisely as true trails on the under surface of the slabs; it is found in every system from the Cambrian to the present time, where it can still be studied on the seashores; all the different forms, under which it presents itself are also still to be seen there. Although it thus has been *proved* that it cannot be any organism, Saporta still adheres to the opposite opinion. Now, if he had read through my work, he would have learnt that I by experiment have demonstrated that Eophyton can not only be produced by drifting plants, but also by the tentacles of Medusæ or other soft bodies. Now there are casts of Medusæ associated with Cambrian Eophytons of Sweden, and their habits were probably—as I have elsewhere[1] tried to show—similar to those of the existing *Polyclonia frondosa*, which creeps on the mud by means of its tentacles, and it is therefore likely that the *Cambrian* Eophytons are of this origin.

It is further stated that "the Chondrites of the Flysch, strongly impregnated as they are with carbonaceous matter, are admitted on all hands to be Algæ, and the author asks how the same origin can be denied to casts of specifically identical Chondrites of the Cretaceous and so on to the Liassic forms." This argument is, however, a real "*petitio principii,*" for it is so far from the actual state of things that the Chondrites of the Flysch are on all hands admitted to be Algæ, that many authors, and among them Dr. Th. Fuchs, of Vienna, whose excellent and exhaustive studies of the Flysch are everywhere known, hold a quite opposite opinion. And as for the supposed carbonaceous matter, it is not much better with this, as will be shown from a communication from Dr. Fuchs published in my work referred to: "The supposed carbonaceous nature of the Chondrites of the Flysch is in my opinion *a perfect mistake.* They are certainly very often quite black, but even in such cases they consist only of dark marl, *not* of coal."

Much more might be said on the fossil Algæ, but as I am about to combat the views held by Saporta more fully in a special work, I will here only add that I have found no statement whatever in his work referred to which would tend to alter my opinion, that almost all the "*Algæ incertæ sedis*" in Schimper-Zittel's "Handbuch der Paleontologie" are not vegetable fossils.

A. G. NATHORST

Stockholm, April 9

[1] A. G. Nathorst, "Om aftryck af Medusar i Sveriges Kambriska [illegible]yer." (*Svenska Vetenshaps Akademiens Handlinger*, Bd. xix. N[illegible], Stockholm: Norstedt och Söner, 1881.)

DR. NATHORST has certainly shown that many of the markings referred to Algæ by some authors might be tracks left by moving animals on a soft mud, but is there reason to suppose that there are conditions under which submarine surfaces of very soft mud with minute tracks have, or could ever have been preserved. On the other hand there is no question about seaweed having existed in Palæozoic and Mesozoic times, and either some of the markings in question are their prints, or no traces of them are preserved. Now it is an uncontroverted fact that even the most indestructible of all vegetable tissue, that of the Coniferæ, has been met with in the same condition of fossilisation, *i.e.* a projecting cast in sandstone on the under side of a slab, and without any internal trace of tissue or even of colouring due to carbon or iron, and Saporta has offered a satisfactory explanation of the origin of such casts. From the relative rarity with which terrestrial plants have been thus preserved, Nathorst almost derides Saporta's application of this explanation to fossil Algæ, yet it is by no means improbable that this may be their normal mode of preservation. The decay of dead olive-green seaweeds in water must be very rapid. The decomposition of some among them sets in almost immediately under water, and a colourless mucilaginous fluid is given off copiously. I have not watched the whole process of decay, but my impression is that the entire substance in some species would eventually pass away in a structureless glairy mass, and therefore that nothing but a hollow impression could ever be preserved. Casts of these would be more likely to be preserved in sand or mud than mere tracks, because the substance of the weed would occupy them, and prevent them from being immediately filled with the same quality of matrix as the surrounding rock, and until what would afterwards be a line of cleavage had been produced. So far therefore from its being exceptional for fossil seaweeds to appear as casts projecting from the under surface of the overlying mud, this is likely to be the normal condition in which fossil algæ are preserved. This is apart altogether from the question whether any of the Palæozoic markings are Algæ, for, these differ so considerably from any existing forms, that in the absence of internal structure it is quite unlikely that there will be any general agreement respecting them. These observations do not apply to the Rhodospermeæ, which scarcely enter into the question. Some simple experiments on the decay of seaweeds in fine sand under water, which any one at the seaside could make, would help to throw light on the subject.

J. S. GARDNER

The Weather and Sunspots

IN NATURE (vol. xxvii. p. 551) Mr. Williams ascribes the great cold of March, 1883, at the Riviera, to the absence of sunspots. There is the less reason for ascribing this cold to sunspots, as till now much more evidence goes the other way. And may it not be contended that this evidence is in favour of warm weather, with minimum sunspots in the tropics or in summer alone. The months of November to March, 1877–78, especially February and March, were so warm over an extensive area, especially in the interior of North America and Western Siberia, that the mean temperatures were nearly without precedent, while in no extensive country of the world the temperature was much below the average.

I give some data for March, 1874 (a season with a considerable number of sunspots), at Suchum-Kale, on the east coast of the Black Sea, a place in the same latitude as Cannes, and similarly situated in respect to sea and mountains; it is sheltered from cold winds, and much warmer than the surrounding country.

The observations in Russia being made at 7 a.m. and 1 and 9 p.m., and no minimum-thermometer used, the minima cannot be strictly compared.

The mean temperatures for a long average at Nizza (which are about the same as at Cannes) are January 47°·1, March 51°·8; at Suchum-Kale, January 43°·0, March 47°·8, being at both about 4° colder. Taking the mean of minimum and maximum as the daily mean at Cannes, and that of 7 a.m. and 1 and 9 p.m. as the daily mean at Suchum, we have: Coldest days of March, 1883, at Cannes, 10th, mean 35°·5, or 16°·3 below average; 11th, 34°·5, or 17°·3 below average; lowest minimum on the 11th, 24°·1, or 27°·[illegible] below monthly mean temperature. Coldest days of March, 1874, at Suchum, 3rd, 19°·9, or 27°·9 below aver[illegible] [illegible] below average. 20°·5, 27°·3 below average; [illegible] The lowest temperature at 7 a.m. was, on the 6th, 16°·4, or 31°·4 *below average monthly temperature.* Thus it is seen that at

Suchum, in the same latitude and in a very similar situation as Cannes, in March, 1874, a year with a considerable number of sunspots, there were three days which were more than 27° colder than the average, while in March, 1883, with little or no sunspots, the coldest days mentioned by Mr. Williams at Cannes was only 17°·3 colder than the average.

I want only to show by this example that if it is wished to prove anything as to the varying intensity of the sun's rays, a large number of observations in distant countries should be given, especially in middle latitudes, the work of Dové having well proved that there is always a compensation to a certain extent between cold and warm areas, and a very great number of these deviations being certainly due to causes which have nothing to do with anything beyond the earth's atmosphere.

St. Petersburg, April 17 A. WOEIKOF

Sheet Lightning

LOOKING to the south and south-east from the Bel Alp, the play of silent lightning among the clouds and mountains is sometimes very wonderful. It may be seen palpitating for hours, with a barely appreciable interval between the thrills. Most of those who see it regard it as lightning without thunder—*Blitz ohne Donner*, *Wetterleuchten*, I have heard it named by German visitors.

The Monte Generoso, overlooking the Lake of Lugano, is about fifty miles from the Bel Alp as the crow flies. The two points are connected by telegraph; and frequently when the *Wetterleuchten*, as seen from the Bel Alp, was in full play I have telegraphed to the proprietor of the Monte Generoso Hotel, and learnt in every instance that our silent lightning coexisted in time with a thunderstorm more or less "terrific" in Upper Italy. JOHN TYNDALL

May 12

I AM glad to find that M. Antoine d'Abbadie's remarks confirm in the main those which I have made on the above subject in NATURE (vol. xxviii. p. 4), especially as to the occurrence of lightning at a great altitude as observed in low latitudes.

In stating that he has frequently observed "thunder without lightning, and lightning without thunder," does M. d'Abbadie mean that, like every one else, he has observed thunder without observing lightning, and lightning without observing thunder? Or have we here a living advocate not only of the dumb lightning, but of the dark (lightningless) thunder?

The thin and local fogs which are not uncommon in thundery weather readily transmit the illumination of a distant flash of lightning. It seems highly probable that in such cases the lightning may be occasionally supposed to be an electric discharge occurring in the fog itself, just as a flickering aurora seen above thin clouds has often been supposed to have its habitat in the clouds themselves.

The suggestion of M. d'Abbadie is a fair one, and I for my part shall be glad to undertake observations of "sheet lightning" this summer in conjunction with any one resident about forty miles from this place, the observers interchanging reports by the earliest post after the hour of observation.

W. CLEMENT LEY

Ashby Parva, Lutterworth, Leicestershire

Hydrogen Whistles

IN his interesting communication on the above topic (NATURE, vol. xxvii. p. 491) Dr. Francis Galton has inadvertently fallen into a mistake which quite seriously affects the numerical deductions which follow. He erroneously assumes that "the number of vibrations per second caused by whistles is inversely proportional to the specific gravity of the gas that is blown through them."

It is well known that the number of vibrations is inversely proportional to the *square root* of the density or specific gravity of the gas. Hence for hydrogen, as compared with air, the number of vibrations per second produced by a given whistle would be increased only about 3·6-fold instead of 13-fold, as he states it. Similarly the number of vibrations by the use of hydrogen in the little [illegible] when set at 0·14 inches would be only about 86,533, instead of 312,000. JOHN LE CONTE

Berkeley, Cal., April 12

[THE objection of your correspondent is valid. I am informed independently and by high authority that the velocity of sound in hydrogen must be considered as barely fourfold greater than in air, the number of vibrations per second emitted by a hydrogen whistle being increased in the same proportion.

In making my earlier estimate I had been misled by an erroneous statement in a work that is still of much general credit and authority, to which I referred for ascertaining the velocity of sound in different gases, as it happened to be the book then nearest at hand, and as I have no special knowledge of the subject. It was the first edition of the *Penny Cyclopædia*, where in the article "Acoustics," p. 95, I lit upon the following passage, which professed to give the precise information I wanted:—"Thus air being about thirteen times as heavy as hydrogen, the velocity of propagation in the latter is about thirteen times that in the former." I need not take up your space by quoting the paragraphs before and after this, which emphasise and corroborate the statement and make it clear that it was no slip of the pen. Possessors of this Cyclopædia (I know nothing of subsequent editions) would do well to look out the passage and put a note of warning by the side of it.

The fourfold gain, or nearly so, of the hydrogen whistle is not to be despised. It is sufficient to establish its rank as the emitter of the largest number of aërial vibrations per second of any instrument yet contrived. My little whistle, of about 1 mm. bore, requires a very small supply of air, a bag that I fill with a single expiration containing enough to keep it in continuous sound for many minutes. As yet I have not got a portable holder for pure, dry hydrogen, but a well-known chemist is kindly making an experiment of one for me.

FRANCIS GALTON]

The Pillar of Light

I HAVE frequently observed this phenomenon. The first time I saw it was on April 8, 1852, when I saw it here at sunset, and on April 11 I saw it at sunrise when I was in the Irish Channel, near to Port Patrick, where I was laying a submarine cable.

In the *Monthly Notices* of the R.A.S. vol. xii. p. 185, there are several notices of its having been seen at that time in various places. I saw it last on April 6 this year, when it had the same appearance as previously, which is well represented by Mr. Symond's drawing on p. 7, except that the lower part is too bright, and it looks more correct when shaded with a pencil. The pillar is always perpendicular to the horizon and to the sun's position. I saw the zodiacal light several times in February, extending as far as the Pleiades, and at an angle of about 45°. I think it is highly probable that the pillar of light is caused by reflection from ice crystals, as we had very cold weather early in April, and have still. These atmospheric phenomena are often best seen reflected from a plate glass window.

Gateshead, May 9 R. S. NEWALL

Remarkable Lunar Phenomenon observed at Weston-super-Mare, August 21, 1861

AT about 8.30 p.m. a band of silvery light appeared proceeding from the lower margin of the moon, in a line perpendicular to the horizon. The width of this band was equal to the exact apparent diameter of the moon's disk. Slowly the band lengthened, until its upper portion reached beyond the moon to the extent of about two diameters, while the lower limb extended itself to about the length of four diameters, where its foot rested apparently on a light fleecy cloud. In a few minutes a similar band traversed the other at right angles, forming a perfect Latin cross, the brilliant face of the moon occupying the place of intersection. The arms of the cross were respectively about two diameters of the moon's face. The portion of the sky in which this occurred was clear, but clouds were slowly drifting from the west, and in ten minutes began to obscure this beautiful and unusual phenomenon.

The only record of any similar phenomenon which I can meet with is to be found in Lowe's treatise on atmospheric phenomena, wherein two instances are described. The observer of one was Dr. Armstrong, and the appearance was seen by him at South Lambeth on February 25, 1842. The other observer was Mr. Lowe himself, who was at Derby railway station when the phenomenon occurred. In both these instances, however, the crossbeam was absent. Although no hypothesis has been suggested to account for this appearance, it may be interesting to note that in the case recorded by Mr. Lowe, the very hour of its occurrence is identical with that of the appearance of the phenomenon seen by me, and the day of the month so closely approximates as to be only one day later. That which Dr. Armstrong saw in 1842 was at the time of the full moon in February.

C. POOLEY

Curious Habit of a Brazilian Moth

AT the last meeting of the Literary and Philosophical Society of Liverpool (April 30) I read the following note on a remarkable habit of a Brazilian moth; and as it is a habit that has perhaps not been observed before, it may be of sufficient interest for insertion in NATURE.

The moth (of which I inclose a sketch) is a species of Panthera (*P. Apardalaria*).

When rambling about the rocky beds of small streams on the Serra da Contareira, near San Paulo, I have often been struck by the great numbers of yellow and black moths that flew up from the water as I disturbed them by my presence. On careful examination I found that these moths were resting upon the wet stones, in many cases even with a film of water flowing over the spot on which they had settled, and were all engaged in sucking up the water through the proboscis (I can hardly call it drinking, for no imaginable thirst could account for the enormous amount of water sucked up), and this water was passing through the moths, minute drops forming on the tip of the abdomen, and falling off as formed. I timed several specimens, and found that the average rate was fifty drops per minute. I have observed the same individual remain in the same position with the action going on unceasingly for three hours; and in all probability it had been there some time before I observed it, and remained after I went away. But even in this length of time the quantity of water passing through the moth was enormous in proportion to its size. The drops I did not actually measure, but they are probably between 1 and 2 millimetres in diameter. Taking them to be 1·5 millimetres in diameter, the total amount of water in the three hours was 15·84 cubic centimetres, or almost exactly a cubic inch. This quantity is equal to about 200 times the volume of the body of the moth!

The tibiæ of the hindlegs are very thick and are armed with long hairs, that by their capillary action prevent the moth being immersed in the water. I have often seen one of them knocked down by a little spurt of water splashing over the stone on which it was standing, and it recovered itself immediately without being wetted in the least.

Upon my return to Brazil I shall try to measure exactly the amount of water passing through one of these moths. And it would be most interesting to find out what is the object of this excessive drinking. Can it be that the moth extracts nourishment from minute particles of organic matter contained in the water?

I may remark that the water of the streams where I have observed the moth is very clear and pure.

E. DUKINFIELD JONES

Acrefield, Woolton, Liverpool, May 5

Leaves and their Environment

I TAKE the following from an experiment which I made two years ago. I think it throws some light on the point under discussion:—

On May 8 six young pea plants, similar in size, &c., were transplanted from the garden into three large flowerpots, a pair in each, and were covered with bell glasses. On next day an apparatus for generating a constant stream of carbonic acid gas was connected to No. 1 bell glass. No. 2 was left normal. No. 3 inclosed a small disk of caustic potash solution. They all had as nearly as possible the same amount of sunlight, and the same measured quantity of water was given to each.

Taking the notes referring to the leaves only I find on May 21: "No. 1, vigorous large leaves. No. 2, much smaller leaves. No. 3, leaves smaller than No. 2, with edges serrated as if the veins were growing on, but could not find food for fleshy part of leaf—really a starved plant."

On May 27 the plants were taken up and washed, when No. 1 weighed 148 grains; No. 2, 115·5 grains; and No. 3, 87 grains. After drying, the weights of Nos. 1 and 3 were as 19 to 13. The longest leaf on No. 1 measured 1⅝ in., and on No. 3 1⅛ in.

J. BROWN

Belfast, May 3

Foam Balls

IN NATURE, vol. xxvii. p. 531, there is a mention by Mr. J. Rand Capron of foam balls. These are common on the coast of the Northern United States, especially of a cold dry day, when, if there be much wind, these huge foam balls, which may reach a diameter of two feet or more, are rolled up the beach. Their weight soon changes their form, so that at last they present the appearance of long white rolls of sparkling foam. This singular appearance was first described in verse, so far as I know, by Dr. S. Weir Mitchell, of Philadelphia. The verse, as I recall it—I quote from memory—is this:—

> "And wilder yet when of a winter day
> The cold dry norther rolls athwart the beach
> The gleaming foam balls into serpents white,
> And all the sand is starred with rainbow light."

Philadelphia, U.S. AN AMERICAN SUBSCRIBER

ANTHROPOLOGY[1]

II.

IN considering the claims of anthropology as a practical means of understanding ourselves, our own thoughts and ways, we have to form an opinion how the ideas and arts of any people are to be accounted for as developed from preceding stages. To work out the lines along which the process of organisation has actually moved, is a task needing caution and reserve. A tribe may have some art which plainly shows progress from a ruder state of things, and yet it may be wrong to suppose this development to have taken place among themselves—it may be an item of higher culture that they have learnt from sight of a more advanced nation. Our own history shows to how small an extent we have been the developers of our own arts and sciences, how largely we have embodied the culture of other nations. It is essential in studying even savage and barbaric culture, to allow for borrowing, so as to clear the lines of real development. When the savage comes into contact with the civilised man, he does not see his way to copy all the high contrivances of this mysterious higher being, but where he thinks he can imitate, he is apt to try, and sometimes succeeds, though oftener fails. After a time of friendly intercourse, the wild man generally learns such substantial secrets of culture as he is in a position to assimilate. Ethnologists have been inclined to look on the wandering Esquimaux of the polar regions as "nature-men," and perhaps no harm has arisen from reasoning on them as such, for they are in many ways fair representatives of the rude nomad hunter and fisher. But I suspect that in some respects they do not show the mere result of the primitive savage working out by slow degrees a somewhat higher culture. Looking at them not as they are now, Europeanised under missionary training, but as they were when Egede and Cranz went out to them from Denmark in the eighteenth century, it seems that their way of life even then had some incidents above the savage level. Their clothing was artistically contrived to resist the intense cold. Its material is sometimes strange to our notions; an undershirt of birds' skins with the feathers inside requires an effort to realise even in our bleakest season. But a leather tunic with sleeves and a hood to pull over the head, a pair of sealskin breeches with leather stockings and boots, form a defence against the cold, at once like that familiar to Europeans, and unlike any unquestionable savage costume, such as the furs which in the Antarctic regions the shivering Fuegians throw over their shoulders. Moreover, all across the polar coast region of the Esquimaux their houses of earth or moulded snow, with compartments like ship-cabins, are warmed and lighted with blubber, burnt in lamps shaped out of potstone with moss to serve as wick, and over these are hung the potstone kettles for their slight cookery. Now,

[1] Two lectures on "Anthropology," delivered on February 15 and 21 at the University Museum, Oxford, by E. B. Tylor, D.C.L., F.R.S. Continued from p. 11.

the kettle carved out of potstone (lapis ollaris) is ancient in Scandinavia, and the plain open dish lamp occurs widely in Northern Europe (it lingered till lately in the Scotch *crusie*). But the lowest races know nothing of so cultured an invention as a lamp. It is of course within the wide bounds of possibility that under the stress of a climate so cold for loose-clad, half-naked men, and where the scanty supply of wood drifted to the shore was too precious for fuel, the Esquimaux, driven by the warlike American Indian tribes of Algonquins and Athabascas, may have discovered how to improve their clothing, and to warm and feed themselves by the aid of lamps, so that they could hold their own against the rigour of polar nature. But if so, how curious that they should have done this by inventing just what the Norsemen could have taught them. Independent Greenland invention, if possible, is hardly probable, and I think a strong case may be made for an easier explanation. We know that the ancestors of the Esquimaux had been in contact with Scandinavians since before our Norman Conquest, when in 1004 the small, sallow, broad-cheeked Skrällings in their skin canoes slew Thorvald with their spears thrown with throwing-sticks, and he was buried with a cross at head and feet at Crossness, which may have been about where long afterwards the Puritan emigrants landed from the *Mayflower*. It seems clear that the Esquimaux had to go north from these delightful regions of New England, but they lived for ages within reach of the Norse settlers in Greenland, whose last survivors in the fourteenth century are thought even to have merged their race in some tribe of the despised Skrällings. Thus it is not surprising that the Scandinavians returning to Greenland after four hundred years more should have found the Esquimaux shaping their skins and furs into semi-European garb, and by the aid of these and the stone lamps and kettles maintaining a polar existence which, without these, would have been difficult indeed. Even that curious Scandinavian institution, the scurrilous nith-songs with which the Norse champions drove one another wild with fury, so that they had to be prohibited by law under heavy penalties, had become a regular Esquimaux custom, and Rink calls them simply *nid-vise*, just as he would have called them among his own Danish forefathers. His first specimen is a Greenland song sung at festive winter gatherings, made to ridicule one Kukouk, who was a poor hunter and fisher, but loved to make friends with the white men ; it begins—

> " Wretched little Kukouk
> He takes care of himself ; " &c.

If this view of a Scandinavian element in the culture of the Greenlanders is sound, we have the curious spectacle of modern Danes going to civilise the wild men and describing their manners and customs as those of savages, without a thought that some of the most curious of them were relics of forgotten life of their own old Norse-land.

That families can go down in the world is only too well known, nay, that whole tribes and nations can in evil days fall off from their old prosperity, intelligence, and virtue. The question asked by the anthropologist is whether a civilised race can sink to the barbaric level, and thence to the lowest or savage level. The answer is as yet in a confused state, but certain elements of truth may already be got at. It appears at any rate that when civilised men take to a wild life, and mix with the people of the wilderness, they may give rise to a race in whom knowledge and comfort and morality are lowered from the ancestral level. This is the familiar case of the Gauchos of the Pampas, Spaniards in language and partly in race, but leading a life which to the soldiers of Pizarro would have seemed gross and brutal. In the forests of Ceylon there still roam families of wild men, the so-called Veddas or hunters who, as names of places show, once lived widely over the country till dispossessed by the invading nations from India. The wildest Veddas are to be found in the park-like hunting-grounds of Nilgala, and in "the land of Bintan, all covered with mighty woods and filled with abundance of deer," as Robert Knox described it two centuries ago. These wild shy people, of stature averaging under five feet, and in skin duskier than the Singhalese around them, with tiny heads covered with a mass of shaggy hair, and showing in their dull and melancholy faces that uniformity of feature and expression so characteristic of low grades of culture, may seem at first sight to lead a life comparable with that of the forest savages of Brazil or New Guinea. Yet their language is a Singhalese dialect ; they are in fact the one known race who may be called savages, and yet speak a language of our own Aryan stock. The following is one of their charms, intended to subdue an elephant in the forest, whom it describes in terms which show a curious transition between the charm and the riddle ; indeed, every one who remembers our own nursery riddle about the cow will be struck with its close resemblance to the Vedda charm :—

> " Ichchata vallay
> Pachchata vallay
> Déla devallay
> Situ appá situ."
>
> " In front a tail,
> Behind a tail,
> On the two sides two tails,
> Stay, beast, stay ! "

This is almost Sanskrit, and it is obvious that with so distinctly Aryan language there must needs come some strain of Aryan blood, for it is almost outside the possibilities of social life that a tribe should adopt a language, without such intermarriage with those who speak it that thenceforth the people will in part have ancestry corresponding to the new tongue. The Singhalese indeed hold the Veddas to be of Aryan descent, and in the Mahawanso they stand as offspring of no less an Aryan ancestor than King Vijayo, who married the native princess Kuvéni, and by her means conquered the Island of Ceylon ; afterwards, when he ungratefully divorced her and took a daughter of King Pandavo of Madura, the native queen wandered away, and her children married in-and-in, as continued till lately to be Vedda custom. This, says the poem, was the origin of these Pulindá or barbarians ; and thus it is that they still claim royal descent, and look down on the Singhalese. Combining the evidence of the Vedda skulls and features with that of their language, we may so far agree with poetic legend as to consider them really outcasts of mixed Aryan and Dravidian or indigenous race. If so, it must be granted that descendants of the Aryan stock, "heirs of all the ages in the foremost files of time," may be found among tiny, shy, wild men of the woods, with sad dull features peering out of their matted locks, who dwell in huts of boughs when they cannot get the preferred shelter of a cave, who live on venison and wild honey and fish drugged by putting poisonous fruit in the pools, and who in their intercourse with the more cultured Singhalese bring themselves into contempt by their simple truthfulness, that utter incapability of cheating and lying which is as characteristic of the savage state as it is rare at higher levels of culture. Truly the condition of these poor relations of ours is of interest. But they are not Aryans on their way upward from primitive rudeness. Their kinsfolk actually till patches of ground and are a settled if a rude people, and these wildest Veddas are evidently a few dwindling clans of outcasts sunk from a higher stage. There is not among them any evidence that they have been in the Stone Age ; a story is told of them like our own legend of Wayland Smith's cave, that in old times when they wanted arrows they would carry loads of meat to the smith's shop in the night, and hang it there with a leaf cut to the pattern of the arrow-heads they expected him to leave out in exchange ; at any rate it is certain that they have always had iron by barter from more civilised neighbours, and their occupations, especially the taking of wild honey, are such as belong to the Singhalese.

In the presence of such examples as these, anthropologists admit that civilisation has always had its ups and downs. A nation may themselves develop some thought or art, or borrow it from abroad, and then ages afterwards lose this knowledge or skill because they have no longer the power or leisure to keep it up. It is only after taking such cautionary examples of the migration and degeneration of culture, that it becomes safe to trace the lines along which civilisation has developed in the world.

"When will hearing be like seeing?" says the Persian proverb. Words of description will never give the grasp that the mind takes through actual sight and handling of objects, and this is why in fixing and forming ideas of civilisation, a museum is so necessary. One understands the function of such a museum the better for knowing how the remarkable collection formed by General Pitt-Rivers came into existence. About 1851 its collector, then Colonel Lane Fox, was serving on a military sub-committee to examine improvements in small arms. In those days the British army was still armed (except special riflemen) with the old smooth-bore percussion musket, the well-known "Brown Bess." The improved weapons of Continental armies had brought on the question of reform, but the task of this committee of juniors to press changes on the heads of the service was not an easy one, even when the Duke of Wellington, at last convinced by actual trial at the butts, decreed that he would have every man in the army armed with a rifle musket. Colonel Fox was no mere theorist, but a practical man who knew what to do and how to do it, and his place in the history of the destructive machinery of war is marked by his having been the originator and first instructor of the School of Musketry at Hythe. While engaged in this work of improving weapons, his experience led his thoughts into a new channel. It was forced upon him that stubbornly fixed military habit could not accept progress by leaps and bounds, only by small partial changes, an alteration of the form of the bullet here, then a slight change in the grooving of the barrel, and so on, till a succession of these small changes gradually transformed a weapon of low organisation into a higher one, while the disappearance of the intermediate steps as they were superseded left apparent gaps in the stages of the invention, gaps which those who had followed its actual course knew to have been really filled up by a series of intermediate stages. These stages Colonel Lane Fox collected and arranged in their actual order of development, and thereupon there grew up in his mind the idea that such had been the general course of development of arts among mankind. He set himself to collect weapons and other implements till the walls of his house were covered from cellar to attic with series of spears, boomerangs, bows, and other instruments so grouped as to show the probable history of their development. After a while this expanded far beyond the limits of a private collection, and grew into his Museum. There the student may observe in the actual specimens the transitions by which the parrying-stick used in Australia and elsewhere to ward off spears must have passed into the shield. It is remarkable that one of the forms of shield which lasted on latest into modern times had not passed into a mere screen, but was still, so to speak, fenced with: this was the target carried by the Highland regiments in the Low Countries in 1747. In this museum, again, are shown the series of changes through which the rudest protection of the warrior by the hides of animals led on to elaborate suits of plate and chain armour. The principles which are true of the development of weapons are not less applicable to peaceful instruments, whose history is illustrated in this collection. It is seen how (as was pointed out by the late Carl Engel) the primitive stringed instrument was the hunter's bow, furnished afterwards with a gourd to strengthen the tone by resonance, till at last the hollow resonator came to be formed in the body of the instrument, as in the harp or violin. Thus the hookah or nargileh still keeps something of the shape of the coco-nut shell from which it was originally made and is still called after (Persian, *nârjîl* = coco-nut). But why describe more of these lines of development when the very point of the argument is that verbal description fails to do them justice, and that really to understand them they ought to be followed in the series of actual specimens. All who have been initiated into the principle of development or modified sequence know how admirable a training the study of these tangible things is for the study of other branches of human history, where intermediate stages have more often disappeared, and therefore trained skill and judgment are the more needed to guide the imagination of the student in reconstructing the course along which art and science, morals and government, have moved since they began, and will continue to move in the future.

It is convenient in illustrating intellectual development to choose a branch where every one, so to speak, carries his specimens about with him. Some eighteen years since I made an attempt to describe and analyse the gesture-language, in order to show the consistency of principle with which men debarred from spoken language, whether deaf-mutes or men unacquainted with one another's languages, contrive to utter their own thoughts and understand the thoughts of others through expressive gestures. In these gestures we have a direct and universal outcome of the human mind, a system by which a deaf and dumb scholar from an English asylum can hold converse at first sight with Laplanders or Iroquois or Chinese. They understand each other because they use signs for the most part self-expressive, and conveying their own meaning to those who never saw them before. Now any idea can be thus conveyed by self expressive signs, not in one way alone but many. A hunter of the prairie, for example, has to express the idea "horse": this he can do by various signs, as by the hand so held as to imitate a horse's head, or by the act of straddling a pair of forked fingers across the edge of the other hand, or by the imitated motion of the gallop; different as these signs are, each tells its own tale. When, however, people have been long used to converse together in gestures, they are apt to cut them down into abbreviated forms which do not show their meaning at first sight, and might even seem to outsiders to be artificial. Thus, a white man, seeing a Cheyenne Indian hold his bent arm forward with the hand closed knuckles upwards, was puzzled as to what this might mean; the Indian, seeing his look of perplexity, took a stick, and bending his head and back, completed the picture into that of a bent old man leaning on a staff, thus showing that the sign meant "old man." Traditional signs may even go on after their reason has passed away, as the sign for "stone," made by hammering with the closed fist on the other hand, a gesture dating from the Stone Age, in which the Indians lived within a few generations, when their only hammer was a stone. These two examples are taken from the recent careful collection of North American gesture-signs by Col. Mallery, published by the Smithsonian Institution. The labour and expense which anthropologists in the United States are now bestowing on the study of the indigenous tribes contrasts, I am sorry to say, with the indifference shown to such observations in Canada, where the habits of yet more interesting native tribes are allowed to die out without even a record. But to return to the gesture-language. This passage of self-expressive signs into what seem arbitrary signs throws strong light on the principles of spoken language, where we find a few self-expressive sounds, such as interjections and names of animals imitated from their cries, while the great majority of words are not even traceable back to the self-expressive stage from which the analogy of gesture-language leads us to suppose that they originally sprang. Moreover, the sequence or collocation of gesture-signs conforms to fixed rules, which display the action of the thinking mind. The subject must precede the attribute: for instance, such a sequence as a "heavy stick" would have no sense to the sign-maker, who necessarily introduces the stick before he can clothe it with an attribute. Phrases, so to speak, out of an American gesture-story illustrate the gesture-syntax. When the finger-tips of the two hands are brought together to show a hut or wigwam, then pointing to one's own breast does the work of the pronoun, "hut-mine." The sequence "buffalo-one-shot-killed" starts with the idea of buffalo, adds that there was one, and then the sign-maker, having placed the idea of that one buffalo before his interlocutor, can imitatively shoot at it, and it falls dead. He can even imply the idea of causation in the sharp following of the shot by the animal's fall, which makes one the instantaneous consequence of the other. In spoken language the theory of syntax or combining-order is a subject of great complexity and difficulty. Of the few philologists who have attempted it, mention may be made of Steinthal, von der Gabelentz, and Max Müller, whose early dissertation is published as an appendix to Bunsen's "Philosophy of Universal History." But while the age-long shifting history of speech has brought the order of sequence of its elements into an entanglement hardly possible to unravel, we have still before us the first clue in the sequence by which man has arranged his gestures, and will do so anew when he is put to pantomime as a means of converse. Thus the philologist, engaged in studying the formation and combination of speech-sounds or words, may have from the anthropologist the natural rules framed by the human mind dealing in the simplest of known ways with the problem how to express thought.

Scarcely less light is thrown on the working of the human mind by the history of that special development of error which since the remotest ages has taken the form of magic. Of late certain events in France have revived popular interest in that curious old-fashioned instrument, the divining-rod, and as I happened to be staying at a friend's house in the Mendip district, where it is still used by well-sinkers and miners, at my request a regular practitioner was sent for. I show the instrument—a forked hazel twig, which is held loosely by its outward-bent ends in the closed upturned hands, so that it can rise or fall easily. On approaching a spring, vein of ore, &c., the rod dips toward it, but when replaced horizontally and passing over the place, it rises toward the bearer's face. That the spring or other object sought has really no effect on the instrument, but that its dipping has to do with the seeker, is sufficiently shown by its being considered to act with the most dissimilar objects—a spring of water, a vein of ore, a piece of metal, a dead body—which have, however, this in common, that they are what the "dowser" is in search of. It does not appear that he fraudulently moves the rod, but my sensations led me to agree with Chevreul that the slight movements of the hands are unconsciously guided to accumulate into impulses sufficient to cause the twig to dip or rise. I noticed that when I could allow my attention to stray, the rod would from time to time move in my hands in a way so lifelike that an uneducated person might well suppose the movement to be spontaneous. It is hardly necessary to say that the rod always moves where the bearer's mind suggests an object. In the present case the special business of the dowser was to find springs of water, and his difficulty was to distinguish between the mere *top springs*, which though acting on the rod were of course practically worthless, and the valuable *main springs* which would repay the sinking of a well. In the trial an incident occurred which threw light on the nature of the whole operation. The rod when brought over my watch, dipped strongly, and the dowser looking up at me with innocent archness said: "You see, sir, it's just over the *mainspring* of your watch." The remark showed how his mind was so simply controlled by association of ideas, that he expected the same action from a *main spring* of water and of a watch, their likeness of name quite overriding their unlikeness of nature. Nothing could have better shown at once the man's sincerity and the purely ideal character of his craft, nor does one often meet with a more perfect illustration of the state of mind where magic has its origin in delusive analogy, whether of things or of their names. Magic has often passed as mystery, but to the anthropologist few arts are less mysterious; he reads by childishly simple association of ideas the open secret of half the magical rules which prevail in savage and barbaric life, and even last on into the midst of civilisation. In the wild north-west of Ireland I learnt not long since the use of the "worm-knot" for curing ailments of cattle; it is a bit of cord in which a peculiar slip-knot is made, and if this knot when pulled over the creature's back comes away clear (as shown), the disease will come away too. On the same principle, the purpose of the pig's heart stuck full of thorns and bricked up in an old chimney (produced) was sympathetically to pierce with pain and shrivel with disease some hated person, probably a reputed witch suspected of "overlooking." It is a curious exercise to read from this point of view the precepts of the modern astrologer, which still show their quasi-reasons, futile but quite intelligible. Suppose one's self seeking for lost property, the significator of the thing missing will be the moon, apparently because herself so often lost and found again. According to her position, east or west, the object must be looked for; if the Moon is in a human sign, as the Twins, it will be in a human habitation, but the sign of the Bull indicates its being in a cow-house. Even the thief's clothes are denoted by the governing planets; under Saturn he will be found in a black suit, or if Mars is in it, his presence will be shown by some red article, perhaps a neck-handkerchief. Folly as this is, it at any rate shows the working of uneducated men's minds, where the argument from analogy appears in its early crude state, not yet cleared by observation, but still on its way to become, under proper checks and reservations, the explorer of the universe and the guide to science.

This is by no means the only example of a delusive theory being, when honestly worked out, productive of scientific truth. In times when the study of races for mere knowledge sake had little hold on scholars' minds, anthropology was much indebted to the fancy that any people whose presence in an outlying region seemed hard to account for must be the "lost tribes of Israel." One nation answered the conditions of this theory about as well as another: the remnants of the ten tribes were found marauding in the Afghan passes, wandering with the reindeer in Lapland, chasing buffaloes on the American prairies, or slaughtering human victims on the teocallis of Mexico. The manners and customs of these countries being studied, showed distinct analogies with Jewish customs, as indeed they would have with German or Chinese, or any other. Enthusiasts such as Rudbeck, or Garcia, or Adair, of course did not see this, but the practical result was that, especially in North and South America, evidence of great value in the history of civilisation was recorded which would have perished had it not been thus caught before it was swept away by European influence. This is a good instance of its being better to have a bad hypothesis than none at all. The ten tribes delusion has now, however, sunk to a lower level than when Lord Kingsborough spent his fortune in publishing the Mexican pictures and chronicles. But in spite of all the new real knowledge as to races, it has even in this country more votaries than ever. On opening, the other day, a book of the curious "Anglo-Israelite" sect, I met with the following passage, written in evident seriousness by a seeker after proofs that the British nation are the Lost Tribes of Israel:—"I am even now acquainted with many words in current use in some parts of the West of England that were in common use by Israel of old, and that I have not found in use in any other country—such as goad, gourd, barm, leaven, comrade, lattice, chambering, flay, score, gallon, cruse, lintel, latchet, girdle, pitcher, platter, glean, &c., &c." It takes a little thought to understand the full depth of ignorance of a man who, finding these words in the English Bible, thought they were used by the ancient Israelites. Why I ask you to notice it is that the author of the volume it is printed in says that 100,000 copies of his work have been sold; there is, indeed, no doubt but that this abject nonsense has a far larger circulation than all the rational ethnology published in England. It opens a window by which we can see into the state of education of its readers, who mainly belong to the lower middle class, and whose thousands of schools are as yet unvisited by the University Delegates on the one hand or the Education Department on the other. Even our Public Elementary education, good as it is in many respects, passes some questionable anthropology. Happening to look a few days ago at a Third Standard book on English History, I was surprised to find a picture of a South Sea Islander, tattooed all over, standing to represent the condition of the ancient Britons, who are described as savages. Now this is hardly an appropriate designation for a pastoral and agricultural people, who had a gold coinage, and whose war-chariots even the Roman legionaries found troublesome to deal with.

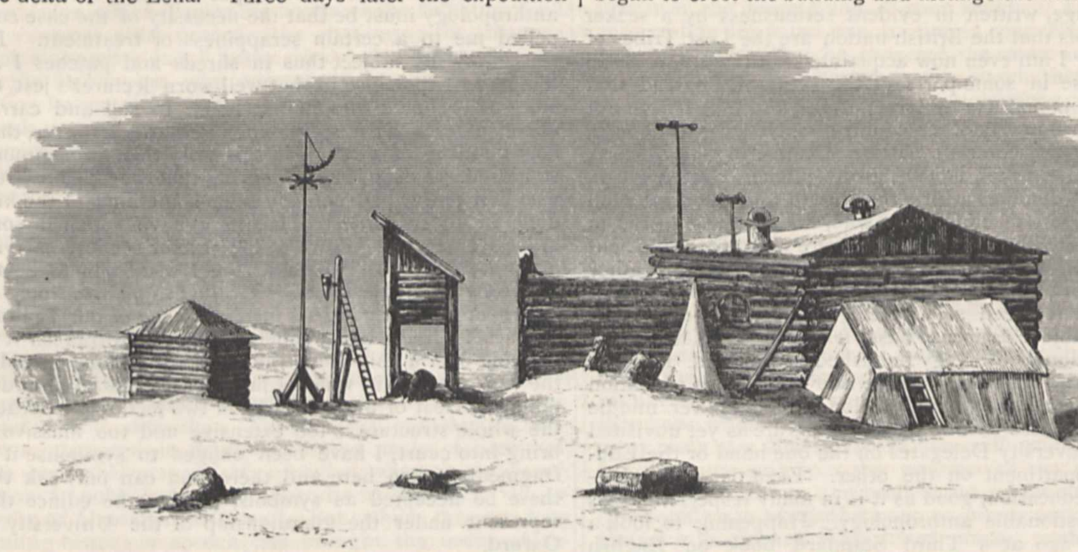
Having now attempted to support the claims of the problems of human nature to fuller recognition in our system of advanced education, it may be well to observe by way of caution that anthropology, while contributing materials to other sciences, does not dictate the conclusions which each science is to draw from them. It has not a rule of morals, a system of politics, or a doctrine of religion to teach, only a series of facts showing the stages through which each of these has been developed, and with these the counsel that the anthropological way of studying human conduct is to trace its principles along the historical line of their change and progress. Anthropology, though acknowledging degeneration as an important factor in human life, gives no encouragement to pessimist theories of society. The clinging to life by savage and civilised alike is a measure of their judgment that with all its ills it is a substantial good, to be valued and defended. That the tendency of mankind is toward industrial progress need not be proved, for it is not denied. That moral progress is on the same footing rests on the main fact that man obtains the happiness he seeks, not only through his own sensations but by sympathy with the enjoyments of others; now beings whose interests are thus consonant with the prosperity of those around them are plainly on the road to good rather than evil. At the same time facts constantly presenting themselves in anthropology guard the student from a prejudiced optimism. He has the picture constantly before him of low-cultured but kindly and truthful tribes of favoured climates, into whose midst the march of civilisation is bringing the beginnings of trade and wealth, and with them temptations to selfishness and dishonesty. At every step in the advance towards prosperity he sees, accompanying the growth of knowledge and the raising of the social standard, a series of concomitant evils, the break-up of the old stage, the failure to assimilate the new. Often a dispiriting lesson, this is yet of the highest practical value, for it elucidates what the statesman should be ever striving to learn, how, in the remodelling of institutions, to gain the utmost advantage while minimising the accompanying loss.

To conclude: my explanation of the unsymmetrical way in which I have here put forward the cause of anthropology must be that the necessity of the case compelled me to a certain scrappiness of treatment. For presenting my subject thus in shreds and patches I am tempted to apologise in that well-worn lecturer's jest, the story of the man who had a house to sell and carried about a brick as a specimen. Perhaps, however, there may be more of a moral in this story than is commonly supposed. I cannot help fancying that the flippant Greek who first told it had actually seen something of the kind done in sober earnest. He may have watched some grave Roman going down to the prætor's court carrying a tile in his hands, which in the lawsuit was to be the legal symbol of the house itself, just as a farm would be represented by a sod of its turf, or as one of our Teutonic forefathers, living in a wooden house, would transfer it by handing over a chip from the doorpost. This indeed is the very position in which I find myself placed in undertaking to treat of anthropology in two lectures. Because the whole structure is too extensive and too massive to bring into court, I have been obliged to symbolise it by fragments taken here and there, and can only ask that these be accepted as symbols, placing the edifice they represent under the guardianship of the University of Oxford.

THE ARCTIC METEOROLOGICAL STATION ON THE LENA

THE last number of the *Izvestia* of the East-Siberian branch of the Russian Geographical Society gives further news of the Lena Arctic Meteorological Station, dated October 24, 1882. This news was brought by the American officers, Messrs. Garber and Schütze, who left the station on October 25, and reached Yakutsk on November 29. Mr. Schütze made a sketch of the station, which appeared in the *Izvestia*, and which we reproduce. The house brought from Yakutsk proved to be comfortable and warm. It has been erected at the Sagastyr arm of the Lena, on Sagastyr Island (in 73° 22′ 30″ N. lat., and 144° 14′ 46″ E. long.); the name of this island is very significant: it means "it blows away." Galleries of planks have been erected behind the house to connect it with four pavilions for instruments. Besides the coal that has been taken from Yakutsk, the station has a good supply of fuel in the driftwood scattered around the station. The Sagastyr arm of the Lena supplies the inhabitants of the station with fresh fish. The health of all the members is satisfactory. Dr. Bunge received a contusion to a rib during the journey, but he is now well, and is besieged by indigenes, who come to him for medical help. Several Tungus families stay at one and two miles' distance from the station, and they are on the best terms with the meteorologists. The temperature is very low and, as there is no snow, the prospects are not very brilliant. The soil is frozen to a great depth, and cracks; the rivers and lakes are covered with a thick sheet of ice, so that the water beneath the ice is shallow, and the fish are in want of air to breathe. The food for the reindeer is frozen. Even at Yakutsk there was but one inch of snow on December 16, and a great inundation is expected for the spring, as well as epidemics, which are said usually to follow inundations.

As to the journey from Bulun to Sagastyr, it was performed not without difficulties. On August 6 a fresh west wind compelled the boats to stop on unfavourable ground, and the wind blowing with increasing force, it soon turned out a strong storm, blowing from north-west on August 9. The boats were thrown aground close by the banks of the river, and filled with water. The waves rolled above their decks. The chief instruments were, however, safe, as they were landed in time. On August 19 the expedition reached the Ketakh settlement, seven miles south of the signal usually drawn on the maps of the delta of the Lena. Three days later the expedition chose Sagastyr Island as the place for the station, and began to erect the building and arrange the instruments.



Russian Meteorological Station at Sagastyr, at the mouth of the Lena, 73° 22′ 30″ N. lat., 144° 14′ 46″ E. long.

The house of the station is obviously very small; and when looking at Mr. Schütze's sketch of this small building, lost amidst the snow-plain, far from any communication with the civilised world, one cannot but admire the devotion of those who have willingly submitted to remain in these inhospitable latitudes for scientific purposes.

THE AURORA BOREALIS

AMONG the numerous varieties of the aurora borealis or northern light, there is one of particular interest as regards the determination of the origin of this phenomenon. This variety, which was observed and reported upon in 1868 by the members of the Swedish Polar Expedition, takes the form of tiny flames or a phosphorescent luminosity, appearing during the winter months in the Polar regions, around projecting objects, viz. mountain cones and ridges. This phenomenon is so prominent that one need not be a scientist to discover it, and it was observed by our well-known philologist, Herr M. A. Castrén, during his journeys in Siberia. Herr Castrén's descriptions of the phenomenon are very minute, and exactly in accordance with its usual appearances, but his observations were, however, not known to me in 1868, and it was only on the return of the expedition that I heard of them. The observations made by the Swedish expedition at Spitzbergen led the Finnish Society of Science in 1871 to despatch an expedition, of which I was a member, to Lapland to ascertain if such a phenomenon could not be called forth, or at all events magnified, by mere mechanical appliances. And assuming that the aurora borealis in general, and the variety of the same just mentioned in particular, is caused by electric currents in the atmosphere, an apparatus of the following nature was erected on Luosmavaara, a mountain-top about 520 feet above the surface of the Lake Enare, in Lapland. It consisted of a number of fine points of copper wire laid out in the shape of a wreath two square metres in area, and connected by a circular wire of the same metal. This wreath was attached to a long pole, from the top of which a single copper wire (0·4 mm. in diameter) led to a galvanometer fixed in a room in the Enare vicarage, some two miles distant east, and from the galvanometer another copper wire led to a disk of platina in the earth.

When this circuit was closed, the galvanometer gave a deflexion, although faint. But on the very same night the apparatus was erected, viz. November 22, 1871, *there appeared an aurora*, which began with a single perpendicular column of light *above the top of the Luosmavaara!* This column was analysed with the spectroscope, and gave the usual yellow-green line, but whether the column was *on* or *behind* the mountain-top could unfortunately not be clearly ascertained. That it, however, had its origin from the apparatus described above appears to me, after the researches were made which I am about to detail, to be beyond a doubt. At the same time, on November 22, 1871, it was observed, when studying the spectrum of the flames which, on that day appeared around the mountain-tops more distinctly than usual, that the characteristic yellow-green line in the spectroscope was returned from nearly every object, as, for instance, the ice of a pond, the roof of a shed, and even, though faintly, from the snow in the immediate vicinity of the observatory. These observations led me to believe that I was within a sphere of electric discharge, whose radius extended considerably around the station.

This interpretation of mine has, however, not been generally accepted by students of the phenomenon, who, on the other hand, have explained the appearance as being an aurora reflected from the earth; but that this theory is erroneous will be clearly demonstrated by the researches detailed below.

In Baron Nordenskjöld's exhaustive investigations of the aurora borealis during the *Vega* expedition, he states that he was unable to discover this phosphorescent phenomenon which I have observed, and that he had noticed in the spectroscope, in connection with the same, a faint band near the line D; but this has nothing whatever to do with the auroral line. In order, however, to make it clear that I have not confused these two lines, I may state that already in 1871 I observed the absorption-band in question, as will be seen from my work at the time on the aurora borealis and the auroral spectrum. In these researches I determined the wave-length of this line, and as the latter is only apparent in daylight or moonshine, while my observations were without exception made in the dark, it is perfectly clear that this line or band has nothing in common with the auroral one. The two lines are, in fact, of such a different character that they cannot be confused for a moment by any one who has had an opportunity of comparing them simultaneously.

During the period which has elapsed since 1871 my efforts have been bent on the closest study of the aurora borealis, and an accidental discovery that a Geissler tube may produce certain forms of light without being in direct connection with either pole of an electric battery further stimulated my attention. This led to the result that the electric current emanating from the pole of an electric apparatus, while the other is connected with the earth, can be made to traverse a layer of air of *ordinary* density without producing any light at all; but when, on the other hand, it encounters a layer of *very thin* air, the luminous phenomenon will at once appear. These researches led me to construct an auroral apparatus for demonstrating this phenomenon as it appears in nature. The knowledge I have gained of the aurora during my continued labours and the above-mentioned observations in particular, made me conclude that an attempt to produce the auroral phenomenon in the very lap of nature by aiding the action of her own forces, ought to give important results and also originate a method for the future study of the phenomenon. In consequence of this idea I made the proposition to the International Polar Congress which I attended at St. Petersburg in 1881, to erect a station at Sodankylä to follow up my researches, although the results might perhaps be of negative value.

The Experiments on Oratunturi.—For the purpose of carrying out the experiment suggested above, the Finnish Polar Expedition was comparatively well equipped, the Physical Faculty of the University at Helsingfors having contributed instruments as well as wire; but the former had to be slightly altered for the research in view. Circumstances, however, occurred which prevented my work being commenced until the end of November, 1882; and after having examined the country and made some preliminary experiments from the steeple of Sodankylä Church, situated close to our observatory, I determined to erect the apparatus on the summit of the Oratunturi Mountain, some twelve miles from the observatory. The top was well suited for the purpose, although surrounded by rising copses of wood, which should, according to theory, be rather detrimental to such experiments. The summit was determined by barometrical measurements to be 1070 feet above the town of Sodankylä. To lead the wire from Oratunturi to Sodankylä was, in consequence of the great distance, impossible, as there were neither wires nor insulators sufficient, and I had therefore to carry out the experiments as follows:—

On the summit of Oratunturi (lat. 67° 21′, long. 27° 17′·3 E. of G.), about 540 metres above sea-level, I laid out the instrument which I have named an "utströmnings" apparatus, *i.e.* a "discharging" apparatus. It consisted of a bare copper wire 2 mm. in diameter, fitted at every half metre with points soldered thereon. The copper wire was laid out in entwined squares, while the points were raised on poles 2½ metres high, provided with insulators, so that each inner coil was about 1·5 m. from the outer one. The apparatus covered a surface area of 900 square metres. From the inner end of this wire an insulated copper wire on poles with telegraph insulators led to the foot of the mountain, where a hut of pine branches was erected; here the wire was connected with a galvanometer, and from this another wire led to a zinc disk in the "earth," *i.e.* in a flowing spring. The elevation of the apparatus above the zinc disk was 180 metres, and the direction of the conducting wire from the mountain north-west to south-east.

From the first day the apparatus was finished, viz. December 5, there appeared almost *every* night a yellow-white luminosity around the summit of the mountain, *while no such luminosity was seen around any one of the others!* The flames were very variable in intensity, and in constant oscillation as those of a liquid fire. Three times it was tested, 2½ miles off in south-east, by a Wrede spectroscope (small size with two prisms), and it returned a faintly continuous spectrum from D to F, in which the auroral line $\lambda = 5569$ with soft variable intensity was observed. The galvanometer steadily gave the deflexions of a positive current from the "utströmnings" apparatus to the earth. The deflexion was so exceedingly variable that the needles were in constant motion when the circuit was closed. A Leclanché's element of ordinary size gave a deflexion which varied according as the positive pole was turned against the mountain or against the earth, but it was always measurable.

It was, however, impossible to continue the observations for any length of time, as hoar-frost quickly developed on the wires and the poles in large quantities, whereby the insulation became affected. The conducting wire from the mountain was of copper insulated with waxed cotton, and 0·8 mm. in diameter. This became so covered with hoar-frost that it broke under the weight in spite of the short distance between the supporting poles, viz. 25 metres. It was, therefore, necessary to examine its entire length before the experiments could be commenced, and as the temperature, as a rule, was under $-30°$ C., our work was greatly impeded.

Although the deflexions, by their great variation in the electric current and perhaps, from changes in the electric forces or the imperfect insulation, cannot be of great scientific value, I subjoin them:—

December 13, 1882.—After several alterations of the galvanometer, and the right sensitiveness having been obtained, the result was, with open circuit: scale-reading[1] of position of equilibrium (*a*) 361·5; (*b*) 362·7; with closed circuit (*c*) 457·8 [or 95·8 parts of the metre = 3°·5, every part of the metre being 2′·2]. This deflexion constantly varied, and often descended to 30 parts of the metre, to rise suddenly again. The temperature was that day comparatively high, viz. $-10°$ to $-12°$ C. The air was hazy.

December 19, 1882.—Plane of equilibrium, 468·0; with closed circuit, 471·6; deflexion = 3·6 parts of the metre.

A Leclanché's element placed in the circuit gave—

1. Plane of equilibrium with open circuit ... ... 469·4
2. With the carbon pole against the earth ... ... 476·4
3. With the carbon pole against the mountain ... ... 467·9

Or the current from the atmosphere = 2·75 parts of the meter.
And the current from the element = 4·25 ,, ,,

The insulation was here worse than in the previous case, and the temperature very low, viz. $-35°$ C.

From December 27 to 29 several experiments were also made, under which the deflexions of the current were greater or less than under the above.

With regard to the construction of the galvanometer it may be mentioned that the instrument consisted of an ordinary wooden frame constructed for the coiling of the wire, while a pair of astatic needles with a mirror could freely swing within the frame thus: one within and the other above the coil. At first the frame was coiled with copper wire 0·5 mm., insulated with indiarubber, but during the experiments this wire was replaced by another 0·4 mm., and insulated with silk impregnated with stearine. The arrangement of the needles was altered several times, but under the above-recorded experiments they were hung on two fine threads of cocoon silk about 20 cm. long and about 1·5 mm. apart. The deflexions were read with a tube and scale from a distance of 0·8 metre.

The Experiments at Pietarintunturi.—On this mountain (lat. 68° 32′·5, long. 27° 17′·3 E. of G.), 950 metres above the sea, a smaller "utströmnings" apparatus was erected in two parts, so that the inner one covered about 80 and the outer 320 square metres. In other respects it

[1] These figures refer to the reading of a divided metre-scale viewed by reflexion in the mirror of the galvanometer by the "subjective" method through a reading-telescope.—[ED.]

was similar to the one at Oratunturi, but the galvanometer here was not so sensitive, as the needles had lost some of their astatic quality through the journey.

On December 29 there appeared above this apparatus *a single column of aurora some* 120 *metres in height!* Its plane was determined from several points, viz., S.S.W. to E.S.E., within an angle of 90° with the plane of the horizon, and there was not the slightest doubt of the aurora appearing just *above* the apparatus. No spectrum analysis was, however, made, as the question of the greatest moment on this occasion was to determine the plane of the column in relation to the apparatus. A faint aurora in the sky at the back of the mountain might perhaps have influenced the analysis; besides which another difficulty arose from the temperature being - 35°, and the instrument in consequence difficult to handle. But that a spectrum similar to the one at Oratunturi and the one of 1871 on the Luosmavaara would have been received I have not the slightest doubt.

The galvanic measurements were effected as follows:— First, the galvanometer was connected with the inner coil of the "utströmnings" apparatus, 80 square metres in area, when deflexion was observed; secondly, it was connected with the outer coil, 320 square metres in area; and thirdly, with both apparatuses combined, or 400 square metres in area.

The deflexions were, in consequence of the small sensitiveness of the galvanometer, weak; but it was clearly noticed that the current increased with the area of the apparatus. As the deflexions varied considerably, it was impossible to ascertain exactly their natural law, but it seemed apparent that the strength of the current was *proportionate to the surface area of the apparatus laid out*. The galvanometer used in this case was similar to the other, but not so sensitive. If I designate the inclinations at Oratunturi with O and those of Pietarintunturi with P, the relative proportion of the sensitiveness of the galvanometers would be

$$P = O \cdot 370,$$

with very nearly the resistance of 20 Siemens units. The metre was here divided into 1''·2. The following shows the results of the experiments:—

December 29, 1882.—With the inner coil, or an area of 80 square metres:—

First deflexion, 2·1 parts of the metre.
Second ,, 0·9 ,, ,,
Third ,, 1·0 ,, ,,
Fourth ,, 2·4 ,, ,,
Fifth ,, 2·0 ,, ,,

The temperature was on that day, as almost during the whole period of experimenting here, −30°, or under. The air was clear.

December 31, 1882.—With the whole surface of the apparatus, or an area of 400 square metres—

First deflexion, 1·6 parts of the metre.
Second ,, 2·8 ,, ,,
Third ,, 3·8 ,, ,,
Fourth ,, 3·1 ,, ,,
Fifth ,, 3·1 ,, ,,

With a Leclanché's element the result was:—

1. With the carbon pole against the mountain deflexion = −0·2
With the zinc ,, ,, ,, +7·5
Of which deflexion by the current from the atmosphere = 3·65
And ,, ,, ,, Leclanché's element = 3·85
2. With the carbon pole against the mountain deflexion = −1·5
With the zinc ,, ,, ,, = 11·9
Of which deflexion by the current from the atmosphere = 5·2
And ,, ,, ,, Leclanché's element = 6·7

The experiments with the different parts of the apparatus resulted as follows:—

1. Plane of equilibrium (*a*) 271·6, (*b*) 271·4 } = 271·5 open circuit.
Inner apparatus A = 80 square metres.
(*a'*) 271·5, (*b'*) 271·7 } = 271·6 closed circuit.
Plane of equilibrium (*c*) = 270·4 open circuit.
Deflexion from A = 0·6.
Outer apparatus B = 320 square metres.
(*a*) Plane of equilibrium = 270·4 open circuit.
(*a'*) ,, ,, 271·8, (*b'*) ,, ,, 271·1 } = 271·5 closed circuit.
(*b*) ,, ,, = 270·1 closed circuit.
Deflexion from B = 1·2.
Entire apparatus C = 400 square metres.
(*a*) Plane of equilibrium = 270·1 open circuit.
(*a'*) ,, ,, 270·9, (*b'*) ,, ,, 270·7 } = 270·8 closed circuit.
(*b*) ,, ,, = 269·7 open circuit.
Deflexion for C = 0·9.

During these experiments it was noticed that the first deflexion was always far greater than the subsequent double, constant ones, and I have therefore noted this one only. When the circuit was closed it was observed that the needle pair always was at rest in the plane of equilibrium.

2. A. Plane of equilibrium 269·7 open circuit.
(*a'*) ,, ,, 270·8 closed circuit, 1st defl. 277·0
(*b*) ,, ,, 269·8 open circuit.
Deflexion for A = 1·0 ... ... 1st defl. = 7·2
B. (*a*) Plane of equilibrium 269·8 open circuit.
(*a'*) ,, ,, 271·3 closed circuit, 1st defl 281·0
(*b*) ,, ,, 270·4 open circuit.
Deflexion for B = 1·2 ... ... 1st defl. = 11·8
C. (*a*) Plane of equilibrium 270·4 open circuit.
(*a'*) ,, ,, 271·8 closed circuit, 1st defl. 284·9
(*b*) ,, ,, 270·4 open circuit.
Deflexion for C = 1·4 ... ... 1st defl. = 14·5

3. A. (*a*) Plane of equilibrium 270·4 open circuit.
(*a'*) ,, ,, 271·7 closed circuit, 1st defl. 278·2
(*b*) ,, ,, 270·8 open circuit.
Deflexion for A = 1·1 ... ... 1st defl. = 7·6
B. (*a*) Plane of equilibrium 270·8 open circuit.
(*a'*) ,, ,, 274·0 closed circuit, 1st defl. 282·0
(*b*) ,, ,, 273·6 open circuit.
Deflexion for B = 1·8 ... ... 1st defl. = 10·8
C. (*a*) Plane of equilibrium 274·5 open circuit.
(*a'*) ,, ,, 279·0 closed circuit, 1st defl. 292·3
(*b*) ,, ,, 275·6 open circuit.
Deflexion for C = 3·9 ... ... 1st defl. = 17·2

If these observations are connected we obtain:—

A = 80 square metres
B = 320 ,,
C = 400 ,,

	Deflexion	1st. defl.	Deflexion.	1st defl.	Deflexion.	1st defl.
1.	0·6	—	1·2	—	0·9	—
2.	1·0	7·2	1·2	11·8	1·4	14·5
3.	1·1	7·6	1·8	10·8	3·9	17·2

If it now be considered that the current from the atmosphere was very variable, besides that the insulation was rather imperfect, the results of these experiments are not so reliable as might be desired. Still, this inference can safely be drawn from them, that the strength of the current *increases* with the area of the "utströmnings" apparatus or with the number of points, but the natural law of the increase I have not yet been able to determine. As will be seen from the subjoined figures the conductive power of the wire was that day very small, although the temperature was comparatively high, viz. at 1 a.m. − 18° C., but falling.

The entire apparatus (C) with one of Leclanché's elements:—

(*a*) Plane of equilibrium 273·1 open circuit.
(*a'*) ,, ,, 275·3 the zinc pole against the mountain.
(*b'*) ,, ,, 270·4 carbon ,, ,,
(*b*) ,, ,, 270·1 closed circuit.

From this we obtain: the current from the atmosphere = 1·5, and from the element = 2·45, which figures show the small conducting power of the wire, as well as the weak current from the atmosphere.

Another inference may be drawn from these observations. If we take the average of the results on December 31 on Pietarintunturi, when the whole apparatus was used, it will be 3·2 parts of the metre, and comparing this with that of December 19 on Oratunturi, when the atmospheric conditions were similar, which was 3·6 parts of the metre, and transform these into minutes, the result will be as follows: 3·6 parts of the metre at 2′·2 = 7·92; 3·2 parts of the metre at 1′·2 = 3′·84, but the sensitiveness of the galvanometer at Pietarintunturi was only 0·36 of that at Oratunturi = 0·37, and the area of the apparatus in the former 200 square metres against 900 in the latter, and further, assuming that the current increases in proportion to the area of the apparatus, we shall have:

$$\frac{3'\cdot 84}{0\cdot 36}\ \frac{900}{400} = 24'\cdot 0.$$

And the deflexion 3′·84 being reduced to the same galvanometer sensitiveness and the same area of apparatus, the actual result is that the experiments at Oratunturi showed a deflexion *three times* greater than those at Pietarintunturi. The latter place is certainly situated a little higher than the former, but in my opinion the increase of the electric force lies in the fact that Oratunturi is in a higher latitude than Pietarintunturi, *i.e.* nearer the plane of the aurora borealis. Although the experiments recorded above suffer from inaccuracy on account of the imperfect insulation, I have come to the conclusion *that the electric current from the atmosphere increases rapidly with the latitude.*

The great deflexion which I obtained at Oratunturi on December 13, 1882, I do not consider refutes this inference, as the atmospheric conditions on this occasion were exceptional, viz. the temperature high and the air hazy.

The experiments in both places have, however, unfortunately been of a somewhat provisional character, which is due to the external impediments in our way. Thus, when experimenting at Oratunturi, the writer had to make a journey in the snow of 20 kilometres, viz. of four hours' duration, then to examine the apparatus on the summit, clean it from hoar-frost, and often repair it, with the thermometer at − 30° C. Then only could the experiments be commenced. It was only possible to work for five to eight minutes at a time, as it was necessary to thaw one's benumbed hands before a bonfire lit on the snow. At Pietarintunturi the road was certainly shorter, but, nevertheless, very fatiguing, as it was necessary first to climb a ridge about 1000 feet, and then journey about 3 kilometres.

These difficulties, and chiefly the imperfect insulations and the weakness of the wires at my disposal, compelled me to abandon experiments of this character.

SELIM LEMSTRÖM

Professor of the Helsingfors University

(*To be continued*)

NOTES

BESIDES Prof. Huxley the following English men of science have been elected Foreign Associates of the U.S. National Academy of Sciences:—Prof. J. C. Adams, Prof. Cayley, Prof. Sylvester, Prof. Stokes, Sir William Thomson, and Sir J. D. Hooker.

MR. HERBERT SPENCER has been elected a Corresponding Member of the Paris Academy of Moral and Political Sciences.

THE remarkable enthusiasm with which the project of the memorial to Charles Darwin was received in Sweden has already been noticed in our columns. The amount of the subscriptions collected, as was said, from all ranks, has just been received by the treasurer of the Darwin Memorial Fund. It is a sum of 382*l.* 12*s.* 6*d.*, the largest, we believe, that has been contributed by any foreign country, and a proof of the zeal on behalf of science that exists in the land of Linnæus.

WE are glad to learn that America has at length subscribed for a table at the Zoological Station at Naples. In view of the very considerable number of American students in European biological laboratories some surprise has naturally been felt that America has not hitherto been represented at Naples. President Carter and the trustees of Williams College are to be congratulated on having taken the lead in a matter the importance of which must be apparent to all who are interested in the progress of morphological study.

DR. WILLIAM CHAMBERS, the head of the eminent publishing firm, well deserves the honour of a baronetcy which he has just received, on account of the public services rendered by him to education and to social improvement throughout a long life; he is just the age of the century, we believe.

The public sale of the late Prof. J. Decaisne's library will take place in Paris from June 4 to 23 next. The catalogue of 480 pages, published by Labitte, of Paris, contains more than 5000 entries, classified according to subject by M. Vesque, assistant to the late M. Decaisne. It is probably one of the finest libraries in botany, horticulture, and general natural history which has been sold since the death of Jussieu. The catalogue contains a portrait of Decaisne and a biography by Dr. E. Bornet.

THE death is announced, at the age of seventy-one years, of Mr. James Young of Kellie, the "Sir Paraffin" of his old friend Livingstone. Mr. Young is best known in connection with his process of distillation of paraffin oil from bituminous coal, which attained great dimensions, and from which he realised a fortune. Mr. Young took a real and active interest in chemical research, and founded the Chair of Economic Chemistry in Anderson's University, Glasgow; he was a Fellow of the Royal Society.

THE departure of the Swedish Expedition to Greenland has been postponed to the 22nd, and Baron Nordenskjöld will join the *Sofia* at Gothenburg, instead of coming to Scotland.

A HYGIENIC exhibition was opened at Berlin on Saturday.

THE Society of Arts *conversazione* will this year be held in the buildings of the International Fisheries Exhibition; the Prince of Wales, the President of the Society, has intimated his intention of being present. The date is not yet announced.

IN connection with the recent discussion on the opening of picture galleries and museums on Sundays, the following facts relating to the Whitechapel Fine Art Exhibition are full of interest. This exhibition, which as may not be known to all our readers, is one which is open for thirteen days at Easter in one of the most desolate parts of this great metropolis. It consists of about two hundred pictures of the highest order of merit, which are placed at the disposal of a responsible committee by the artists or those who are fortunate enough to possess them. It is open gratuitously from ten in the morning until ten at night, except on Sundays, when the opening takes place at two o'clock, after morning service. This year, it will be seen from the numbers we give below, that no less than 34,644 of the poorest of the poor visited the pictures; and as they were to a very large extent "personally conducted" round the rooms by ladies and gentlemen who freely gave up their time to the work, the way in which they appreciated the pictures is thoroughly well known. The same men and women came again and again, bringing their friends to show them the pictures in which they themselves had taken the greatest interest. One of the most important points that we wish to urge now is, that on the last day the exhibition was open, which was Sunday, between the hours of two and ten more than 3000 working men and women visited the collection, and we are informed that when the rooms were most crowded, there was always not only absolute order and good temper, but a reverence for the spirit of the place. This, we think, is a sufficient reply to those who say that if picture galleries and museums were opened on Sunday, they would not be visited. Seeing that a love of science and nature must be at the bottom of all true love for art, we feel ourselves bound to thank Mr. Barnett and those who have helped him in this humanising work; and as it is known with what sympathy artists and possessors of pictures placed them at the disposal of the committee, we think it a pity that the Whitechapel example is not more generally followed. It is not necessary to give the numbers for 1882, but we may just say that very nearly 10,000 more people visited the exhibition this year, which clearly shows that the interest taken in it is not a transient one, but one which increases from year to year. And the figures do not do justice to the success of the exhibition, for they mean something more than they would at an exhibition in the West End; the Whitechapel people went to see, and they made a business of seeing. The attendances were as follows:—

Tuesday	March 20	933
Wednesday	,, 21	2,094
Thursday	,, 22	1,431
Good Friday	,, 23	2,722
Saturday	,, 24	2,581
Easter Sunday	,, 25	1,632
,, Monday	,, 26	3 369
,, Tuesday	,, 27	3,304
Wednesday	,, 28	3,523
Thursday	,, 29	3,212
Friday	,, 30	2,681
Saturday	,, 31	4,045
Sunday	April 1	3,117
	Total for 13 days	34,644

AT the first meeting of the Sociological Section of the Birmingham Natural History and Microscopical Society for the study of Mr. Herbert Spencer's "System of Philosophy" held last week at the Mason College, the President (Mr. W. R. Hughes) explained that the new Section had originated in a wish to unite, for the purposes of mutual help, those who were already students of Mr. Herbert Spencer's system, but were unknown to each other; and to introduce to the synthetic philosophy those already engaged in some special biological study, but as yet unfamiliar with the principles common to all departments of natural history. He read a letter from Mr. Herbert Spencer expressing cordial sympathy with the objects of the Section, and adding some valuable suggestions as to the course of work to be undertaken by the Section. Whether we admitted or rejected Mr. Spencer's principles, the President said, there was no doubt of the wonderful influence they were exercising in this country, on the Continent, and in America. He enumerated many reasons why Birmingham was peculiarly adapted for the study of sociology, saying it was central, healthy, industrious, and earnest in all it undertook, active in reform, versatile in its trades, and therefore free from commercial panics, many-sided in religion, untiring in political activity. During the present century no town had exhibited a more remarkable social development, and therefore there was no town more fit for the study of sociology. Its development was of a type peculiar to a large industrial organisation, and was in marked contrast to that kind of development which would obtain under a military or ecclesiastical or agricultural organisation. Sociological generalisations made there might therefore be regarded as typical and unique. The President's address was followed by a discussion upon the first two chapters of the "Essay on Education."

MR. CLEMENT L. WRAGGE has undertaken to reorganise the meteorological work at the Ben Nevis Observatory, which he first commenced about two years ago, under the auspices of the Scottish Meteorological Society, and hopes to have the observing system reopened and in order by June 1. Mr. William Whyte, of Fort William (formerly assistant), will then receive further instruction from Mr. Wragge, and will take charge, having been appointed by the Society to carry on the work during the summer of the present year, in consequence of Mr. Wragge's intention to resume his travels in the course of a few months, and to revisit Australia. The voyage will be made a scientific one, and Mr. Wragge hopes to add largely to his natural history and ethnographical collections now at Stafford. He is arranging to carry on ocean meteorological observations on a large scale, following mainly the plan adopted by the *Challenger* expedition. Negretti and Zambra's new deep-sea thermometers are to be employed.

THE German gunboat *Hyäne* visited Easter Island last autumn, and determined its exact position, which was found to be 27° 10′ S. lat., and 109° 26′ W. long. The commander of the *Hyäne*, Capt. Geiseler, has reported minutely to the German Admiralty Office on the ethnology of the island, and this report is accompanied by numerous drawings of old colossal statues, stone houses, monuments, tombs of chiefs, &c. At the same time Capt. Geiseler made a collection of ethnological specimens which has been forwarded to Germany by way of Apia. The report is now printed and published by Mittler and Sohn (Berlin).

PROF. BASTIAN has been nominated honorary president of the Berlin Geographical Society. The following gentlemen have been elected as honorary members: Prof. von Richthofen, Dr. Gustav Nachtigal, Prof. Neumayer, Dr. Pogge, Dr. Buchner, and Lieut. Wissmann. The latter has also received the Society's Silver Knights-Medal.

AT Berlin an aurora borealis was observed on April 29 at 9 p.m. The phenomenon brightened up the whole sky, across which numerous bright red cloud-streaks seemed to shoot.

MR. ERNEST GILES, the explorer, contemplates organising a grand final expedition to traverse the remaining unexplored portions of the Australian continent, and to endeavour to discover some more trustworthy traces of Leichhardt.

IN the "Publications of the Massachusetts Society for the Promotion of Agriculture," Mr. S. H. Scudder has given an interesting account of the habits of a small moth (*Retinia frustrana*), and of the ravages caused by it on the pitch pine of Nantucket Island (*Pinus rigida*). Of late it has become so abundant as to threaten the total destruction of the pines. Like its European congeners its larvæ bore into the interior of the healthy young shoots and destroy them. The remedy recommended is the radical one of taking off from every tree those shoots that show themselves to be infested, but the author is fully alive to the difficulties attendant upon such a recommendation, especially those of expense. The insect has not yet made its appearance on the adjoining mainland, but it seems to have been observed in other more distant parts of the Eastern States. In Europe (and indeed in Britain) much damage is done to conifers (especially Scotch fir) by allied species, and they more especially infest quite young trees. Some of them principally affect the lateral shoots, and these, if not too numerous, cause no lasting injury to healthy young trees; but one especially (*R. turionella*) attacks the leading shoot, and is far more serious; in this case, if the tree be strong and healthy, a lateral shoot takes the place of the destroyed "leader," and recovery is effected by this means.

NEWS has at last been received from Dr. Pogge, the companion of Lieut. Wissmann, on his journey across Africa, and who remained in Africa after Wissmann left. It appears that

Dr. Pogge reached the Mukenge safely in September last, bringing large collections with him. He had written and sent to Malange for means for his return journey.

A REPORT on the Peter Redpath Museum, Montreal, the foundation of which was laid by the Marquis of Lorne in September, 1880, describes the opening ceremony in August, during the meeting of the American Association. Mr. Redpath in a very few words handed over the Museum to the University, and speeches were made by the Chancellor, Dr. Carpenter, Prof. Hall, and Dr. Dawson. Already collections have been placed in the Museum, which promises to become one of the first rank.

THE current number of the *Agricultural Students' Gazette*, edited by students at the Royal Agricultural College, Cirencester, contains an instructive article on Devonshire Orchards and Cider-making, by C. B. Northcote, a member of the College. Miss Ormerod contributes a paper on the Coffee Grub in Ceylon, embodying our information on this pest up to the present time, from information largely derived from a pamphlet by Mr. Haldane on the subject. Mr. Rutherford gives a concise paper on the Agriculture of the Cotswolds; Prof. Garside one on Glanders, adducing evidence that it is a germ disease due to a bacillus. There is also an interesting and instructive collection of chemical curiosities, answers to examination questions; and in addition reports on the experimental field plots, on the weather, on the amount of chlorine in the rain water of the district, and on many other more purely college matters. This magazine fully keeps up to its advanced standard, and has a value in a circle far wider than its immediate connection with the Agricultural College.

WE have received the *Proceedings of the Medical Society* of the Kazan University, which contains, besides purely medical papers, several valuable papers of general interest. We notice among them a lecture, by Prof. Scherbakoff, on carbonic and azulmic acids in the soil as a measure of the oxidation of its organic constituents. It is known that since more attention has been given to the sanitary conditions of different soils, Herr Pettenkofer has proposed to measure the amount of putrefied organic matter in the soil by the amount of carbonic acid it contains. Prof. Scherbakoff makes a complete analysis of the chemical and putrefactive processes that are going on in the soil, and comes to the conclusion that, unhappily, the carbonic acid does not give a measure either of the oxidating capacity of the soil or of the decomposition of the organic matter. The same conclusion is arrived at with regard to azulmic acid, which is formed only under the action of special ferments, as appears from the classical researches of MM. Schlesing, Müntz, and Pasteur, so that oxidation of the organic elements of the soil may go on on a large scale without azulmic acid appearing as a result of the process. We notice also a paper, by M. Orloff, on the influence of wet and dry chlorine upon different materials when used for disinfection, the author giving the results of a series of experiments on various linen, cotton, silk, and woollen stuffs. The tables of diseases at Kazan and in several districts of the province are also of great interest; they show, for instance, that the number of cases of malarial fever is really enormous, as it has reached, in the town of Kazan, the figure of 23,000 cases during five years. As to cattle and horse diseases, their number is still more striking, as every year the province loses no less than 4300 to 4600 head of horned cattle, to which must be added sometimes—as in 1877—3250 cattle and horses exterminated by the Siberian plague.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. F. J. Wicks; a Ring-tailed Coati (*Nasua rufa*), a Kinkajou (*Cercoleptes caudivolvulus*) from Demerara, presented by Mr. Ernest Francis; a Herring Gull (*Larus argentatus*), British, presented by Mrs. Andrews; a Smooth Snake (*Coronella lævis*), European, presented by Mr. W. A. B. Pain; a Bateleur Eagle (*Helotarsus ecaudatus*) from Africa, two Germain's Peacock Pheasants (*Polyplectron germaini*) from Cochin China, purchased; a Bennett's Wallaby (*Halmaturus bennetti*), four Brown-tailed Gerbillus (*Gerbillus erythrurus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

D'ARREST'S COMET.—The following approximate positions of this comet are deduced from M. Leveau's elements:—

At Greenwich Midnight

	R.A. h. m. s.	Decl. ° ′	Log. Distance from Earth.	Log. Distance from Sun.
May 25	13 13 47	+13 8·9	0·2983	0·4312
27	— 12 51	13 6·9		
29	— 12 0	13 4·9	0·3015	0·4267
31	— 11 15	13 1·9		
June 2	— 10 35	12 58·0	0·3051	0·4221
4	— 10 1	12 53·2		
6	— 9 34	12 47·6	0·3090	0·4175
8	— 9 13	12 41·1		
10	13 8 57	+12 33·9	0·3132	0·4128

THE OBSERVATORY OF RIO JANEIRO.—We have received the *Bulletin Astronomique et Météorologique de l'Observatoire Impérial de Rio de Janeiro* for January and February. In the first number are observations of the nucleus of the great comet of 1882 made by M. Lacaille. While stationed at Olinda (Pernambuco) for the observation of the transit of Venus, he remarked on November 16 a small nebulosity 6° south of the nucleus of the great comet: it was circular, and had a slight central condensation. On November 20 he saw it again; its aspect was the same as on the previous day, it had the same right ascension, but its declination was 1° further south. On November 22 and 26 it was observed in the same position as on the 20th. M. Lacaille believes that this small nebulosity was no other than a fragment of the nucleus of the great comet. On returning to Rio, he found on January 8, on examining this nucleus with the 10-inch equatorial and power of 500, that it was highly elongated and subdivided into four small nebulosities, the centres of which had the appearance of stars of the twelfth magnitude. The aspect of the fourth as compared with the others, was less condensed, but rather more lengthened out. On the following night he was surprised to find that the first nebulosity was no longer in the position that he had seen it on the 8th, but that it was situate outside the elongated nucleus, and its centre had lost the appearance of a star of the twelfth magnitude. The second nebulosity was precisely in the position of the day preceding. The fourth had sensibly approached the third. On January 10 the four nebulosities retained the same relative positions. Several days of cloudy weather followed, but on January 15 he found that there was a fifth nebulosity in the elongated nucleus. These changes are well shown in a lithograph accompanying M. Lacaille's observations. In the February number of the *Bulletin* are observations of the same comet, made at Athens by Dr. Julius Schmidt, as detailed in a letter addressed by him to the Emperor of Brazil. It relates chiefly to the nebulosities which were remarked by Dr. Schmidt in the vicinity of the nucleus of the great comet on October 9, 10, and 11, his drawings showing the fantastic forms presented by the nebulosities being lithographed.

THE OBSERVATORY OF MOSCOW.—Volume IX. (livraison i.) of *Annales de l'Observatoire de Moscou*, has been issued. Amongst the contents are a short paper by M. Bredichin on the resisting medium; Researches on the first comet of 1882 (Wells), and observations of the minor planet Victoria, taken in connection with others to be made at the Cape and other southern as well as northern observatories, as part of a plan organised by Dr. Gill, for the determination of a new value of the solar parallax. M. Bredichin compares the observed phenomena of the tail of the first comet of 1882 with the indications of theory.

KIELL ON TYCHO BRAHE'S NOVA 1572.—It has often been stated in our astronomical text-books, that John Kiell, Professor of Astronomy at Oxford, considered that the period of the celebrated star in Cassiopeia in 1572, was "about 150 years," or only half that which had been more generally assigned to it.

We suspect that this statement has arisen from a misconception of Kiell's words, while referring to the star in his *Introduction to the true Astronomy or Astronomical Lectures*, &c., the first English edition of which appears to have been published in 1721. In the third edition, 1739 (which is before us), at p. 56, we read, after his reference to the phenomena in 1572, "Leovitius, from the history of those times, tells us that in the time of the Emperor Otho, about the year 945, a new star appeared in Cassiopeia, just such a one as was seen in his time in the year 1572. And he brings us another ancient observation—that there was likewise seen in the *northern* region of the heavens, near the constellation Cassiopeia, in the year 1264, an eminently bright *star*, which kept itself in the same place, and had no proper motion. It is probable that these two stars might have been the same with that which was seen by Tycho, and that in about 150 years the same star may again make its appearance."

It will be remarked that Kiell makes no reference to any star seen midway between 945 and 1264, nor between 1264 and 1556, and it seems his meaning is clear, that Tycho's star, with a period of some 300 years, might make its appearance again "in about 150 years" from the time at which he wrote, as it might do were its changes accomplished in about three centuries. The misinterpretation of Kiell's words has led to his being credited with the opinion that the period is about 150 years, an idea which he probably never intended to express.

ELECTRICITY APPLIED TO EXPLOSIVE PURPOSES[1]

IN introducing the subject the lecturer indicated the principal advantages which it had been early observed would result from a certain mode of firing explosive charges by electric current instead of by the ordinary fuzes, the best of which had inherent defects, greatly limiting their use for any but the simplest operations. He traced the history and development of electric firing from the crude experiments of Benjamin Franklin, about the year 1751, through the various stages in which frictional electricity, volta-induction apparatus, and magnetoelectric machines had supplied the means of generating the current, the tendency of late years being to revert to a modified form of voltaic battery for one class of work, and to employ dynamo-electric machines for another class. The history and development of the low-tension or wire fuze, and of the various fuzes employed with electric currents of high tension, were also discussed, and their relative advantages, defects, and performances were described.

The only sources of electricity which at present thoroughly fulfilled the conditions essential in the exploding-agent for submarine mines were constant voltaic batteries. They were simple of construction, comparatively inexpensive, required but little skill or labour in their production and repair, and very little attention to keep them in constant good working order for long periods, and their action might be made quite independent of any operation to be performed at the last moment.

When first arrangements were devised for the application of electricity in the naval service to the firing of guns and so-called outrigger charges, the voltaic pile recommended itself for its simplicity, the readiness with which it could be put together and kept in order by sailors, and the considerable power presented and maintained by it for a number of hours. Different forms of pile were devised at Woolwich for boat and ship use, the latter being of sufficient power to fire heavy broadsides by branch circuits, and to continue in a serviceable condition for twenty-four hours, when they could be replaced by fresh batteries, which had in the meantime been cleaned and built up by sailors.

The Daniell and sand batteries first used in conjunction with the high-tension fuze for submarine mining service were speedily replaced by a modification of the battery known as Walker's, which was after some time converted into a modified form of the Leclanché battery.

The importance of being able to ascertain by tests that the circuits leading to a mine, as well as the fuzes introduced into that circuit, were in proper order, very soon became manifest; and many instances were on record in the earlier days of submarine mining of the disappointing results attending the accidental disturbance of electric firing arrangements, when proper means had not been known or provided for ascertaining whether the circuit was complete, or for localising any defect when discovered.

The testing of the Abel fuze, in which the bridge, or igniting and conducting composition, was a mixture of the copper phosphide and sulphide with potassium chlorate, was easy of accomplishment (by means of feeble currents of high tension), in proportion as the sulphide of copper predominated over the phosphide. Even the most sensitive might be thus tested with safety; but when the necessity for repeated testing, or even for the passing of a signal through the fuze, arose, as in a permanent system of submarine mines, the case was different, this fuze being susceptible of considerable alterations in conductivity on being frequently submitted to even very feeble test-currents, and its accidental ignition by such comparatively powerful test- or signal-currents as might have to be employed, became so far possible as to create an uncertainty which was most undesirable.

Hence, and also because the priming in these fuzes was liable to some chemical change detrimental to its sensitiveness, unless thoroughly protected from excess of moisture, another form of high-tension fuze, specially adapted for submarine mining service, was devised at Woolwich. This, though much less sensitive than the original Abel fuze, was sufficiently so for service requirements, while it presented great superiority over the latter in stability and uniformity of electric resistance; and, though not altogether unaffected by the long-continued transmission of test-currents through it, the efficiency of the fuze was not affected thereby.

Although high-tension fuzes presented decided advantages in point of convenience and efficiency over the earlier form of platinum wire fuze, the requirements which arose, in elaborating thoroughly efficient permanent systems of defence by submarine mines, and the demand for a battery for use in ships which would remain practically constant for long periods, caused a very careful consideration of the relative advantages of the high- and low-tension systems of firing to result in favour of the employment of wire fuzes for these services. In addition to the disadvantages pointed out there was an element of uncertainty, or possible danger, in the employment of high-tension fuzes, which, though partly eliminated by the adoption of voltaic batteries, in place of generators of high-tension electricity, might still occasionally constitute a source of danger, namely, the possibility of high-tension fuzes being accidentally exploded by currents induced in cables, with which they were connected, during the occurrence of thunderstorms, or of less violent atmospheric electrical disturbances.

Experiment, and the results obtained in military service-operations, had demonstrated that if insulated wires, immersed in water, buried in the earth, or even extended on the ground, were in sufficient proximity to one another, each cable being in circuit with a high-tension fuze and the earth, the explosion of any of the fuzes by a charge from a Leyden jar, or from a dynamo-electric machine of considerable power, might be attended by the simultaneous ignition of fuzes attached to adjacent cables, which were not connected with the source of electricity, but which become sufficiently charged by the inductive action of the transmitted current. It therefore appeared very possible that insulated cables extending to land- or submarine-mines, in which high-tension fuzes were inclosed, might become charged inductively during violent atmospheric electrical disturbances to such an extent as to lead to the accidental explosion of mines with which they were connected. In a Report by von Ebner on the defence of Venice, Pola, and Lissa, by submarine mines, in 1866, he refers to the accidental explosion of one of a group of sixteen mines during a heavy thunderstorm, as well as to the explosion of some mines, by the direct charging of the cables, through the firing station having been struck by lightning. Two instances of the accidental explosion of tension fuzes by the direct charging of overhead wires during lightning discharges occurred in 1873 at Woolwich.

Subsequently an electric cable was laid out at Woolwich along the river bank below low-water mark, and a tension fuze was attached to one extremity, the other being buried. About eleven months afterwards the fuze was exploded by a charge induced in the conductor during a very heavy thunderstorm.

In consequence of such difficulties as these experienced in the special application of the high-tension fuzes to submarine purposes, the production of comparatively sensitive low-tension fuzes, of much greater uniformity of resistance than those employed in former years, was made the subject of an elaborate

[1] The fifth of the series of Six Lectures on the Applications of Electricity, delivered on Thursday evening, April 19, at the Institution of Civil Engineers, by Sir F. A. Abel, F.R.S., Hon.M.Inst.C.E.

experimental investigation by the lecturer. Different samples of comparatively thin wires, made from commercial platinum, showed very great variations in electrical conductivity. Very considerable differences in the amount of forging to which the metal, in the form of sponge, had been subjected, did not importantly affect either its specific gravity or its conductivity, and the fused metal had only a very slightly higher degree of conductivity than the same metal forged from the sponge. The conductivity of very fine wires could therefore be but slightly affected by physical peculiarities of the metal, and the considerable differences in conductivity observed in different samples of platinum were therefore chiefly ascribable to variations in the degree of its purity. It appeared likely that definite alloys might furnish more uniform results than commercial platinum; experiments were therefore made with fine wires of German silver and of the alloy of sixty-six of silver with thirty-three of platinum employed by Matthiessen for the reproduction of B.A. standards of electrical resistance. Both were greatly superior to ordinary platinum in regard to the resistance opposed to the passage of a current; German silver was in its turn superior to the platinum-silver alloy; although the difference was only trifling in the small lengths of fine wire used in a fuze (0·25 inch), while the comparatively ready fusibility of platinum-silver contributed, with other physical peculiarities of the two alloys, to reduce German silver to about a level with it. Moreover, the latter did not resist the tendency to corrosive action exhibited by gunpowder and other more readily explosive agents which had to be placed in close contact with the wire bridge in the construction of a fuze, while the platinum-silver alloy was found to remain unaltered under corresponding conditions. Experiments having also been made with alloys of platinum with definite proportions of iridium, the metal with which it is chiefly associated, very fine wires of an alloy containing 10 per cent. of iridium were eventually selected as eminently the best materials for the production of wire fuzes of comparatively high resistance and uniformity, this alloy being found decidedly superior in the latter respect as well as in point of strength (and therefore of manageableness in the state of very fine wire 0·001 inch in diameter) to the platinum-silver wire. The fuzes now used in military and submarine services were made with bridges of iridio-platinum wire containing 10 per cent. of the first-named metal.

The electrical gun-tubes in the navy were fired by means of a specially arranged Leclanché battery, and branch circuits worked to the different guns; in broadside firing it was important that the wire bridge of any one of the gun-tubes which was first fired should be instantaneously fused on the passage of the current, so as to cut this branch out of circuit; in this respect the comparatively fusible platinum-silver alloy appeared to present an advantage, hence the naval electrical fuzes were made with bridges of that alloy. Uniformity of electrical resistance had become a matter of such high importance in the delicate arrangements connected with the system of submarine mines, as now perfected, that the very greatest care was bestowed upon the manufacture of service electric fuzes and detonators, which were in fact made in all their details with almost the precision bestowed upon delicate scientific instruments, and the successful production of which involved an attention to minutiæ which would surprise a superficial observer.

One of the earliest applications of electricity to the explosion of gunpowder was the firing of guns upon proof at Woolwich by means of a Grove battery and a gun-tube, which was fired by a platinum wire bridge, a shunt arrangement being used for directing the current successively into the distinct circuits connected with the guns to be proved. When the high-tension fuze had been devised, gun-tubes were made to which it was applied, and an exploder was arranged by Wheatstone, having a large number of shunts, so that as many as twenty-four guns might be brought into connection with the instrument, and fired in rapid succession by the depression of separate keys connected with each.

The firing of cannon as time-signals was an ancient practice in garrison towns, but the regulation of the time of firing the gun by electrical agency from a distance appears first to have been accomplished in Edinburgh, where, since 1861, the time-gun had been fired by a mechanical arrangement actuated by a clock, the time of which is controlled electrically by the mean-time clock at the Royal Observatory on Calton Hill.

Shortly after the establishment of the Edinburgh time-gun, others were introduced at Newcastle, Sunderland, Shields, Glasgow, and Greenock. The firing of the gun was arranged for in various ways; in some instances it was effected either direct from the Observatory at Edinburgh, or from shorter distances, by means of Wheatstone's magnetoelectric exploders. At present there were time-guns at West Hartlepool, Swansea, Tynemouth, Kendal, and Aldershot, which were fired electrically, either by currents direct from London, or by local batteries, which were thrown into circuit at the right moment by means of relays, controlled from St. Martin's-le-Grand.

About thirteen years ago the electrical firing of guns, especially for broadsides, was first introduced into the navy, with the employment of the Abel high-tension gun-tube and voltaic piles. The gun-tubes then used were manufactured originally for the proof of cannon and for experimental artillery operations, and were of very simple and cheap construction. Experience proved them to be unfitted to withstand exposure to the very various climatic influences which they had to encounter in Her Majesty's ships, and in store in different parts of the world. The low-tension gun tubes, having a bridge of very fine platinum-silver wire, surrounded by readily ignitible priming composition, was therefore adopted as much more suitable for our naval requirements.

The arrangements for broadsides or independent firing, and also for the firing of guns in turret-ships, had been very carefully and successfully elaborated in every detail, including the provision of a so-called drill- or dummy electrical gun-tube, which was used for practice and refitted by well instructed sailors. The firing-keys, and all other arrangements connected with electrical gun-firing, were specially designed to insure safety and efficiency at the right moment.

The electric detonators for firing outrigger-torpedoes, or for other operations to be performed from open boats, corresponded, so far as the bridge was concerned, with the naval electric gun-tubes, and were fired with a specially fitted Leclanché battery. These electric appliances were now distributed throughout the navy, and the men were kept, by instruction and periodical practice, well versed in their use.

The application of electricity to the explosion of submarine mines, for purposes of defence and attack, received some attention from the Russians during the Crimean War, under the direction of Jacobi; thus a torpedo, arranged to be exploded electrically when coming into collision with a vessel, was discovered at Yeni-Kale during the Kertsch expedition in 1855. Some arrangements were made by the British at the conclusion of the war to apply electricity to the explosion of large powder-charges for the removal of sunken ships, &c., in Sebastopol and Cronstadt Harbours. In 1859, a system of submarine mines, to be fired through the agency of electricity by operators on shore, was arranged by von Ebner for the defence of Venice, which, however, never came into practical operation. Early in 1860 Henley's large magnetoelectric machine, with a supply of Abel fuzes, and stout indiarubber bags, with fittings to resist water-pressure, was despatched to China for use in the Peiho River, but no application appeared to have been made of them. The subject of the utilisation of electricity for purposes of defence, however, did not receive systematic investigation in England or other countries until some years afterwards, when the great importance of submarine mines, as engines of war, was demonstrated by the number of ships destroyed and injured during the war in America.

The application of electricity to the explosion of submarine mines was very limited during that war, but arrangements for its extensive employment were far advanced in the hands of both the Federals and Confederates at the close of the war, men of very high qualifications, such as Capt. Maury, Mr. N. J. Holmes, and Capt. McEvoy having worked arduously and successfully at the subject.

The explosion of submerged powder-charges, by mechanical contrivances, either of self-acting nature or to be set into action at desired periods, was accomplished as far back as 1583, during the siege of Antwerp, by the Duke of Parma, and from that period to 1854 mechanical devices of more or less ingenious and practicable character had been from time to time applied to some small extent, in different countries, to the explosion of torpedoes. The Russians were the first to apply self acting mechanical torpedoes with any prospect of success, and had the machines used for the defence of the Baltic been of larger size (they only contained 8 or 9 lb. of gunpowder), their presence would probably have proved very disastrous to some of the English ships which came into collision with and exploded them.

Various mechanical devices for effecting the explosion of torpedoes by their collision with a ship were employed by the Americans, a few of which proved very effective. But although in point of simplicity and cost, a system of defence by means of mechanical torpedoes possessed decided advantages over any extensive arrangements for exploding submarine mines by electric agency, their employment was attended by such considerable risk of accident to those at whose hands they received application that, under many circumstances which were likely to occur, they became almost as great a source of danger to friend as to foe.

The most important advantages secured by the application of electricity as an exploding-agent of submarine mines were as follows:—They might be placed in position with absolute safety to the operators, and rendered active or passive at any moment from the shore; the waters which they were employed to defend were therefore never closed to friendly vessels until immediately before the approach of an enemy; they could be fixed at any depth beneath the surface (while mechanical torpedoes must be situated directly or nearly in the path of a passing ship), and they might be removed with as much safety as attended their application.

There were two distinct systems of applying electricity to the explosion of submarine mines. The most simple was that in which the explosion was made dependent upon the completion of the electric circuit by operators stationed at one or more posts of observation on shore; such a system depended, however, for efficiency, on the experience, harmonious action, and constant vigilance of the operators at the exploding- and observing-stations, and was, moreover, entirely useless at night, and in any but clear weather.

The other, which might also be used in conjunction with the foregoing, was that of self-acting mines, exploded either by collision with the ship, whereby circuit was completed through the inclosed fuze, or by the vessel striking a circuit closer, whereupon either the mine, moored at some depth beneath, was at once fired, or the necessary signal was given to the operator on shore.

Continental nations had followed in our footsteps, in providing themselves with equipments for defensive purposes by submarine mines, and the Danes, Swedes, and Norwegians had pursued the subject of submarine mines with special activity and success.

In the United States the subject of the utilisation of electricity as an exploding-agent for war purposes was being actively pursued, and important improvements in exploding instruments, electric fuzes, and other appliances had been made by Smith, Farmer, Hill, Striedinger, and others already mentioned, while no individual had contributed more importantly to the development of the service of submarine explosions than General Abbot, of the United States Engineers.

Illustrations of actual results capable of being produced in warfare by submarine operations had hitherto been very few; but of the moral effects of submarine mines there had already been abundant illustrations. In the war carried on for six years by the Empire of Brazil and the Republic of Uruguay and the Argentine Republic of Paraguay, the latter managed, by means of submarine mines, to keep at bay, for the whole period, the Brazilian fleet of fifteen ironclads and sixty other men-of-war. In the Russo-Turkish war, submarine mines and torpedoes were a source of continued apprehension; and the French naval superiority was paralysed, during the Franco-German war, by the existence, or reputed existence, of mines in the Elbe.

The application of electricity to the explosion of military mines, and to the demolition of works and buildings, had been of great importance in recent wars in expediting and facilitating the work of the military engineer. The rapidity with which guns, carriages, &c., were disabled and destroyed by a small party of men who landed after the silencing of the forts at Alexandria, illustrated the advantages of electrical exploding arrangements, combined with the great facility afforded for rapid operations by the power possessed of developing the most violent action of gun-cotton, dynamite, &c., through the agency of a detonator.

The application of electricity to the explosion of mines for land-defences during active war was not an easy operation, inasmuch as not only the preparation of the mines but also the concealment of electric cables and all appliances from the enemy entailed great difficulties, unless the necessary arrangements could have been made in ample time to prevent a knowledge of them reaching the enemy.

But few words were needed to recall to the minds of civil engineers the facilities which the employment of electricity to explosive purposes afforded for expediting the carrying out of many kinds of works in which they were immediately interested. Electrical blasting, especially in combination with rock-boring machines, had revolutionised the operation of tunnelling and driving of galleries; and although in ordinary mining and quarrying operations the additional cost involved in the employment of fuzes, conductors, and the exploding-machine was not unfrequently a serious consideration, there were, even in those directions, many occasions when the power of firing a number of shots simultaneously was of great importance. There was little doubt, moreover, that accidents in mining and quarrying would be considerably reduced in number if electrical blasting were more frequently employed.

The conveniences presented by electric firing arrangements under special circumstances were interestingly illustrated by a novel proceeding at the launch of a large screw steamer at Kinghorn in Scotland, which was recently accomplished by placing small charges of dynamite in the wedge-blocks along the sides of the keel and exploding them in pairs, hydraulic power being applied at the moment that the last wedge was shot away.

In the deepening of harbours and rivers and in the removal of natural or artificial submerged obstructions, the advantages of electric firing were so obvious that extended reference to them was unnecessary.

A substitute for electrical firing which had been applied with success to the practically simultaneous firing of several charges consisted of a simple modification of the Bickford fuze, which, instead of burning slowly, flashed rapidly into flame throughout its length, and hence had received the name of instantaneous fuze or lightning fuze. The fuze burned at the rate of about 100 feet per second; it had the general appearance of the ordinary mining fuze, but was distinguished from the latter by a coloured external coating. Numerous lengths of this fuze were readily coupled up together so as to form branches leading to different shot-holes, which might be ignited together so as to fire the holes almost simultaneously. In the navy this fuze was used as a means of firing small gun-cotton charges to be thrown by hand into boats when these engaged each other, the fuze being fired from the attacking boat by means of a small pistol, into the barrel of which the extremity was inserted.

THE IRON AND STEEL INSTITUTE

THE annual meeting of this society took place at the Institution of Civil Engineers on May 9, 10, and 11 last. On the first day the chair was taken by the retiring president, Mr. Josiah T. Smith, but after some formal business had been gone through he vacated it in favour of the president elect, Mr. B. Samuelson, M.M., F.R.S. The latter proceeded to read an able address, dealing mainly with the great progress which had taken place in the iron and steel industries since the Institute was founded in 1869. He remarked on the very large makes of pig iron which were now going on in American blast furnaces, and stated that these were found to be economical even as regards fuel and wear and tear of the lining. He then dwelt at some length on improvements in the manufacture of coke, especially with a view to recovery of the waste products. The deterioration which was feared would result as regards the coke itself had not appeared in the case of the Simon-Carvés ovens, worked by Messrs. Pease and Co., who were recovering oil and tar to the value of 4*s.* 2*d.* per ton of coal. Against this was to be set increased expenses to the amount of 1*s.* 4*d.* per ton of coke, and also interest on first cost and maintenance. He further referred to the Jameson process lately described before the Institution of Mechanical Engineers, and observed that this principle was being applied to recover oil and ammonia from smouldering waste-heaps at the pit bank. Passing on to the manufacture of steel he spoke with much approval of the Bicheroux gas puddling furnaces at Ougrée in Belgium, where gas obtained from slack was used for puddling, and gave more heat for steam-raising purposes than the old system. Speaking of the future demand for iron and steel, he pointed out that the United States had fifty times and Russia five times as many miles of railway per million of people as had our Indian Empire; and strongly urged the further development of railways in the latter country. The address also touched upon many other points connected with iron and steel, such as the increased production of Bessemer and Siemens steel, the great diminution in their price, the immense increase in shipbuilding, the proposed improvements in the patent law, and the better relations now existing between masters and workmen. At the conclusion of his address the president presented the Bessemer medal to Mr. J. G. Snelus of Workington for his achievements in introducing the basic system for the making of steel. A similar medal, bestowed upon Mr. S. G. Thomas, was reserved until his return to England.

The meeting then took up the discussion on a paper by Mr. Snelus on the Chemical composition and testing of steel rails. This paper was read at the Vienna meeting of the Institute and its discussion postponed. Mr. Snelus now added particulars of a new test for the hardness of rails, which consisted in driving a uniform punch under uniform pressure into a piece of the rail, and measuring the depth of the hole produced. Various experiments had satisfied him that this depth would be inversely proportional to the hardness of the rail. M. Cazés, engineer to the Lyons Railway, read a very long note in French on the same subject, the point of which was that, as he maintained, the wearing power of the rail depends not only on its chemical composition but also on the temperature in rolling and the amount of compression which it has experienced while being rolled. He stated that a percentage of carbon varying from ½ to 1 per cent. was found in France to give the best result for rail heads.

Two papers were then taken together: the first of these was by Mr. Wm. Parker (Chief Engineer and Surveyor of Lloyd's), on the Use of steel castings in lieu of steel and iron forgings for ship and marine engine construction. This paper gave the results of an important investigation made by Lloyd's Registry into the applicability of steel castings to heavy articles such as crank shafts, sternposts, &c., which had hitherto been chiefly made in forged iron. The result was to convince the Committee that such constructions can be made of cast steel quite as good for the purposes intended as those of wrought iron, and without the uncertainty which always exists more or less in large iron forgings. The making of such castings is mainly in the hands of three firms, who, however, differed very materially in their views of the best mode of proceeding, especially as to whether the metal should be melted in crucibles or in the ordinary open hearth. At Terre Noire, where such castings are also made, great importance is attached not only to annealing but also to tempering them in oil; and the author gave particulars of experiments made on this subject, which showed that such treatment had a marked effect in increasing the strength of the specimens whilst slightly diminishing their ductility. Reference was made to the distinction between forged steel and cast steel, and a number of experiments were quoted on similar bars of both these materials and also of wrought iron. In these experiments the strength of the cast steel specimens was in every case greater than those of the wrought iron, whilst the ductility was about the same. With the forged steel the tensile strength was designedly made low, but the ductility was very high. Taking the product of the ultimate strength and ultimate elongation as representing roughly the quality of the material, it appears that the cast steel is one-third higher than wrought iron on an average, whilst the forged steel is three times as high. Transverse tests, both by steady pressure and by impact, and also torsion tests, gave results practically similar. Finally some experiments made by Mr. James Neilson on steel, partly as cut from the ingot and partly as hammered or rolled down to a comparatively small thickness, showed that the latter process produced a very decided increase both as to strength and ductility in some cases, although the results were not very uniform. For casting crank shafts and similar work Lloyd's Committee considered that the tensile strength of the steel should not exceed thirty tons per square inch, and that a piece 1¼ inch square should bend cold through an angle of 90 degrees.

The second paper was by Mr. Wm. D. Allen, of Henry Bessemer and Co., and was on Bessemer steel in its cast and unwrought state. The object of this paper was to dispel the idea that Bessemer steel is not a safe material for casting on account of the frequent presence of cavities or blowholes within it. From daily experience Mr. Allen affirms that perfectly sound castings can be made from Bessemer steel, provided that the ladleful is alloyed either with ferromanganese or with some iron ore rich in silicon. In order that this alloy may mingle perfectly with the steel, the ladle should be violently stirred by means of an agitator, already described to the Institute. Of such steel, the reader stated that nearly 500 hydraulic cylinders had been made, and tested up to two, three, or even four tons per square inch.

An interesting discussion followed these papers. Mr. James Riley doubted whether the crucible process produced a result more uniform than the open hearth, and spoke strongly in favour of annealing and tempering in oil. He also doubted whether the work put upon forged steel gives the advantage which was claimed for it. Mr. Hall (Messrs. Jessop and Sons, of Sheffield) defended the crucible process, but disparaged Bessemer castings. Sir Henry Bessemer, however, considered that the agitator had overcome the difficulty which previously existed in making sound castings in his steel, while Sir Wm. Siemens observed on the danger that castings may contract unequally in cooling, and on its complete cure by annealing. He suggested an explanation for the curious fact of the advantage due to oil-hardening, namely, that the oil produced a compression of the outer layers, which acted on the rest of the mass, and was of more effect than any mechanical pressure could be.

The second day's proceedings opened with the reading of two papers on the vexed question of hot blasts and high furnaces. The first was by Mr. Wm. Hawdon, who gave the particulars of comparative experiments made on one of Messrs. Samuelson's furnaces at Middlesborough, which had been supplied alternately with blast from pipe stoves at 990° F. and with blast from firebrick stoves at 1400° F. The final result was an increase in the make per week from 400 to 458 tons of pig, a diminution in the coke per ton from 23·8 to 22·3 cwt., and at the same time an improvement in the quality of experiments at the various temperatures of blast between these two limits showed a gradual rate of improvement. At the same time the temperature of the escaping gases was diminished from 468° to 448°, and the volume, of course, was diminished also. Comparing the two modes of heat, he showed that the area of heating surface in proportion to the cubic capacity of the stove was much greater in the firebrick than in the pipe stove, giving a corresponding improvement in effect, and that the gases escaping from the chimney, which in the pipe stoves had a temperature of 1240° F., in the brick stoves were as low as 250° F. The result was to effect a very considerable saving of gas used for heating the blast, which gas may, of course, be utilised for steam-raising or other purposes.

The second paper was by Mr. I. Lowthian Bell, F.R.S., and dealt with the Value of successive additions to the temperature of air used in smelting iron. This paper was to some extent a rejoinder to those of Mr. Charles Cochrane, recently read before the Institution of Mechanical Engineers. Mr. Bell first considered the proposed application of hydrogen, or what is called water gas, to the blast furnace, and showed that this could produce no advantage in saving of heat. He then dealt with the question of the possible economy of coke in an ordinary furnace, and reiterated that a limit was placed to such economy by the fact that when the escaping gas consists of carbonic oxide and carbonic acid in the proportion of 2 to 1, these combined gases can no longer produce any effect in reducing iron ore. Hence the very great saving of fuel which had at first been effected by enlarging the size of the furnaces and increasing the heat of the blast, had now nearly reached its limit; which Mr. Bell still held to be represented by a capacity of about 15,000 cubic feet, and a temperature of about 1000° F. He dealt with the suggestion that the heating of the blast could be advantageously used to replace the heat evolved in burning carbonic oxide and carbonic acid; and showed that to effect any great improvement in this particular would require blasts of too high temperature to be practically available. He commented upon the results with the furnace at Messrs. Samuelson's works, and considered that the increase in the make must be due to the increased quantity of blast rather than to its higher temperature. He admitted a saving in coke of about 1 cwt., but he observed that on the other hand the furnace was driven very slowly, only supplying about half the weight of pig iron per cubic foot of capacity which was usually supplied by the furnaces in the Cleveland district.

Mr. Bell's position was strongly assailed by Mr. Charles Cochrane; but although the questions between them were still further debated, it can hardly be said that they are completely settled. The problem is one which involves several independent factors, and a variation in any one of these might produce a large effect on the final result. It was so far fortunate that on this occasion the question of stoves and of blast furnaces was considered together, and not separately, as is often the case; but there are still other matters to be taken into consideration. One of these, for instance, is the distance between the tuyeres at the bottom of the furnace. Mr. Cochrane confidently predicted that an alteration in this particular would effect a very important saving in Messrs. Samuelson's furnaces. The large economy actually realised by the use of brick stoves was commented upon by several speakers; but the advantage of increasing the capacity of furnaces appeared to be doubted by two very high authorities upon the subject, Mr. Edward Williams and Mr. E. Windsor Richards.

On Thursday afternoon the paper read was on the Northampton iron ore district, by Mr. W. H. Butlin. It gave an interesting description of this district, well known to travellers on the main line of the Midland Railway, in which, however, the deposits of ore have only been developed within the last thirty years. The paper also contained analyses of the ore, which is of a very variable character, and also of the limestone, slags, &c.

On Friday morning the first paper read was by Mr. John Stead of Middlesborough, on a New method for the estimation of minute quantities of carbon. The author had found that the colouring matter, which is produced by the action of dilute nitric acid upon white iron and steel, has the property of being soluble in potash and soda solutions, and that the alkaline solution has about two and a half times the depth of colour produced by the ordinary acid solutions. Hence it occurred to him that the colour-matter might be separated from the iron, as an alkaline solution, by simply adding an excess of sodium hydrate to the nitric acid solution of iron, and that the colour solution thus obtained might be used as a means of determining the amount of carbon present. This method is found to succeed well, as small a quantity as 0·03 per cent. of carbon being readily detected. The method of using it was described, and also experiments made to determine (1) the influence of heating the nitric acid solution for a longer or shorter time; (2) the effect of using an excess of nitric acid to dissolve the steel; (3) the effect on the solvent power of using a greater or less quantity of soda solution; (4) the effect of the presence of small quantities of chlorine. All these experiments proved satisfactory as regards the new process. An improved form of chromometer was also described.

The next paper was on the Production and utilisation of gaseous fuel in the iron manufacture, by Mr. W. S. Sutherland of Birmingham. It was of a somewhat discursive character, containing various suggestions, especially as to a method of making wrought iron by the converter process; but its chief object was to describe the production of a cheap form of heating gas, which the author has used largely for the welding up of boiler-flues, tubes, &c. In this process part of the fuel is burnt, as completely as possible, to carbonic acid and water, but the resulting heat is stored up partly in the remainder of the fuel and partly in regenerators, that in the regenerators being made to heat up to a sufficiently high degree a quantity of steam. This superheated steam is passed through the hot fuel, and forms with it carbonic oxide and hydrogen, which go away to be stored up and used. With this process about 55,000 cubic feet of gas is made per ton of Staffordshire coal, and at a cost of about 3*d.* per 1000 cubic feet, its heating power being about one-half that of coal gas. The author pointed out that it was most important to prevent as far as possible the formation of carbonic acid, and that for this a high temperature (say 1200° C.) was required.

The following papers were taken as read:—On Coal-washing machinery, by Mr. Fritz Baare; on the Tin-plate manufacture, by Mr. E. Trubshaw; and on Improvements in railway and tramway plant, by Mr. Albert Riche.

SCIENTIFIC SERIALS

American Journal of Science, April.—Review of De Candolle's origin of cultivated plants, with annotations on certain American species, by A. Gray and J. H. Trumbull.—Remarks on Glyptocrinus and Reteocrinus, two genera of Silurian crinoids, by C. Wachsmuth and F. Springer.—Smee battery and galvanic polarisation, by H. Hallock.—The age of the Southern Appalachians, by O. B. Elliott.—Evolution of the American trotting-horse, by W. H. Brewer.

IN the *Annalen der Physik und Chemie* for 1883, part i., Ernst Pringsheim has an elaborate paper on the theoretical and practical aspects of Crooke's radiometer. This is followed by essays on Stokes's law of fluorescence, by Ed. Hagenbach; on special cases of crystallisation, by E. Lommel; on the heat-conducting power of fluids, by L. Graetz; on the relation of specific heat in gases and vapours, by P. A. Müller; on the constant result of internal friction and galvanic conduction in relation to temperature, by L. Grossmann; and on A. Guebhard's proposed method of determining equipotential lines, by Hugo Meyer. Part ii. contains papers by O. Grotian on the power of electric conduction of some cadmium and quicksilver salts in liquid solutions; by W. C. Röntgen, on the change produced by electric power in the double fracture of quartz (continued in part iv.); by A. Kundt, on the optical character of quartz in the electric field; by H. Meyer, on the magnetising function of steel and nickle; by A. von Waltenhofen, on the history of recent dynamoelectric machines, with some remarks on the determination of the working powers of electromagnetic motors; by J. Wagner, on the tenacity of solutions of salts; by S. von Wroblewski, on the absorption of gases by fluids under high pressure; by A. Schuller, on distillation in vacuum; by K. R. Koch, on the elasticity of crystals of the regular system; by C. Bohn, on absolute masses; by E. Gerland, on the methods adopted by R. Kohlrausch in his researches in contact electricity. In part iii. papers are contributed by F. Neesen, on the specific heat of water; by E. Ketteler, on the conflicting theories of light (continued in part iv.); by W. G. Hankel, on the thermoelectric properties of helvine, mellite, pyromorphite, mimetesite, phenakite, pennine, strontianite, witherite, cerussite, titanite; by F. Niemöller, on the dependence of the electromotor power of a reversible element on the pressure exercised on its fluidity; by C. Tromme, on experimental researches in magnetism; by K. Vierordt, on sound measurement; by A. Ritter, on the constitution of gaseous bodies; by K. R. Koch, on a method of testing the micrometric screw. Part iv. contains papers by F. Kohlrausch, on the galvanic gauging of the surface coil of a wire bobbin; by C. Tromme, on electrical research; by M. Baumeister, on the experimental investigation of torsion elasticity; by E. Wiedemann, on thermochemical research; by G. Kirchhoff, on the theory of light radiation; by W. Wundt, on sound measurement.

Journal de la Physique, February.—On a spectroscope with great dispersion, by M. Cornu.—On the comparative observation of telluric and metallic lines, as a means of observing the absorbent powers of the atmosphere, by the same.—Researches on the photometric comparison of differently coloured sources, and in particular on the comparison of different parts of the same spectrum, by MM. Macé de Lepinay and Nicati.—On electric shadows and various connected phenomena (second article), by M. Righi.

Verhandlungen der k.k. Zool.-botan. Gesellschaft in Wien, 1882, Bd. xxxix. Heft 2 (March, 1883), contains:—Zoology.—Biological notes on some beetles belonging to the Dascyllidæ and Parnidæ, by Th. Beling.—On Platen's ornithological collections from Amboyna, by W. Blasius.—On a new tortoise, by J. v. Hornig.—On the genus Colias, by A. Keferstein.—On the skin glands in some larvæ, by Dr. Klemensiewicz (Plates 21 and 22).—On new Hymenoptera, by Fr. Kohl (Pl. 23).—On the Myriapods of Austrian-Hungary and Servia, by Dr. R. Latzel.—The butterfly fauna of Surinam, v., by H. B. Möschler (Plates 17 and 18).—On a new mite in the inside of the quill feathers in the hen, by Dr. C. Nörner (Plates 19 and 20).—On a collection of birds from Central Africa, sent by Dr. E. Bey, by A. v. Pelzeln.—On Pselaphidæ and Scydmænidæ, from Java, Borneo, and Central and South America, by E. Reitter.—On *Icaria scudderi*, by Dr. H. Weyenbergh.

THE *Atti* of the Roman *Accademia dei Lincei* for January and February, 1883, contains papers by E. Millosevich, on the ingress of Venus on the solar disk, December 6, 1882; by A. Lugli, on the mean variation of temperature in Italy, as affected by latitude and elevation; by A. Viola, on the relations of some physical properties of gaseous bodies under constant pressure and of constant bulk; by L. Pigorini, on barbaric stations still existing in the Emilian provinces; by Tommasi-Crudeli, on the malaria of the Tre Fontane district, which appears not to have been beneficially affected by the Eucalyptus plantations elsewhere found so efficacious; by S. Tacchini, on meteoric dust and the chemical analysis of the sands of the Sahara; by the same author, on Finlay's comet and on the new asteroid (232) discovered on February 1 by Palisa; by S. Brioschi, on the algebraic relations between the hyperelliptical functions of first order; by S. Ferrari, on the relations between the meteoric elements and some agricultural returns for the year 1880 in Italy.

THE *Rendiconti* of the *Reale Istituto Lombardo di Scienze e Lettere* for February and March, 1883, contains papers by G. Ascoli, on Irish glosses, especially those of the Ambrosian Codex; by M. E. E. Beltrami, on the theory of magnetic layers; by Z. Volta, on an unpublished drama of Luigi Ceretti; by G. A. Maggi, on the transmission of undulatory motion, and especially of luminous waves, from one isotropic medium to another; by P. F. Denza, on the observations of the transit of Venus made at the observatory of the Collegio Carlo Alberto in Moncalieri.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 12.—"The principal cause of the large errors at present existing between the positions of the Moon, deduced from Hansen's Tables and observation; and the cause of an apparent increase in the secular acceleration in the Moon's mean motion required by Hansen's Tables, or of an apparent change in the time of the Earth's rotation," by E. J. Stone, F.R.S.

The errors in the lunar theory have been traced to the effects of changes in the *unit of time* which have, apparently unconsciously, been introduced, from time to time, into astronomy with changes in the adopted data.

The argument is clearly seen by a consideration of the different expressions for the longitudes of, what may be called, the mean sun which have been adopted for the determination of the sidereal times at mean noon.

If B, H, and V denote the longitudes of the mean sun according to Bessel, Hansen, and Le Verrier, we have for 1850, January 1, Paris mean noon, $+ t$

$$B = 280^\circ\ 46'\ 36''{\cdot}12 + 1296027{\cdot}618184t + 0{\cdot}0001221805 \, . \, t^2$$
$$H = 280\ 46\ 43{\cdot}20 + 1296027{\cdot}674055t + 0{\cdot}0001106850 \, . \, t^2$$
$$V = 280\ 46\ 43{\cdot}51 + 1296027{\cdot}678400t + 0{\cdot}0001107300 \, . \, t^2.$$

In all these expressions the unit of time has been *supposed* to be a Julian year of 365·25 mean solar days. The constant differences $7''{\cdot}08$ and $7''{\cdot}39$ in $B-H$ and $B-V$ are not unimportant, for they introduce abrupt changes in the record of time; but the differences in the coefficients of t and t^2 show that the *same* unit of time cannot have been adopted in these expressions. The measure of time must be continuous; let, therefore, 1 and $(1+x)$ be the units in B and H,

$$\text{then } 1296027{\cdot}618184 \, . \, t + 0{\cdot}0001221805t^2$$
$$= 1296027{\cdot}674055t(1+x) + 0{\cdot}0001106850t^2(1+x)^2.$$

If, therefore, $n = 1296027{\cdot}674055$

$$x = -\frac{0{\cdot}055871}{n} + \frac{0{\cdot}000114955 \, . \, t}{n}$$

To reconcile B and H, therefore, x must contain a variable term. Similar remarks apply to the difference between B and V.

Now let N be the moon's mean motion referred to 1 as the unit of time, and $(N+\delta N)$ the moon's mean motion referred to $(1+x)$ as the unit of time,

then $(N+\delta N)(1+x) = N$,

$$\text{and } t\delta N = \frac{N}{n} \left\{ 0{\cdot}055871 \, . \, t - 0{\cdot}0000114955 \, . \, t^2 \right\}$$
$$= 0''{\cdot}747 \, . \, t - 1''{\cdot}54 \left(\frac{t}{100} \right)^2.$$

But Hansen determined his mean motion of the moon so as to force an agreement between his theory and observations reduced with Bessel's unit 1; and his tables, therefore, represented the observations well for many years, whilst 1 was adopted as the unit of time; but directly the unit of time was changed by the adoption either of H or V, then the effects of the erroneous determination of the moon's mean motion by Hansen became apparent. The change of error in longitude of Hansen's Lunar Tables between 1864, when Le Verrier's Solar Tables were adopted in the *Nautical Almanac*, and 1880, amounts to more than 10".

The effect of the change of unit is also shown in the comparison of Le Verrier's Solar Tables with observation, but of course only to about the thirteenth part of the amount shown by the Lunar Tables. The necessity of adopting some definite unit of time by fixing the constants in the expression for the longitude of the mean sun is insisted upon.]

If $L_0 + n_0 t + S_0 t^2$ is the expression adopted for the longitude of the mean sun, the quantities L_0, n_0, S_0, must never be changed. The correction δL, which from time to time may appear necessary to obtain the mean longitude of the sun from the longitude of the mean sun must not be allowed to change the adopted values of L_0, n_0, and S_0. The true longitude of the sun will then

$$= L_0 + n_0 t + S_0 t^2 + \delta L + \text{periodic terms.}$$

It would appear that speculations respecting changes in the time of rotation of the earth on its axis are at least premature until the theories have been revised with a unit of time freed from changes of adopted constants which are at present inextricably mixed up with any effects which would result from a change in the time of rotation of the earth on its axis.

The longitude of the mean sun when properly investigated, differs from the mean longitude of the sun by a secular term—

$$0''{\cdot}3113 \left(\frac{t}{100} \right)^2.$$

As this difference has been usually neglected in the determination of the sidereal time at mean moon, an error of about

$$13 \times 0''{\cdot}3113 \, . \left(\frac{t}{100} \right)^2, \text{ or } 4'' \left(\frac{t}{100} \right)^2$$

has been thrown upon this secular acceleration of the moon's mean motion. This accounts for the difference between Adam's theoretical value, and that deduced from eclipse observations.

Chemical Society, May 3.—Dr. W. H. Perkin, president, in the chair.—The following papers were read:—On a new oxide of tellurium, by Dr. E. Divers and M. Shimosé. When the compound of sulphur trioxide and tellurium, discovered almost simultaneously by the authors and by Weber, is treated in a vacuum, sulphur dioxide is evolved and a new oxide of tellurium is formed containing one atom of tellurium to one atom of oxygen. The decomposition takes place between 180° and 230°. The oxide is black, and quite stable at ordinary temperatures in dry air. No compound of this monoxide has yet been prepared, but in its properties it is essentially different from a mixture of tellurium and dioxide.—On tellurium sulphoxide, by Dr. Divers and M. Shimosé. The authors prepared this compound by pouring sulphur trioxide on to tellurium finely powdered and dried. It was purified from sulphur trioxide by heating to 35° and exhausting with the Sprengel pump. It is a red amorphous solid, quite stable at ordinary temperatures in sealed tubes. When heated in a vacuum to 90° it is changed into a bright fawn-coloured modification.—On a new reaction of tellurium compounds, by Dr. Divers and M. Shimosé. When sulphuric acid containing a small quantity of tellurium dioxide or sulphate in solution is poured into a hydrogen-generating apparatus, and the escaping hydrogen passed through a second portion of the telluretted sulphuric acid, a beautiful red colour, due to tellurium sulphoxide, is rapidly developed.—On a simple modification of the ordinary method for effecting the combustion of volatile liquids in Glaser's furnace with the open tube, by Watson Smith. The author causes the end of the combustion tube to project from the furnace, and volatilises the liquid by gently warming the current of gas with a Bunsen burner.—On the production of ammonia from the nitrogen of minerals, by G. Beilby. The author gives the results obtained with typical oil and coal shales when distilled (1) at a low red heat, (2) at a low red heat in a current of steam, (3) at a low red heat in a current of steam, the residual coke being afterwards subjected to the prolonged action of steam, so that a large portion of the coke is consumed and the nitrogen in it liberated as ammonia. Thus a sample of oil shale furnished by (1) 2·7 lbs. of nitrogen per ton as ammonia, by (2) 3·9 lbs., by (3) 12·0 lbs.—On the specific gravity of paraffin wax, solid, liquid, and in solution, by G. Beilby.

Zoological Society, May 1.—Prof. W. H. Flower, F.R.S., president, in the chair.—The Secretary read an extract from a letter addressed to him by Mr. W. L. Crowther, C.M.Z.S., respecting the possibility of obtaining living specimens of the Thylacine of Tasmania.—The Secretary exhibited, on behalf of Mr. H. Whitely, the skin of a Bird of Paradise (*Diphyllodes gulielmi*) from the Island of Waigiou, which was believed to be the second example of this rare species yet obtained.—The Secretary exhibited a set of Radde's international colour-scales, and explained the way in which it was intended to be used.—A communication was read from Mr. F. Moore, F.Z.S., containing the second part of a monograph of the sections *Limnaina* and *Euplœina*, two groups of Diurnal Lepidoptera belonging to the subfamily *Euplœinæ*. The present paper contained the descriptions of many new genera and species belonging to the group *Euplœina*.—Mr. Alfred Tylor, F.Z.S., read a paper on the colouration of animals, showing that the character of the ornament or decoration differs in the two great divisions of the animal kingdom—the Invertebrata and Vertebrata. Mr. Tylor pointed out that the law of emphasis, well known in architecture, was, in his opinion, applicable to natural history, and showed that the prominent characters of the animal are picked out in colour in precisely the same manner whenever colour is present. He divided his subject into several sections, and exhibited illustrations of the more important families in coloured diagrams.—A communication was read from Dr. O. Boettger, of Frankfort-on-the-Main, containing the description of new species of land-shells of the genus *Clausilia* from the Levant, collected by Vice-Admiral Spratt, F.R.S.—Mr. W. F. Kirby gave an account of a small collection of Hymenopterous and Dipterous insects obtained in the Timor-Laut group of islands by Mr. H. O. Forbes.

Mathematical Society, May 10.—S. Roberts, F.R.S., vice-president, in the chair.—Prof. M. J. M. Hill, of the Mason College, Birmingham, was elected a member.—Prof. Cayley made a communication on Mr. Wilkinson's rectangular transformation.—Mr. Tucker read abstracts of papers by Prof. Genese, relations between the common points and common tangents of two conics; by Mr. W. R. W. Roberts, on the motion of a particle on the surface of an ellipsoid; and by Mr. R. A. Roberts, on unicursal twisted quartics, part ii.; he also made a communication on two concentric circles. The circles considered were a circle which the author proposes to call the "Triplicate-Ratio" (T.R.) circle and Brocard's circle. If through a point (P) within a triangle ABC (whose trilinear co-ordinates are $2a\Delta/\kappa$, $2b\Delta/\kappa$, $2c\Delta/\kappa$, where κ stands for $a^2+b^2+c^2$), straight lines DPE^1, EPF^1, FPD^1 be drawn, then the circle $DD^1EE^1FF^1$ is the T.R. circle. The origin of the name is due to the fact that the intercepts on the sides (DD^1, EE^1, FF^1) are equal to a^3/κ, b^3/κ, c^3/κ respectively If $\lambda^2=a^2b^2+b^2c^2+c^2a^2$ and $\mu=\lambda/\kappa$, then the triangles DEF, $D^1E^1F^1$, which are equal to one another, and are similar to ABC, have their sides $=\mu a$, μb, μc. The lengths D^1E, E^1F, F^1D are equal to abc/κ, so that the perimeter of the above-named hexagon is $(a^3+b^3+c^3+3abc)/\kappa$. Other curious properties were pointed out. If the angle BFD be denoted by ω, then $\cot\omega=\cot A+\cot B+\cot C$, and several other trigonometrical relations were indicated. If through A, B, C lines are drawn parallel to the sides of DEF, $D^1E^1F^1$, these by their intersections determine five of the points on Brocard's circle, the other two Brocard points being P and H (the centre of the circum-circle). Lastly the trilinear equations to the two circles were given, and it was shown that the two circles are concentric. The T.R. circle also divides each side into segments which are in the duplicate ratios of the sides.—The Rev. M. M. U. Wilkinson read a second paper on spherical functions.

Geological Society, April 25.—Mr. J. W. Hulke, F.R.S., president, in the chair.—Rev. William Franklen Evans, Ernest Hall Hedley, and Henry James Plowright were elected Fellows, and Dr. J. S. Newberry, of New York, a Foreign Member of the Society.—The following communications were read:—On the skull of *Megalosaurus*, by Prof. R. Owen, C.B., F.R.S. The specimens described in this communication were obtained by Edward Cleminshaw from the freestone of the Inferior Oolite near Sherborne (Dorset) from some blocks which had been quarried for building purposes. These were sent by him to the British Museum, where the remains have been developed. One block includes a great proportion of the right side of the facial part of the skull, the missing parts being the fore-end of the premaxillary, the suborbital end of the maxillary, and the upper hinder pointed termination of the same bone. Ten teeth are preserved in the maxillary bone. Another block contains the outer side of the right mandibular ramus with teeth and with some other fragments. In a third block is the anterior part of the left mandibular ramus with portions of the teeth. These remains were described in detail; and in conclusion the author discussed the bearing of these and other Megalosaurian remains upon our knowledge of the structure of that animal and its affinity to existing Reptilia, and criticised some of the evidence on which the relationship of the Dinosauria to birds is inferred, a relationship which he had suggested in 1841, but upon grounds which appeared to him to be more satisfactory.—Notes on the Bagshot sands, by Mr. H. W. Monckton, F.G.S.—Additional note on boulders of hornblende picrite near the western coast of Anglesey, by Prof. T. G. Bonney, F.R.S.

Institution of Civil Engineers, April 8.—Mr. Brunlees, president, in the chair.—The paper read was "On the Diamond Fields and Mines of South Africa."

EDINBURGH

Mathematical Society, May 11.—Mr. J. S. Mackay, F.R.S.E., president, in the chair.—Mr. D. Munn, F.R.S.E., gave an address on the geometry of the nine-point circle, and Dr. C. G. Knott, F.R.S.E., a paper on Newton's "Opticks."

BERLIN

Physical Society, April 30.—Dr. Pringsheim reported on his recently published measurements of the wave-lengths of the least refractive rays of the solar spectrum. In order to obtain them he used a radiometric torsion apparatus, similar to those used by Crookes, which carried a small mirror by which the revolution of the torsion beam caused by the ray could be observed. The source of light were solar rays reflected into a dark room by a heliostat, first united in a focus by a concave mirror, then rendered parallel and directed upon a revolvable grating-mirror, which produced a whole series of spectra. The various divisions of the first spectrum were directed upon the torsion-apparatus by means of a slit, and it was noted up to what wave-length the mirror still performed part of a revolution. In order to exclude the visible rays of the second spectrum which were mixed with the infra-red ones of the first spectrum, and prevent their reaching the torsion-apparatus, Dr. Pringsheim cut them off partly by an iodine solution, partly by an ebonite plate according to Abney. The extreme limit of the spectrum where an effect was still observed was at the wave-length $\lambda=0{\cdot}00152$mm.—Prof. Neesen reported upon a treatise entitled "On the Contractions of Volume as a Measure of Chemical Affinities," sent to the Society for publication in its *Transactions* by Herr Müller Erzbach of Bremen. He shows in a number of salts formed by selenic and chromic acids that in chemical combination a stronger contraction of volume corresponds to a greater chemical affinity, and is shown in the heat generated when the combination takes place; while in those salts which show a smaller contraction, acid and base are bound together by less powerful affinity.

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