THURSDAY, DECEMBER 17, 1874

THE TRANSIT OF VENUS

IT is not too early to congratulate the world of science upon a grand triumph. The telegrams which have of late been flowing in almost incessantly from all parts of the Northern Hemisphere—now from far Japan and from Siberia, recording the success of French, Russian, and American parties; and now from America, giving fuller details regarding the doings of the latter—leave no doubt whatever that the weather has been better at the northern stations than might have been expected, seeing that the observations have been made in the winter half of the year.

Nor has the Northern Hemisphere been the only one to give us news. We already know of success at Melbourne and Hobart Town, at which place was an American party similarly equipped to those at Wladiwostok in Asiatic Russia, and at Nagasaki; and Prof. Newcomb has already telegraphed to the *Times* that the eighty photographs taken by the American method at these places, combined with the 113 taken at Hobart Town, are sufficient to give us a value of the solar parallax with a probable error of perhaps one-fortieth of a second of arc. This gives a foretaste of what photography is likely to do for us in this and the coming Transit of 1882.

Before we proceed to detail the observations at the various stations, it will be well to re-state the various ways in which a Transit of Venus may be observed. This we will do almost in the words employed in a former article.

We have the utilisation of a Transit—

(*a*) By the determination of times of contact at different stations, combined with a knowledge of the longitudes of those stations.

(*b*) By the determination of the least distances between the centres of the sun and Venus during the Transit, observed from different stations.

This last determination may be made by—

(1) What is called Halley's method; or, if we wish that the world should forget a great work accomplished by a former great Astronomer Royal, we may term this the "method of durations."

(2) By the Photographic method; or,

(3) By the Heliometric method.

Premising that the first method of determination (*a*) was devised by Delisle, we have now our nomenclature sufficiently complete for present purposes, and we may begin with the stations at which this method can be *best* employed. Of these we have four groups: Accelerated Ingress, Sandwich Islands; Retarded Ingress, Kerguelen's Land, Heard or Macdonald Island, Mauritius, Bourbon, and Rodriguez; Accelerated Egress, Campbell Island, Emerald Island, Auckland Island, Royal Company's Island, and New Zealand; Retarded Egress stations in Western Russia, Persia, and Egypt.

Of these groups the northern ones can be used for Delisle's method solely, as only Ingress or Egress is seen; Ingress in the Sandwich Island group, Egress in the Western Asiatic group. But the southern groups may be used for all methods.

For the methods we have grouped under (*b*), stations in Eastern and Southern Asia, combined with those in the Southern seas which we have already named, and stations between them, such as Melbourne and Adelaide, may be employed.

From the Sandwich Islands, Kerguelen's Land, Heard Island, Mauritius, Bourbon, and Rodriguez we have of course not yet heard; we consequently know nothing of Delislean observations of Ingress.

Of observations of Accelerated Egress on this method we know nothing, but with regard to Retarded Egress we know that the English and Russian parties in Egypt have been wonderfully successful. At Teheran, a second class Russian station, but better adapted than Egypt for applying the Delislean method, the observations were a perfect success. At Ispahan, a German party obtained nineteen good photographs; but north of this, in the most favourable point of all, the Russian parties at Ormsk, Astrachan, and in all that region, there was complete failure.

From the English party several telegrams have been received since our last number appeared: a long one by the *Times*, and several shorter ones by the Astronomer Royal. These we give:—

"*Cairo, Dec.* 9.—The Transit of Venus was observed in all its phases by the astronomers of the Government Expedition in Egypt this morning, at the Central Station at Mokattam Heights. It was observed by Captain Orde and Mr. F. M. Newton at Suez, and by Mr. Hunter at Thebes. It was photographed by Captain Abney, and observed by Dr. Auwers and Prof. Dollen, also by Colonel Campbell and others. During the last three days the weather has been very bad, and this morning the telescopes in Cairo and Suez were directed to the eastern quarter of a sky clouded over, showing, however, a few breaks, which gave hope. Glimpses through the clouds exhibited Venus as a distinct black spot on the sun, but no opportunity was given for a micrometer measurement for nearly half an hour after sunrise; then a few chances were given through the openings in the clouds. A decided opening occurred of a very hopeful character about ten minutes before contact, and after one more cloud, which passed over two or three minutes only before the critical epoch, the observation of internal contact was made satisfactorily at every station in Egypt, and the photoheliograph has done its work well. The sky was quite clear for the measurement of cusps and any observations that could be made of external contact. The astronomers are satisfied with their observations. The phases are declared in Cairo to have so closely resembled those shown by Sir George Airy's model at Greenwich that it was hard to divest the mind of the idea that it was only model practice again. The Khedive has taken a warm interest in the work, and guarded the Mokattam station from intrusion by cavalry pickets. By means of the telegraph line he put up to Mokattam Heights, interchanges of telegraph time signals have been made between that station and Greenwich, Suez, and Thebes. This expedition has now, therefore, nearly completed its work, and in a few days will probably break up."

Capt. Ord Brown, R.A. (Mokattam), Dec. 9:—

"The egress of Venus was observed at Mokattam this morning. There has been much bad weather and anxiety. All well now. Contact seen through very slight haze with [the] Lee [Equatorial] at about 13h. 25m. 25s. sidereal, and with De la Rue 13h. 22m. 21s. (Observe in the Greenwich book of observation with the model; my egress is always after other observers, except Mr. Gill's.) Clouds spoilt much double image work, but many limbs and cusps were taken. The phases closely resemble those of the model, except a line of light round the planet's edge, which appeared with strong sun just after the above contacts. It perplexed me and made me lose my best cusps. When I found that it continued two minutes and that it would be so indefinitely, I turned to cusps. I have exchanged bad telegraph signals twice with Thebes, and good ones three times with Suez.—Mokattam Country, lat. 59° 58′ 14″."

Mr. Hunter (Suez):—

"Sky cleared partly a few minutes before contact. Contact satisfactorily observed, and a considerable number of micrometer measurements made."

Capt. Abney (Thebes):—

"Beautiful morning. Sun rather shaky at first, nice and sharp at time of contact, and good observations, though differing slightly in time. Sun pictures good. The fifty photographs in Janssen's slides include internal contact; external contact not taken. No black drop apparent in photographs after careful examination."

This, then, is all we know at present, or are likely to know for some little time, of the work done at purely Delislean stations. We now come to the stations at which the various methods of determining the least distance between the centres of the sun and Venus during the Transit, observed from different stations, are applicable.

And we may clear the ground by referring to the news from the southern stations first. From Hobart Town has come the best news in a telegram to the *Times*:—

"Prof. Harkness, of the American Transit Expedition at Hobart Town, reports, although the weather was bad, observations were particularly successful; 113 photographs were taken during the passage over the sun's disc."

We had previously heard of success from Melbourne and Adelaide; but these stations are not so well situated as Hobart Town, and it is doubtful if all the resources of a first-class fixed observatory, possessed by Mr. Ellery at Melbourne, will make up for his comparatively poor position.

We now come to that region where, in fact, the whole interest of the Transit has centred during the past week: to Asia and the adjacent Japanese Archipelago, neglected in the English arrangements even after the Board of Visitors of the Greenwich Observatory had very clearly indicated their opinion of the original official programme, by insisting upon the employment of the "method of durations" in the Southern Hemisphere. But, fortunately for the credit of English science, an English possession—India—has something to say in the Asiatic work. On the representation of Col. Tennant (who has done so much for astronomical science by his observations in India) of the importance of a station in the northern part of that country—a representation which was at once warmly received by the Viceroy—the Home Government at once took the matter up, and the result has been that a first-class observatory was erected at Roorkee. This was the Asiatic station from which news (which we chronicled last week) was first received.

But Northern and Eastern Asia was thickly studded with Russian, American, and French parties. In the Russian territory, Nertchinsk, Orianda, Charbarovka, Kiachta, Tschita, Port-Possiet, Wladiwostok, and many other places that we might name, were strongly occupied, and the wealth of results, whether in photography or heliometric measures, has been marvellous. One of these places—Wladiwostok—was occupied by an American as well as a Russian party. Herr Struve's telegrams to the *Times* regarding the observations at these places are as follows:—

"*Wladiwostok.*—Transit of Venus observed at both contacts, numerous chords and distances of the two limbs were measured.

"*Port-Possiet.*—Much clouds and mist; two interior contacts observed, and thirty-eight photographs taken.

"*Charbarovka.*—First two contacts and some chords observed.

"*Tschita.*—Contacts observed, and four series of measures with heliometer.

"*Orianda.*—Satisfactory observation of last two contacts.

"*Nertchinsk.*—Three contacts observed, and two diameters and twenty distances of the planet measured with heliometer.

"*Teheran.*—Full success of observations.

"*Thebes.*—Splendid weather; very important observations.

"*Kiachta.*—Much cloud; got only eight photographs.

"*Naratow.*—Clouds; complete failure.

"*Possiet.*—Photographs satisfactory after development, though taken through mist."

The work done by the American party at Wladiwostok, as stated in a Reuter telegram in the papers on the 11th, was as follows:—

"*Copenhagen*, *Dec.* 9.—Prof. Hall telegraphs from Wladiwostok to-day, at 10 A.M., that the observations of the Transit of Venus made at that place by the American party under his direction have not been very successful, on account of the hazy and cloudy weather. The first and second contacts were observed, and thirteen photographs were taken."

In Japan there were French parties under Dr. Janssen at Nagasaki and Kobe, an American party also at Nagasaki, and Russian and Austrian parties at Yokohama.

The telegrams giving the account of Janssen's work we must transcribe as they were received.

"*Nagasaki*, *Dec.* 9.—M. Dumas, Secretary Académie des Sciences and Minister Instruction, Paris.—Transit observed and contacts obtained. Fine telescopic images. No ligament. Venus seen over sun's corona. Photographs and plaques. Cloudy at intervals. Two members of our mission have made observations with success at Kobe."

"Transit observed at Nagasaki and Kobe. Interior contact, no ligament. Photographs revealed several clouds during transit. Venus seen over corona before contact. Gives demonstration of the existence of the coronal atmosphere."

The American party at Nagasaki has recorded its work in the following terms:—

"Day cloudy, but obtained second contact well—two observers; first and third contacts through clouds, and doubtful; 150 micrometric measures of cusps, separation of limbs, and diameter of Venus; thirty-one meridian transits both limbs, Sun and Venus; eighteen micrometric measures for difference of declination of limbs at meridian. About sixty good photographs. Ends threatening rain. Telegraph difference of longitude with Wladiwostok in November. All well."

The following is a copy of another telegram to the *New York Herald*:—

"*Wladiwostok, Siberia, Dec.* 9 (10.10 M.E.).—Prof. Hall reports much haze and cloud at Wladiwostok. First and second contact of Venus observed, and thirteen photographs taken near middle of transit. A calm bay, with temperature 34°; instruments and photographic apparatus working finely. All the American party working well."

The Russian and Austrian parties give no details; they only announce their success.

There is now a certainty that in the Southern Hemisphere the eastern stations will be more strongly occupied than the western ones. The Americans were foiled in their gallant attempt to occupy the Crozets, because they had not time to wait for weather moderate enough for them to land their instruments. The party has therefore gone on to Campbell Island, where they will already find a French party. It is difficult to restrain one's pen when we think of the combination of want of a true appreciation of the conditions of the problem, and want of that old spirit which used to make us take up posts of difficulty, which has prevented England being represented here. A successful Polar Expedition will scarcely wipe away the national disgrace which is ours in consequence of official action in this matter, and the French and Americans may well be proud of the position they now occupy.

The *Times* thus relates the French landing on Campbell Island:—

"A letter has been received to-day (Dec. 11), dated Campbell Island, Oct. 4, from the chief of the French Expedition stationed there. This had been carried to Bourbon by the ship which had transported the expedition to Campbell Island, and which left it to wait at Bourbon until the time came for fetching the astronomers away. The first idea was to keep this ship off the coast of Campbell Island in order that the observers might live on board; after struggling three days against horrible weather they at last landed on the island, and they soon perceived that it was impossible to keep the ship off the shore, which was without shelter and exposed to terrible gusts of wind, so that it ran the greatest risk of being lost. The members of the expedition, seeing that if the ship were to go down they were exposed to very serious danger—for they would be abandoned on an uninhabited island without means of communication, while everybody would think they still had the ship at their disposal—decided to unload the ship and establish themselves in the island and to send away the vessel, which would come and fetch them immediately after the observation of the phenomenon. This project was carried out. The observers began by organising temporary shelter, and then they built sheds to protect the instruments, the necessary utensils, and the provisions. The process of unloading was very long and troublesome, because the expedition, which has many members, had brought provisions for one year. While exploring the island they found nearly in the middle of the island a vessel which a hurricane had thrown there, and they were thinking of utilising the wreck, either by splitting it up or by placing themselves inside it, for protection against wind and weather. But two or three days afterwards another hurricane blew the ship out to sea, and they saw it no more. They were then obliged to do the best with all they had brought with them, for they were living in hourly dread of sharing the fate of the wreck.

"It is thought that since the 4th of October, the date on which the ship left for Bourbon, up to the moment of the transit, the expedition will have completed its organisation, its observatories, and have been able to fulfil its mission. As soon as the ship reaches a telegraphic station, the expedition will hasten to communicate particulars to the Institute of France. Nothing is known, of course, as to the exact period when these communications will be received. The particulars relative to the difficulties of this expedition and the dangers to which it is exposed have been received here with all the more interest that it was feared only two days ago that the Campbell Island station would not be organised in such a way as to make the observations under favourable conditions. It is still feared the weather may not have been favourable, and that so much fatigue and effort may not have been rewarded with the magnificent result it deserves."

It will be seen not only that a large number of observations have been made bearing on the main point, but that many side issues of great interest are raised. Dr. Janssen's observations have decidedly been amongst the most remarkable, not only with regard to the absence of the ligament, but as touching the visibility of Venus on the coronal atmosphere. Any detailed reference to these and many other points we must, however, leave for a subsequent article. We have been anxious in the present one to put our readers in possession of the results of the observations, so far as we at present know them, in the most authentic and intelligible form.

CHAPPELL'S "HISTORY OF MUSIC"

The History of Music. Vol. I. From the Earliest Records to the Fall of the Roman Empire. By William Chappell, F.S.A. (London: Chappell and Co., 1874.)

MUSIC is now being cultivated in a much more earnest and thorough manner than heretofore, not only as a practical art, but as a matter of theoretical and historical interest, as is evidenced by the late formation of a "Society for the study of the Art and Science of Music," the object of which is to encourage musical studies of a higher character than those comprised in ordinary musical training. Hence, as the early history of music is one of the most interesting as well as one of the most obscure topics connected with the art, an authoritative new investigation like that before us is of real value.

Mr. Chappell, who has had much to do during his life with practical music, brought out some years ago a "History of the Ballad Literature and Popular Music of the Olden Time," a book which has become now of standard authority on such matters. It seems that the eminent historian Mr. Grote suggested to him that he would do well to carry his inquiries further back, and to attempt to unravel the state of music among the Greeks. His account of his progress is worth extracting. He says:—

"Mr. Grote's enthusiasm for the Greeks somewhat exceeded mine; and, although my recollection of the language was fresher than now, I did not suppose that, even if I should succeed, a knowledge of Greek art and science would greatly advance those of the moderns; therefore I received the proposal rather lukewarmly. But when favoured with the twelfth and last volume of the 'History of Greece,' with an inscription from the illustrious author, in deference to his long antecedent recommendation I took the first step forward, by buying the works of the Greek writers upon music.

"I had taken note of the odd uses of Greek words in manuscripts of the Middle Ages written in Latin; therefore, while reading the Greek authors on music, I continued to copy out such definitions of musical terms as I then encountered. I began without expectation of success as to understanding the music of the Greeks, owing to the number of able men whom it had baffled; but my little glossary seemed to afford the clue, and soon made me interested in the subject. It became evident that the Roman perversion of Greek musical terms had been one of the great difficulties in the way of previous inquirers (although by no means the only one), for I could then understand the system."

All this confirms the character of the author as an earnest, painstaking inquirer, and affords therefore a guarantee for the value of his historical investigations.

Mr. Chappell comments on the two great English musical histories of the last century by Burney and Hawkins, and contends that much of the obscurity in which they left the ancient music was caused by their obtaining their information second-hand, namely, from Boëthius and other commentators, chiefly Latin, on the Greek writers. Many of these had not sufficient knowledge of the subject to understand the original technical terms, which they therefore rendered either erroneously or obscurely, and thus error and obscurity have been introduced into succeeding writings.

"It may," says Mr. Chappell, "at first appear unaccountable that, among the numbers of learned men who made the attempt to understand the Greek system during so many ages, no one should have succeeded, especially considering that it would hereafter be shown, even to the quarter-tone, to be our modern system of music. So simple a result seems ludicrous. But this general failure is to be accounted for by the fact that the Romans had twisted round the meanings of the Greek words in so extraordinary a fashion that perhaps 'tone' and 'diatonic' are the only two which remain nearly identical in the two languages. So that, to unriddle the subject, the student had first to unlearn all that he had been taught as to the meanings of musical terms, and then to begin again, trusting only to the Greek authors. No Latin treatise would avail, nor would any modern language in which musical terms had been derived through the Latin, or through the Western Church. The misuse of Greek technical language by Romans was by no means limited to music."

To eliminate these errors, the author tells us, and we believe him, that he has in every case, where possible, gone to the fountain head, and that the information he gives us may consequently be depended on.

We have thought it right to show at some length what are the author's qualifications for his work, and on what grounds he lays claim to our attention and credence; for, in *historical* works this is all-important; few of us have opportunity, and still fewer have inclination, to grope for ourselves among the mouldy lore of antiquity; we are glad enough to find others who will do it for us, and are ever ready to take as authentic whatever they tell us they have found there. Hence correctness and care are cardinal virtues in historical works; the want of these qualities renders such works worse than valueless, as merely promoting the dissemination of error.

The history of music, interesting as it is, is not, properly speaking, a subject to be treated of largely in NATURE; but, in justice to the meritorious author, we may venture to mention some of the results of his labours.

In the first place, he shows that the system of music used by the Greeks did not originate with them, but was borrowed from more ancient nations. He finds, for example, that "the number of notes in the Egyptian scale was precisely the same as the Greek, including the three Greek scales, diatonic, enharmonic, and chromatic." No Greek writer alludes to any difference between the Egyptian and Greek systems of music, although the best Greek works on the science of music, saving the Problems of Aristotle, were written on the soil of Egypt." Then he turns to the Chaldæans, or learned men of Babylon, and again finds (through an astronomical comment which, as usual, supposes the motion of the planets to be regulated by musical intervals, and thus to make everlasting harmony) that the Chaldæans had the same musical intervals of fourth, fifth, and octave, as the Egyptians. From these he was led to Hebrew music; remarking that proofs are not wanting of the similarity of this to the music of surrounding nations; so that "henceforth we may fairly conclude that we have at last arrived at the musical system of ancient Asia, and that it is our A, B, C, D, E, F, G."

The author, of course, enters largely into the progress of music in Greece. We read of the early tetrachord lyre, of its enlargement by Terpander; of the great improvements made by Pythagoras in the addition of the octave, the fifth, and other notes; of his important determination of the proportions of the lengths of strings, subsequently transmitted to posterity by the great geometer Euclid; of the chromatic and enharmonic scales, hitherto so perplexing; of the improvements in certain harmonic ratios made by Didymus and Ptolemy, and so on; from all which we undoubtedly gather a far clearer view of what Greek music was than can be obtained from either of our English histories.

The result is that the ancients anticipated almost exactly the diatonic scale of modern times. Their scale passed over to the Latins; it was adopted without change by the early Church; and by this means it has come down, unaltered, to our time. If we run up two octaves on the *white* keys of the modern piano, beginning and ending with A, we are playing the same notes as the Greeks used, any time after Pythagoras. We may add that if we use only the *black* keys (and many modern tunes may be thus played), we sound a scale precisely corresponding to one of the Greek "chromatic" genera.

The scale, be it remembered, is the *material* from which music is made. To discover what sort of melodies the ancients constructed from this material is another thing. Mr. Chappell has, however, presented us with three real Greek tunes, set to hymns to Calliope, Apollo, and Nemesis respectively. They have been, it is true, decked out, by the skilful aid of Mr. Macfarren, in an anachronous dress of modern harmony and rhythm, suggesting the idea of Pythagoras in a periwig; but, at any rate, they are no more incongruous in this respect than the so-called "Gregorian" chants, as sung with modern embellishments at a Ritualistic church-service.

The question has been often and warmly discussed whether the ancients used what we call harmony, or whether they did anything analogous to our singing or playing in several parts. Our author believes that they did, but in this matter he has not the argument all his own way. The late M. Fétis, who devoted the last years of his life to the preparation of a great History of Music,* has made a most elaborate investigation of this point, partly in the third volume of his work, and still more fully in a separate memoir published by the Academy of Sciences of Brussels. It is ably and forcibly argued, in opposition to many learned German critics who have held Mr. Chappell's view, and M. Fétis arrives at the conviction that "the supposition of the existence of harmony among ancient nations is one of the most remarkable extravagances of modern times." Mr. Chappell is very positive in his own opinion, but when we come to compare the two essays we cannot help seeing what a poor match his desultory guerilla argumentation is for the powerful disciplined logic of his more experienced antagonist, and cannot hesitate for a moment which side should prevail.

But even if we were inclined to believe with our author that the ancient Greeks did use some sort of harmony (other than the octave, which M. Fétis freely allows them in common with all nations), we are not much the forwarder: for even Mr. Chappell appears quite at a loss to form any reasonable idea of what this harmony was like. After all, therefore, the dispute is little more than "'twixt tweedle-dum and tweedle-dee."

The subject of ancient *musical instruments* is as important and as interesting as that of the music itself: and, indeed, they have in all ages had such a necessary connection, and have been so dependent on each other, that improvement in one has gone hand in hand with improvement in the other.

Mr. Chappell has devoted much attention to the evidence as to the nature of the instruments used in ancient times. This, he says, has always been found a difficult subject to treat upon, partly because so few of the instruments named by classical writers can be identified by pictorial or written descriptions, and partly because such descriptions, when they do exist, are often obscure or contradictory, particularly when obtained only through the medium of incorrect translations. He goes through a long list of ancient instruments of the three classes—wind, percussion, and string—and has given a large fund of information about them.

But what he prides himself most upon is the elucidation of the construction of the hydraulic organ, about which there has hitherto been much doubt and difficulty. He shows that this has arisen either from misapprehension of the ancient descriptions or from a want of sufficient knowledge of mechanism to understand the technical details; and he gives, in a most interesting chapter, an account of the instrument, which evidently presents a high claim to be the true one. In this particular we are delighted to award him the merit of a real triumph over his enemy, M. Fétis, who says, after speaking of the ambiguity of the description of the instrument left by Vitruvius:

> "Sous ce rapport l'incertitude persiste, et tout porte à croire qu'elle ne sera jamais dissipée, à moins que le hasard ne fasse découvrir un des instruments du mécanicien d'Alexandrie, dans les recherches faites à Pompeii."

* "Histoire générale de la Musique, depuis les temps les plus anciens jusqu'a nos jours." Par F. J. Fétis. Paris: Firmin Didot. Four volumes of this are now ready, bringing the history down to somewhat later than the time of Guido d'Arezzo; and, we understand, materials have been left for still more.

If it were only for his solution of this difficulty, Mr. Chappell's work deserves high praise.

We cannot expect every historian to be a Gibbon or a Hume, and though we readily testify to the merits of Mr. Chappell's work, we are obliged to say it is not without its faults. One is the tendency of the author to be diffuse and discursive in his style, to such an extent, indeed, as to give the work the character rather of an amusing gossip than of a serious history.

Another of Mr. Chappell's peculiarities is his strong tendency to over-confident dogmatic assertion, which renders it often difficult for the reader to distinguish between statements he has evidence for, and mere opinions of his own. Every writer on history should remember that on that subject dogmatism is utterly out of place: no man's *ipse dixit* is worth the paper it is written on: if he cannot or will not show chapter and verse for all he has to say, he had better let history alone. Hypotheses and speculations on obscure points are all very well; they are often useful for discussion, and sometimes turn out right; but they must be put forward clearly as what they are, and not given as truths.

Mr. Chappell has a high opinion of his own qualifications for his work, which is quite pardonable; but this is unfortunately coupled with an unduly low estimate of the competency of other historians, which is not pardonable. His contemptuous sneers at M. Fétis, for example, are in the worst taste; and if the Nestor of musical literature were alive to reply, we would not be in Mr. Chappell's shoes for a trifle. As it is, did it never occur to him that, as M. Fétis's history has now a wide circulation, and is becoming, in fact, the European standard book on the subject, readers who have access to both works might be tempted to retaliate by comparisons not altogether in favour of the English historian? Those who live in glass houses should not throw stones.

We have alluded above to an anachronism in the form in which Mr. Chappell has presented some of the Greek tunes. There are other analogous cases where he produces confusion by ascribing to the ancients ideas that have only arisen in modern times. He talks, for instance, often of the *key* and the *key-note* of Greek music. Does he mean to assert that any ideas existed in those days analogous to what we understand by these terms now? And when he sees, in an ancient picture, a man shown clapping his hands, he calls him a "conductor beating time." Had Sir Michael Costa really a prototype among the Egyptians, who gesticulated four in a bar?

We wish we had no worse faults to find than these, which are, after all, only peculiarities of style (and *le style c'est l'homme*); but unfortunately there is one part of the work which, as it affects the interests which it is the peculiar object of NATURE to promote, we are bound, though most reluctantly, to speak strongly on. The followers of Zoroaster hold that every man is subject to the alternate influence of two spiritual agencies, one prompting him to good, the other inciting him to evil. Ormuzd (we think that is the name) has been active with Mr. Chappell, leading him through the pleasant pages of Aristotle and Plato, and dictating to him all the agreeable matter in which we have been delighting, while the serpent-like Ahriman has been looking grimly on. But, the history ended, the turn of the evil tempter has arrived, and the good angel has retired, veiling his face with his wing, and dropping (if angels can weep) a tear over the calamity which he had no longer power to avert.

In plain language, Mr. Chappell has been minded, in an evil hour, to wander away from his legitimate domain of Ancient History, and to indite a long disquisition on the by no means kindred subject of Modern Science, treating especially on the laws and phenomena of acoustics, and their bearing on the nature and relations of musical sounds. In this his aggressive spirit is again manifested. All scientific men interested in the theory of music know that within the last few years Prof. Helmholtz, of Heidelberg, one of the first physicists of Europe, has brought out a work, "Die Lehre von den Tonempfindungen, als Physiologische Grundlage für die Theorie der Musik," which, for the profundity of its knowledge both of the physical and musical elements of the question; for the novelty and importance of its views; for the skill and conclusiveness of its experimental demonstrations; and for its general masterly style, has deservedly excited the admiration of all Europe. It has gone through three editions in Germany, has been also published in French, is now being translated into English, and has served as the basis already of several other English works, the author of one of which describes it as "a profound and exhaustive treatise, which does for acoustics what the Principia of Newton did for astronomy." Now, Mr. Chappell presumes to criticise this work in a tone which clearly shows not only that he is unaware of the reputation of its author, but that he is under some strange hallucination as to his own qualifications for setting up as judge in the matter. He attributes to Helmholtz both theoretical ignorance and experimental error; puts forward his own confused notions as "the *true* (in offensive opposition to Helmholtz's *false*) physiological basis for the science of music;" and sums up with the following paragraph, which, comparing the scientific position of the two writers, may certainly be considered a curiosity of criticism:—

"I am persuaded that the *Tonempfindungen* is a hasty book . . . . the value of time was too largely considered in its composition, and some very necessary experiments, such as those upon harmonics, were omitted. But since success has been so widely attained, it may be hoped that the author will find time to revise the next edition, and, in doing so, that he will bear in mind an admirable motto for men of science, *Chi va sano, va piano.*"

A HASTY BOOK!—why, its very first sentence states that it is the result of *eight years' labour!* Experiments on harmonics omitted!—why, they form the substance of the entire book, from beginning to end! From these, and many other misapprehensions of Mr. Chappell's, we are led to doubt whether he can even have read the great work he ventures so freely to criticise.

Prof. Helmholtz has always maintained cordial relations with this country, and in the name of English science we think we owe him an apology that anything like this should have appeared in our language under a quasi-scientific guise. He will, however, know that historians may rush in where philosophers would fear to tread, and we need hardly assure him that no English scientific man, competent to judge of his work, would be in the least likely to endorse Mr. Chappell's criticisms.

We lament Mr. Chappell's mistake on another ground. Practical musicians have generally but little knowledge of the scientific data on which their art depends; such information is never taught in England to professional students as any part of their musical education; it is studied almost exclusively by men of science and amateurs. All right-minded persons would gladly desire to promote the wider spread of knowledge of this kind; but we cannot but feel that when a practical musician takes it into his head to attack scientific authorities who are universally respected, and scientific doctrines which are universally established, a great obstacle is thrown in the way of that cordial sympathy and co-operation which ought to exist between the two classes. On the one hand, the scientific man will be angry at the perverse unteachableness of the musician; while, on the other hand, the musician, who may easily mistake error for truth, will be set against the theorist and be more disinclined than ever to receive information from him.

It would be an ungracious task to point out in detail Mr. Chappell's errors; we would rather recommend him, instead of waiting for Prof. Helmholtz to "revise his next edition," to read the work as it is, more thoroughly and carefully, and with more respect for the character of its author. And in the meantime, out of sincere good will, we earnestly advise him to expunge all this irrelevant matter; it not only damages his valuable book, but, what is worse for him, it tends to engender in the minds of the best class of readers a want of confidence in his judgment and accuracy as regards other things.

FOSTER AND BALFOUR'S "EMBRYOLOGY"

The Elements of Embryology. By M. Foster, M.A., F.R.S. and Francis M. Balfour, B.A. Part I. (London: Macmillan and Co., 1874.)

"STEP by step the simple two-layered blastoderm [of the hen's egg] is converted into the complicated organism of the chick." The separate cells of which it is originally composed have, to all appearances, the most uncomplicated relations one to another; nevertheless, in accordance with laws of which we have not the least conception, under the influence of slight external warmth, by a series of fissures, inflections, and developments in special directions, they convert the store of albuminous material that, together with them, is included within the egg-shell, into an organism so elaborate as a fully developed bird, which can run about and feed itself immediately it makes its appearance in the theatre of active life. The physicist, thoroughly acquainted as he may be with all the principles of statics, dynamics, heat, light, and electricity, finds himself quite at a loss to explain or to predict any single one of the numerous changes which have taken or will take place in this blastodermic membrane during any period, however short, that it has been the subject of observation. Neither the chemist nor the physiologist will find himself in any more advantageous position, except that the latter, from previous experience, will be able to state dogmatically the succession of the steps of the developmental process. We group these phenomena, apparently so extra-physical, under the term "vital"; and if at any time it should be shown, which is well within the region of possibility, that they depend on the manifestation of a force other than one of those with which we are at present acquainted, the disciples of the "vitalistic" school will have reason to exult over those "physicists" who do not admit the existence of any yet undiscovered mode of motion. As yet, the fact that one's parents in their earliest days went through the same changes as oneself is not considered a sufficient basis for any logical hypothesis on the subject of the progressive development of one's constituent elements.

Again, since the time of Von Baer, the marvellous parallelism which is so continually observed between the various development-stages of living beings considerably removed from one another in the scale of zoological affinity, has made the study of embryology an essential part of the science of Comparative Anatomy; in other words, the whole life-history of the individual, and not only the period of maturity, is now known to be necessary for our accurate comprehension of the pedigree of the animal kingdom, in the same way that it may be considered to reflect it. This conception has of late borne fruit in the all-embracing hypotheses of Prof. Hæckel and Mr. E. Ray Lankester, as well as in the new classification of the animal kingdom so recently promulgated at a meeting of the Linnean Society by Prof. Huxley (NATURE, vol. xi. p. 101). It may, however, be mentioned that there is a limit to generalisation in this direction; for the theory of natural selection allows us to assume that some of the forces which come into play to produce variation in the individual, and therefore generally, may do so at the very outset of embryonic life; and, if they do so, differences from the ancestral type may then appear in all the embryonic stages from the commencement. Such a view of the question helps to explain otherwise most involved subjects, such as the existence of "gastreæ" of two entirely different types; the development of the notochord from different layers of the blastoderm in different groups of animals; and other varying features of early embryonic life.

These remarks all indicate how large a field is opened up for the student of every branch of natural science by the study of embryology; and it is evident that before any considerable progress can be made in any of the many intricate problems involved, a minute acquaintance with the fundamental facts of development is indispensable. The work before us is the first systematic attempt which has been made, in this country at least, to place the whole subject on the required footing; and in how satisfactory a manner this has been accomplished will be attested by all who have taken the opportunity of studying it. When supplemented by the other two volumes promised by the authors in their preface, it will form a complete history of the most important changes known to occur during the embryonic life of the different groups comprising the animal kingdom. For a long time past such a work has been a great desideratum. The monographs of different authors are scattered over a whole library of books; many who require to employ the known results have but little time to investigate each sufficiently to form a sound opinion of their own, and fewer still are able to prosecute the somewhat special line of investigations on their own account. All working biologists, therefore, owe much to Dr. Foster and Mr. Balfour for the great care they have taken to sift the literature of the subject, as well as for their independent investigations, which add so considerably to our knowledge of a branch of biology which has but little attracted the attention of our own countrymen.

To turn to the subject-matter of the work itself. There are advantages possessed by the hen's egg, found in no other vertebrate embryo, which have led the authors to take the *history of the chick* as the starting-point for their subsequent descriptions. It is "the animal which has been most studied, and the study of which is easiest and most fruitful for the beginner." This must be evident to anyone who has had the least experience. A chronological order is followed, in which the changes which occur day by day, and sometimes even hour by hour, are fully traced through the earlier days of incubation; the incidents of the later days being much more briefly summarised, because they pertain more to the bird as a bird, than to it as a member of the sub-kingdom Vertebrata.

As above remarked, but little of the embryological work which has been undertaken since the time of the illustrious Harvey has been conducted in this country; it is therefore not to be wondered at that we are far behind the times regarding it. Many important points which for some time past have been familiar to foreign investigators, mostly German, are not sufficiently laid stress on, or are omitted altogether, in our physiological treatises and text-books. Among these may be mentioned the evanescent nature of the "primary groove" in the mesoblastic layer of the blastoderm, and its replacement by the "medullary groove," from which alone, and not from the former, the spinal canal is subsequently formed. "The primary groove, then, is a structure which appears early, and soon disappears without entering directly into the formation of any part of the future animal. Apparently it has no function whatever. We can only suppose that it is a rudiment of some ancestral feature," remark our authors.

The much-debated subject of the development of the blood-vessels and corpuscles is entered into in detail, and fresh investigations by one of the authors are recorded, which agree in many respects, as they remark, with those of Remak and Klein. The vessels are shown to be formed by the union of processes sent out from the mesoblast cells of the pellucid area. The nuclei of these cells enlarge and break up into numerous small ones, the majority of which acquire a red colour, and become converted into blood-corpuscles; whilst the rest, changing into a spindle-shaped form, develop into the synovial lining of the blood-vessels.

Another point of special interest is the development of the permanent vertebral column. As all know, the protovertebræ are developed at the sides of the notochord, with a neural arch attached mainly to the posterior end of each; whilst the root of a spinal nerve occupies the anterior portion. "On the fourth day the transparent lines marking the fore and aft limits of the protovertebræ are still distinctly visible. On the fifth day, however, they disappear, so that the whole vertebral column becomes fused into a homogeneous mass whose division into vertebræ is only indicated by the series of ganglia. This fusion . . . is quickly followed by a fresh segmentation, the resulting segments being the rudiments of the *permanent* vertebræ. The new segmentation, however, does not follow the lines of the earlier division, but passes between the ganglionic and the vertebral portions, in fact, through the middle of each protovertebra. In consequence, each spinal ganglion and nerve ceases to form the front portion of the primary vertebra formed out of the same protovertebra as itself, but is attached to the hind part of the permanent vertebra immediately preceding. Similarly, the rudiment of each vertebral arch covering in the neural tube, no longer springs from the hind part of the protovertebra from which it is an outgrowth, but forms the front part of the permanent vertebra, to which it henceforward belongs. . . . . By these changes this remarkable result is brought about, that each permanent vertebra is formed out of portions of two consecutive protovertebræ."

Such being the case, the question suggests itself as to what becomes of the portion of the new column corresponding to the anterior or cephalic end of the protovertebræ nearest the skull which has no other semivertebra wherewith to blend. It has no neural arch, and does not enter into the formation of the cranium, for the protovertebra does not enter that complicated structure. We are not informed as to its destination. May it not be that it persists as the odontoid process of the axis, which, from not being able to maintain an independent existence, joins, late as we know, the second cervical vertebra? This hypothesis involves a difficulty, no doubt, as to the nature of the atlas, but seems to throw some light on the peculiarity in the conformation of the axis.

The development of the vascular system, through the various complex stages, is most fully explained, with the assistance of several very instructive diagrams. We can hardly help having a feeling of regret that the common fowl does not resemble its allies, the Mound-makers (*Megapodidæ*), in having only a single carotid artery instead of two, because then we should have the question answered as to the method by which the companion vessel is lost, which is at present not in the least understood.

We must refer our readers to the work itself for an account of points so important as the development of the Wolffian bodies and their ducts, the spinal cord, the heart, the nasal pits, as well as the many other details respecting the different organs which go to form the adult bird; original observations will be found on most; and where these are absent, the excellence of the *résumé* of the work of others will clearly prove with what conscientious care the authors are carrying out the evidently pleasurable labour they have imposed upon themselves. As far as the permanent kidneys are concerned, it must be mentioned that, from their manner of development, it is shown that their separation morphologically from the Wolffian bodies is an occurrence of *purely secondary importance*.

Now that we have a text-book of embryology produced under such favourable auspices, it is to be hoped that the far-spread ignorance on that subject, which is at present but too apparent on all sides, will no longer exist, and that a higher standard whereon to commence further investigation will quickly develop itself amongst all English students of biology.

A. H. G.

OUR BOOK SHELF

A Monograph of the Post-tertiary Entomostraca of Scotland. By G. S. Brady, H. W. Crosskey, and D. Robertson. (Palæontographical Society.)

In this part of its publications the Palæontographical Society has done good service to that large body of geologists who take interest in the story of the old glaciers and icebergs of Britain and love to gather when they can the traces of the life which peopled the frigid sea once spread over some of the richest tracts of our present islands. The descriptions here given of the localities and sections of the glacial deposits are perhaps all that could be at present attempted, but they offer a very puzzling problem to the reader who would fain know something of the chronology of the deposits. Nothing can show more satisfactorily the labour which has in recent years been bestowed upon these Post-tertiary clays than the fact that a few years ago not one of the minuter forms of Crustacean life had been noted as occurring in them, while now more than 130 species belonging to twenty-seven genera of Entomostraca have been carefully examined and described by the authors of this Monograph. The names of Brady and Robertson are a sufficient guarantee for the faithfulness of these descriptions, while Mr. Crosskey's knowledge of the localities and his expertness as a collector have given an additional fulness and value to the Monograph. Though but dry reading for ordinary people, these pages, with their accompanying admirably executed plates, will be a valuable boon to many a student of Post-tertiary geology.

The Races of Mankind: being a Popular Description of the Characteristics, Manners, and Customs of the Principal Varieties of the Human Family. By Robert Brown, M.A., &c. Vol. II. (London: Cassell, Petter, and Galpin. *No date.*)

This work, which seemed in the first volume to promise some scientific value, is now down to the level of the popular picture-book. There are some good pictures in it, and no doubt the boys and girls who have it given them will pick up ideas from its compiled information. We have not done more than look into it here and there, finding errors small and great. At p. 4, a Spanish-American is called, with curious felicity of blundering, "Don Jose Marie del Muchos Dolores." At p. 22 we read, "The smallest shopkeeper in Germany expects to be addressed as Mr. Court-Councillor." At p. 284 is a picture of a Bushman playing on the goura, or musical bow. This appears to be an altered copy of the illustration in Mr. J. G. Wood's "Natural History of Man," vol. i. p. 295; but whereas Mr. Wood's artist knew that Bushmen have narrow heads, and drew his accordingly, the present draughtsman, by his alteration, has given his native a skull of enormous width. At p. 113, in contradiction to the weight of ethnological evidence, the Australians and Tasmanians are treated as belonging to the same race. When we add that many of the illustrations are taken without acknowledgment from Figuier's "Human Races," it is not necessary to inquire further where M. Figuier got them.

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 Royal Agricultural Society and the Potato Disease

My main object in writing to you was to correct what Mr. Jenkins admits were "grotesque statements," and to claim for a distinguished English botanist credit for work done by him thirty years ago, which I was unwilling, without protest, to see assigned to anyone else.

No doubt the Royal Agricultural Society was not founded for the advancement of science in general or of botany in particular. When, however, it transcends the practical limits it has imposed upon itself, and promotes a purely scientific investigation, the way it sets about it is, I suppose, a fair object of criticism in a scientific journal.

Mr. Jenkins complains that I have not taken the trouble to read the official reports published by the Society, and thinks my criticisms upon them might have had some value. As a matter of fact I have done so, and my difficulty is to be quite sure that I understand what the last and most important really means. To say nothing of the occurrence of "jonidia" for "conidia," I find the following sentence:—"Prof. de Bary expresses sanguine hopes that he has at last discovered the certain nids (*sic*) or resting-places of the oospores or active primary germs of the fungus." It would not have occurred to me to describe oospores—in other words, resting-spores—as *active*, and it has been suggested to me as not impossible that oospore may also be a misprint in place of zoospore. There is the more necessity for caution in the matter, as the publications of the Royal Agricultural Society do not seem to receive the botanical revision that might have been expected. Only last year—and it was not a solitary blunder—a fungus was figured in the Society's Journal as Aspergillum (*sic*), which was obviously no Aspergillus at all, but the common Bread-mould (*Ascophora Mucedo*). No doubt, in due course, we shall have the opportunity of reading, at full length, what Prof. de Bary has added to our knowledge of the matter; but in the meantime we should not forget what is due to those who have already worked at the subject in this country.

Mr. Jenkins denies that the Society offered prizes for disease-proof potatoes. I find that in the report of the judges on the abortive essay competition, presented to the Council, Dec. 10, 1873, it was recommended, "That valuable prizes be offered for (*a*) The best disease-proof early potato; (*b*) The best disease-proof late potato." Again, the recently published official report to which I have already referred commences: "The judges appointed to inspect the growth of the six varieties of potatoes, *which were entered for competition as disease-proof*," &c. It may be that this is "colloquial" language, and does not mean what it says; but Mr. Jenkins must know that if by any chance any one of the potatoes tried had run the gauntlet of the three years' trial, it would have been advertised far and wide as stamped with a "disease-proof" character by the Royal Agricultural Society.

Mr. Jenkins complains that I suggest an offensive spirit as actuating the Society in its communications with Prof. de Bary. I can only say that I used the Society's own language. I find that the judges, in their report, *after declining* to recommend any one of the ninety-four essayists for a prize, propose "That a sum of money (say 100*l*.) be granted *for the purpose of inducing* a competent mycologist to undertake the investigation of the life-history of the potato-fungus" (as if nothing had been done in it already). The joint Botanical and Journal Committee thereupon gave notice that they would ask for a grant of 100*l*. to carry out this recommendation. I am not aware that the British Association proceeds in this way in distributing its funds, and I leave Mr. Jenkins to reconcile what I have quoted with his statement, "that the *first* step taken by the Council of the Society was to direct me to write to Prof. de Bary."

Let me sum up the substance of my criticisms. The potato disease has been before the scientific world for thirty years, and has been investigated by Berkeley in England, Montagne and others in France, De Bary in Germany. The Royal Agricultural Society takes charge of a competition which induces ninety-four persons to write on a subject on which it was *à priori* in the last degree improbable that they could have any really important unpublished facts to bring forward within the limits of even the extended time at which the essays were to be sent in. On the failure of this scheme prizes are offered for disease-proof potatoes, "disease-proof" being subsequently defined to mean immunity from disease in twenty different districts for three years. Were a disease-proof potato a probable thing, it might clearly be trusted to establish its own reputation. Lastly, the amateur world of prize essayists having proved fruitless, the cryptogamic botanists of this country—many of them men of European fame, who would doubtless have willingly responded to an appeal from the Council to co-operate in the matter—are passed over *en bloc*, and the matter is placed in the hands of a German scientific man—highly and worthily distinguished, doubtless—but who, I am convinced, would be far from approving the slight placed on our countrymen, one of whom has accomplished what will ever be a classical research in this very subject. I submit that when I applied the expressions "spasmodic," "ill-considered," and "wanting in scientific method" to these proceedings, I was not using inappropriate language.

W. T. THISELTON DYER

Sensitive Flames

PERMIT me to thank Prof. Herschel for his all too kind acknowledgment of the aid my former brief communication to NATURE may have been to him. In a paper on Sensitive Flames that is awaiting the needful leisure to complete, I have given a brief history of this subject—which, by the way, so far as regards the discovery of sensitive flames, Prof. Herschel has partly misapprehended, though there can be no doubt the valuable letters of Prof. Herschel will play an important part in the development of these phenomena. I am glad to find that, so far as Prof. Herschel has recorded his views, they corroborate the results of my own experiments (begun as long ago as 1867) in search of the cause of the sensitiveness of various fluid jets, and the application of sensitive flames to acoustic investigation and other practical ends. For reasons, into which I will not enter here, I was led to postpone this inquiry, and it is only comparatively lately that it has been resumed.

The keynote to the whole of the phenomena is, I believe, to be found in Savart's beautiful investigations on liquid jets. Any fluid body, gaseous as well as liquid, escaping from an orifice in a tranquil stream, consists of a continuous and a discontinuous region, and is subject to the play of opposing forces which excite pulsations in the jet, the number of which is directly proportional to the velocity of the issuing stream, and inversely as the diameter of the orifice. When a note is sounded approximately in unison with the vibration number of these pulsations, the jet of water, smoke, or flame is thrown into more vigorous vibration, and a strained condition of the jet is set up.

Hence it is easy to obtain a series of sensitive flames, issuing from orifices of decreasing size, capable of responding (within a certain range) to the successive notes of the gamut; the higher notes affecting, of course, those flames from the smaller orifices, and which also require to be under greater pressure of gas than the flames responding to the lower notes. The relative rate of vibration of these flames is at once clearly seen by viewing them together in a moving mirror. But I will not weary your readers by further entering upon a subject with which already they must be somewhat tired.

W. F. BARRETT

Royal College of Science, Dublin, Nov. 30

Fossils in "Trap"

I AM much obliged by your insertion of my letter on "Fossils in Trap." You are right in supposing that the trap I referred to was crystalline augitic trap. If it had been tufa I should not have written to you as I did, as I was well aware that fossils in tufa were of common occurrence. Shortly after I wrote I found that the *Favosites gothlandica* which shows the section is still imbedded in a portion of the slate, which is olive-coloured, and closely resembling the trap. This is so intimately connected with the trap that it is impossible to trace a *line* of connection.

Halifax, Nova Scotia, Nov. 14 D. HONEYMAN

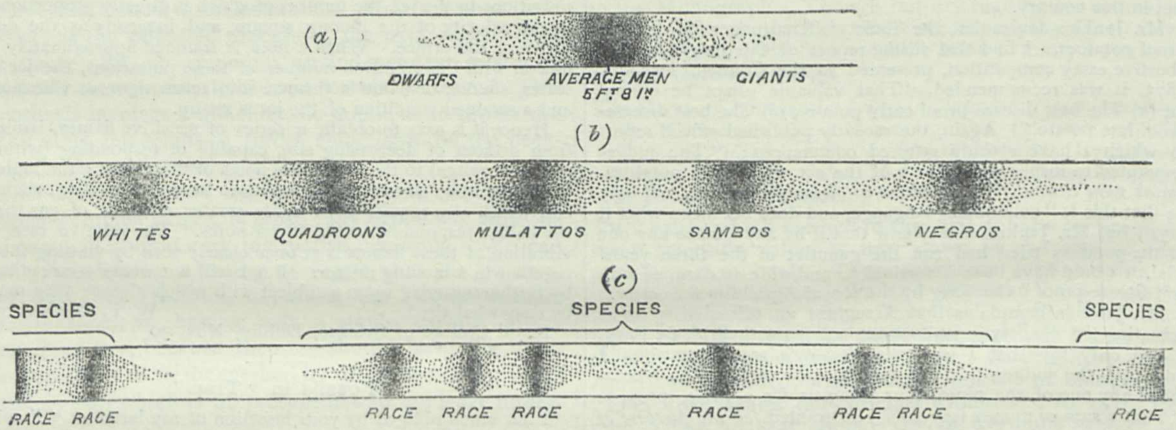
[Dr. Honeyman's discovery would appear to resolve itself into the simple fact that his "trap-dyke" has involved in its mass fragments of the fossiliferous strata through which the molten rock has risen—a fact, we presume, with which every practical geologist who has worked amongst igneous rocks must be more or less familiar.—ED.]

THE RELATION OF RACE TO SPECIES

IN a notice of Quetelet's works, published in NATURE, vol. v. p. 358, I raised the question whether this eminent statistician's method of defining a race or population might be applied to provide naturalists with a means of defining species. Since then, the consideration of Mr. Francis Galton's explanatory diagram, given at p. 28 of his work on "Hereditary Genius," has led me to attempt to carry this problem a stage further.

Instead of using, with Quetelet, a binomial curve to show the constitution of a race, with its central type and varieties, Mr. Galton sets before our minds the very individuals who compose the mass, each one being represented by a dot. His diagram, adapted in (*a*) of the present figure, stands for a population descended from a common ancestral stock, the individuals congregating most closely about the place of the central type or standard individual, and gradually decreasing in numbers as they become more different from that type or standard. In this graphic representation, the race can, of course, only be arranged in order as to some one quality. In the particular case for which Mr. Galton uses it, this quality is stature. The individuals of the mean or average height (say, 5 ft. 8 in.) are shown as most crowded, while the taller and shorter men become fewer and fewer as their stature becomes more unusual, till at last we come to one or two outlying giants and dwarfs, beyond whom no more individuals exist. Here, then, is set before us the distinctest idea of a race, both as to its type and as to its limits of variation on either side. I now proceed to apply the method of this diagram to a more complex state of things.

In nature we habitually find races blending into one another. Our own species shows this perfectly, when mixed breeds are considered. Let a population partly of Europeans and partly of negroes be placed on a West Indian island. These two races being classified according to colour, a few of the darkest Europeans would be seen to make some slight approach towards a few of the lightest negroes; but there would be no individual of either race who could be mistaken for one of the other. They would, therefore, at the outset be represented by two such groups of dots as (*a*), with a blank space between. But as soon as the first generation of mulattos come into existence the case will be altered. An intermediate race has arisen with its definite central type, and its variants now coming much closer to the whites on one side and to the blacks on the other. In the next generation there will be quadroons and sambos (cross between negro and mulatto, Spanish *zambo*). Now the fusion will be so complete, that of many individuals it will hardly be possible to say whether they are quadroon or mulatto, while in the same way others may be either mulatto or sambo, or either sambo or negro. One or two more generations would still further obliterate the distinction between adjoining varieties, but for convenience sake the figure (*b*), showing the blended races, is taken only in the second generation. In this way the whole human species, or any species of plants or animals, may be ideally classified into its various races, either in fact blending into one another, or capable of so blending by intercrossing. A species thus classified into its component races is shown either in (*b*) or the central part of (*c*).



Let us now attend to the effect of variation, artificial or natural. Starting with a single race, this may in the course of time and circumstance develope within itself a number of varieties or races. Nor, if variation is promoted either under domestication or by various conditions of life acting for a long series of generations, is there any difficulty in conceiving two adjacent varieties to recede from one another and the intermediate individuals to die out, till a wide gap is left between the two races. At first this gap, though real, would be capable of being at any time bridged over by cross-breeding, and thus would only be a temporary break. But as variation went on, a critical period would at last be reached, when individuals from the two sides could no longer produce fertile offspring. Then a separation of one species into two would have taken place. This change is illustrated in (*c*), where the extreme forms of two adjacent species are seen to the right and left, still perceptibly near the extremes of the original species from which they have parted, but never to be joined to it again unless by a process of backward variation most unlikely to happen across any width of interval. This ideal representation was at first intended rather to show the actual distribution of animals in existing species than to involve a hypothesis as to how these species originated. But, after consulting Mr. T. R. Stebbing, I see the desirableness of making the diagram express both facts and hypothesis, leaving those who will to take them apart. The whole figure, as it stands, contains an ideal of evolution or development from a single race of animals at (*a*), into a species made up of several races at (*b*), and thence into any number of separate species at (*c*).

EDWARD B. TYLOR

TRANSIT OF VENUS

Colonel Tennant's Station at Roorkee, India.

THE full and very able account of the preparations for observing the Transit of Venus drawn up by Prof. Forbes and published in these columns do not include those which have been made by the Government of India under the authority of the Secretary of State in Council. When Prof. Forbes wrote, these were not sufficiently advanced to admit of description. Now that they are completed it is desirable that an account of them should be made public.

At an early period Col. J. F. Tennant, R.E., F.R.S., brought the subject before the Viceroy in India, and proposed the organisation of a station in the north-west, near Roorkee, well known as the seat of the great Civil Engineering College. The Viceroy heartily responded to the suggestion, and communicated his views to the Home Government.

Some time was unfortunately lost in official correspondence, and it was not until July 1873 that I received final authority to order the necessary instruments. With the sanction of the Secretary of State, I conferred with Mr. Warren De la Rue on the subject, and am indebted to that gentleman's zeal and experience for a great deal of valuable assistance.

The following is a list of instruments prepared by Mr. De la Rue and myself, and sanctioned by Government:—

1 Photoheliograph.
1 Equatoreal.
1 Altazimuth.
1 Transit Instrument.
1 Chronograph.
1 Standard Clock.
3 Journeyman Clocks.

I will briefly describe each of these.

Photoheliograph.—This is of the same identical size and construction as those supplied to the stations equipped under the auspices of the Astronomer Royal. It is well known that these have been constructed from the designs of Mr. De la Rue and under his close personal supervision. I therefore advised that that gentleman, as the first authority on the subject, should be requested to superintend the provision of this particular instrument, and that he should have *carte blanche* for the introduction of any improvements or additions suggested by his constantly enlarging experience. My recommendation was adopted, and I need hardly say that Mr. De la Rue, with his usual public spirit, at once gave his services to the Government and to science. This instrument, like its prototypes, was made by Mr. Dallmeyer. Janssen's apparatus, modified by Mr. De la Rue, for multiplying the photograms, has been supplied to this instrument.

Equatoreal.—This has an object-glass of six inches clear aperture. It was made by Messrs. Cooke and Sons, of York, and is generally of the form which those artists have made familiar to the astronomical world. It is a universal instrument, being capable of adjustment for any latitude to 67° 39′, and in either hemisphere, for which latter purpose it has reversible driving-gear and two hour-circles readily interchangeable. Besides a striding level for making the declination axis horizontal, two delicate levels are suspended from the centre of the telescope. A graduated circle and delicate level is also attached to the telescope near its eye end. This, in conjunction with a micrometer in the eye-piece, will qualify the instrument for determining latitude by the differential observation of two stars of nearly equal zenith distances, north and south. The instrument has two micrometers—a parallel wire by the makers, and a double image micrometer by Messrs. Troughton and Simms, precisely similar to those supplied for other stations under the auspices of the Astronomer Royal. The parallel wire micrometer has a contrivance intended to enable the observer to record the readings of the divided head without withdrawing his eye from the eye-piece, as suggested by Mr. Christie, of the Royal Observatory. The form actually adopted by the makers differs both from that employed by Mr. Christie and that prescribed by me. I am unable to express any opinion on its efficiency, as it arrived from the makers after the instrument had been despatched to India, and I could not try it.

The instrument is well supplied with eye-pieces and all necessary adjuncts, and the arrangements for bringing down the various adjusting and slow-motion screws to the observer's hand when observing are very complete, and many of them, I believe, as novel as they are ingenious and effective.

I had but one brief opportunity of trying the object-glass, but that sufficed to satisfy me that it is of a high order of excellence.

Altazimuth.—The place of this is supplied by the great theodolite constructed, from my designs, by Messrs. Troughton and Simms, for the Great Trigonometrical Survey of India, and now about to be employed for the first time. The main features of this instrument were described by me in a paper read before the Royal Society and published in its Proceedings, No. 135, of 1872. I may briefly state here that the horizontal circle is three feet in diameter, read by five equidistant micrometers, the circle being fixed and the micrometers revolving; that the vertical circle is two feet in diameter, read by either two micrometers fixed, or by four capable of being shifted so as to change the divisions and thus reduce errors of graduation; and that the telescope has an aperture of 3·25 in., and a focal length of 36 in., to which can be attached either of two parallel wire micrometers—one for measuring in the vertical, the other in the horizontal plane, according to the class of observation, the wires having both a dark and a bright field illumination.

This is no doubt the most elaborate and most powerful instrument of its class in existence.

Transit Instrument.—This is of a peculiar and, I believe, in many respects novel form. It is from the designs of Signor Magnaghi, of Genoa, modified by the makers, Messrs. Cooke, of York, and myself. Signor Magnaghi's object was to produce an instrument capable of determining latitude, as well as of performing the usual functions of a transit instrument, those of determining time and longitude. The telescope has an aperture of 3 in. and a focal length of 34⅞ in. with a transit axis of the usual form 18¼ in. in length between the pivot shoulders. The stand is of cast iron, and consists of first a massive circular base plate 24 in. in diameter, supported, according to the system introduced by me, by three shakeless foot-screws, upon a single masonry pillar. On the base plate revolves horizontally a second similar plate with the two pillars cast hollow in one piece with it. This upper revolving plate moves stiffly, and is provided with slow-motion screws and four powerful clamping bolts. When it is bolted to the lower base plate, the two may be considered practically to be one mass. The horizontal motion thus provided is used, first, for effecting the ordinary azimuthal adjustment of the instrument to the plane of the meridian; and secondly, the direction of the meridian having been found, for placing the telescope with great facility in the plane of the prime vertical for the determination of latitude by that method.

An apparatus is also supplied for lifting the telescope and reversing its pivots on their bearings, the transit axis level remaining suspended from the pivots during the process. This arrangement also admits of the instrument being used to determine latitude by the differential observation of two stars of nearly equal zenith distances, north and south; for which purpose the telescope is provided with a parallel wire micrometer and a delicate level.

Chronograph.—Col. Tennant attached, justly I think, great importance to means being provided for the electric record of the time observations which form so essential a portion of the undertaking. He wished to have a chronograph which should be capable of recording at the same time, and without confusion, observations made with four different instruments, viz., the Photoheliograph, Equatoreal, Altazimuth, and Transit Instrument, and he indicated the apparatus described by Lord Lindsay in Monthly Notices, R.A.S. I therefore examined Lord Lindsay's chronograph, then under construction by Messrs. Cooke, of York, but, though no doubt suitable for a fixed observatory, such as his lordship is establishing, I thought it too large and ponderous for the present service. I accordingly arranged with Messrs. Cooke a much lighter and, I may add, a less costly plan. By substituting continuous bands of paper, similar to those used in telegraphic instruments, for sheets of paper carried by the large barrels employed by Lord Lindsay, great increase of compactness and lightness were secured. Four such bands are worked by one central clock movement, each or all being readily thrown out of gear at will. The marking is effected by steel prickers driven by electro-magnets, on the same principle as in the chronograph of the Royal Observatory, Greenwich, though the mechanical details were different. These prickers can also be thrown out of action by a convenient arrangement of resistance coils. The length of a single second of time can, by changing two wheels, be made either one, three-quarters, or half an inch. I had prescribed that the central clock work should be regulated either by the late Léon Foucault's lever governor or by Mr. Siemens' centrifugal fluid governor, but the maker, without consulting me, applied a Watts ball governor with friction-brake, such as is employed for the driving-clock of equatoreals. I did not expect that this comparatively primitive contrivance would secure a sufficiently uniform velocity. But on trial by Capt. Campbell, R.E., and myself, it was found to answer its purpose so well that I am inclined to think a great deal of needless refinement and expense has been wasted on elaborate governors for chronographs.

Standard Clock.—This is by Messrs. Cooke, of York, and has nothing peculiar in its construction. It has a Graham's dead beat escapement, and a mercurial (metal jar) compensated pendulum, with the contact apparatus necessary for connecting it electrically with the chronograph.

Three Journeyman Clocks.—These were intended by me to be connected electrically with the Standard Clock, and thus show identical time for each of the principal instruments from which the latter might not be visible. I was not satisfied with the mode of driving adopted by the makers, and should have had them altered if time had admitted. Col. Tennant is also dissatisfied with them, but I hope that, with careful adjustment and attention to the batteries and contacts, they may be found effective during the short period of the phenomenon.

It fortunately happened that whilst these instruments were undergoing examination by me, Capt. W. A. Campbell, R.E., of the G. T. Survey of India, who is to assist Col. Tennant with the Venus observations, was in England. The Government, on my application, appointed Capt. Campbell to assist me in testing the instruments, and thus the two objects were gained of securing his valuable experience and skill, and of familiarising him with the instruments which he would have to use.

I have heard from Col. Tennant of the safe arrival at Roorkee of the photoheliograph, altazimuth, chromograph, and clocks, and of the expected arrival in a few days of the Equatoreal, his last letter to me to that effect being dated 9th Oct., 1874. There would only remain the Transit Instrument, which was much delayed in construction. It was despatched hence on Sept. 18, 1874, and is no doubt now in Col. Tennant's hands.

In the foregoing statement I have confined myself to those arrangements which I have been personally concerned in making. But other places in India will be provided with equipments more or less complete for observing the Transit of Venus—amongst others I may mention Peshawur, Bombay, and probably more than one station in the southern part of the peninsula under the care of Mr. Pogson, Government Astronomer at Madras. The Government of India has thus not been unmindful of the just claims of astronomical science.

A. STRANGE, Lieut.-Colonel,
Inspector of Scientific Instruments
to the Government of India

Lambeth Observatory, Nov. 1874

PRACTICAL SCIENCE AT CAMBRIDGE

DR. MICHAEL FOSTER, in concluding his course of Practical Physiology this term, remarked on the diligence and industry of his class under many difficulties. At the beginning of the term he asked their indulgence for the imperfect accommodation he was able to offer them. Thirty students had been entered, and the space available was about sufficient, properly, for ten. Three students had to be placed at each table, instead of one. Several other gentlemen joined the class subsequently, making the class number about thirty-five. Two ladies also attended the lectures, and were provided with a separate place of study. Dr. Foster at his last lecture said that in the previous year the want of accommodation had been so keenly felt by himself and class that he was inclined to discontinue his course. He had, however, conducted it through another term, with a larger number of students; and, as the result, although he expressed pleasure at the work accomplished by his class, he was more than ever inclined to give it up. The present course would, however, be completed next term; but he was not able to promise its repetition in the succeeding winter. Want of accommodation militated so greatly against the quality of the work done, and so limited the kind of work that could be attempted, that the benefit seemed almost to be outweighed by the limitations and disheartening accessories.

The publication of these remarks may serve to draw attention to the general condition of practical science in Cambridge. Chemistry and geology are perhaps the subjects for the practical study of which we now have the most reasonable facilities. The Chemical Laboratory has been recently enlarged and improved, and in addition to the ordinary practical courses Prof. Liveing has this term given lectures with practical illustrations in spectroscopic analysis. The lectures have been given during four successive hours of the afternoon, to four sets of students, the number of students in each class being limited to four or five; so that thoroughly efficient work could be done. The facilities for study at the Geological Museum have been improved by Prof. Hughes. A typical collection of fossils has been selected and arranged by Mr. Keeping, and provided with catalogues. A typical series of minerals has been arranged and catalogued by Mr. W. E. Koch, B.A., of St. John's College, derived from the ample stores accumulated by the late Prof. Sedgwick. Several large series of rock specimens have been more conveniently arranged for inspection, including those catalogued by the Rev. T. G. Bonney. In addition, advanced students have free access to the many valuable special collections in the Woodwardian Museum. The Geological Library in the museum has been improved and catalogued; a valuable section-cutter and an excellent microscope have been purchased, and in other ways the means for the practical study of geology, so far as it can be carried on in a museum, have been greatly improved.

In Experimental Physics the best conditions for practical study have been secured in the building of the Cavendish Laboratory, in its being furnished with some of the most perfect and valuable physical apparatus in existence, and in the appointment of Prof. Clerk Maxwell and his able demonstrator, Mr. Garnett. No doubt at the earliest possible moment a practical elementary course will be organised, to include those observations which every student of natural science should become familiar with. Sufficient time has not yet elapsed since the completion of the laboratory for the establishment of such an elementary class; but when it is established a great boon will be conferred on Natural Science students, who, in the study of biology and geology, labour under many difficulties caused by a want of sufficient practical acquaintance with physics. It would be very desirable, also, if some elementary non-mathematical lectures on physics could be given for the benefit of Natural Science students; such lectures might be given by the Demonstrator, so as not to interfere unduly with Prof. Maxwell's researches and advanced mathematical lectures. It is true that Mr. Trotter gives valuable lectures on physics at Trinity College, but these are restricted to members of those colleges which are associated with Trinity for Natural Science studies.

The great hindrance to the success of the Cavendish Laboratory at present is the system fostered by the Mathematical Tripos. The men who would most naturally be the practical workers in the laboratory are compelled to refrain from practical work if they would gain the best possible place in the Tripos list. Very few have courage so far to peril their place or to resign their hopes as to spend any valuable portion of their time on practical work; for, while they might be acquiring sound physical conceptions and going through long laborious details, others are assiduously cramming book-work, wearing out their energy in attacking those problems which are here set before the student as affording the best mental training, and in learning those short cuts and dodges which conduce to obtaining marks in an examination. For a man to do practical work in physics at Cambridge implies considerable exercise of courage and self-sacrifice.

Students of the foregoing subjects, however, have better facilities for study than students of Biology. In Practical Physiology and Histology almost everything is required. A large room, properly lighted and fitted, is needed for elementary courses; and, considering the numbers already attracted to Dr. Foster's summer and winter classes, in spite of difficulties and defects, it would seem desirable to provide accommodation for at least a hundred students. Rooms should also be provided, specially adapted for advanced work in Histology, for researches in Physiology, for preparation of experiments and of materials for the classes, and, in addition, a good lecture-room.

In Comparative Anatomy and Zoology the museum has been much improved in the last few years, but its growth is greatly restricted by want of funds. The accommodation for practical dissection of animals consists only of the superintendent's private room, which, at the cost of great inconvenience, has been generously thrown open to students.

Finally, as regards Botany, while there are a good garden and a carefully kept herbarium for systematic study, there is no class of any kind for practical study of Vegetable Histology and Physiology. And yet, recently, the standard for obtaining an ordinary degree in Botany has been considerably raised, and students are expected to show knowledge of the forms, sizes, and development of cells of every kind. The demand for an acquaintance with Vegetable Histology, which, to be real, must be acquired by assiduous and carefully directed microscopical study, while no instruction in such work is given, puts a premium on cramming of the most unfruitful kind, and reduces natural science studies to a lower level than those mathematical and classical studies whose exclusive pursuit scientific men desire to see abandoned. It would be better to examine only in those portions of morphology and classification which can be learnt in a botanic garden, than to set elaborate questions in Histology and Physiology which necessitate elaborate cramming on the part of the student. G. T. BETTANY

Cambridge, Dec. 10

M. BECQUEREL ON SOLAR PHYSICS

THE Paris Academy of Sciences having appointed a Commission to consider the founding of an Observatory for Physical Astronomy in the vicinity of Paris, M. Becquerel the elder, a member of the Commission, has expressed his opinion on the subject in a report of which the following is a translation:—

To study the physical constitution of the sun and stars, Astronomy employs in general telescopes and the spectroscope; this last instrument shows us that the heavenly bodies are composed of the same elements that are found in the earth; whence it may be concluded that the forces governing matter are of universal existence. This question I have considered in a work now going through the press, and which will appear before the end of the year; its title is "On the Physico-chemical Forces and their Intervention in the Production of the Phenomena of Organic and Inorganic Nature." All questions relating to these subjects are there treated, not theoretically, but by the experimental method.

I have endeavoured to show that to arrive at a knowledge of the sun's constitution it is necessary to call to our aid the geological constitution of the globe and volcanic phenomena from the earliest times down to the present epoch.

The following are the reasons which have led me thus to deal with the subject:—

The identity of formation of the sun and earth and of all the planets which gravitate around our principal star being admitted, the conclusion may be drawn that his present physical condition is the same as that of our planet during the first periods of its formation, when the crust did not exist or had but little thickness. The cooling of the earth has been considerably more rapid than that of the sun by the effects of celestial radiation, the volume of the sun being 1,326,480 times that of the earth. It is thus permitted to compare the chemical and physical effects occurring in the sun at present with those which were produced in the earth at its origin, from which conclusions may be drawn as to the actual constitution of this star.

The collection of vapours which constituted the earth, submitted to a gradual cooling, passed successively from the gaseous to the liquid state, after which its surface became covered by a solid crust, of which the thickness increased with time. There were then produced a mass of chemical and physical phenomena.

We may distinguish three principal calorific epochs during the formation of our planet.

The first is that in which all the elements were in a gaseous state in consequence of a temperature excessively elevated; all the constituents were then dissociated.

The second is that in which, the temperature being sufficiently lowered, affinities commenced to exercise their action; the compounds formed passed successively from the gaseous to the liquid and solid states. During all the chemical reactions which occurred there would be produced an enormous disengagement of electricity arising from the energy of these reactions, and, as a consequence, a recomposition of the two electricities which would rend with vivid gleams the atmosphere already formed. Thunder would burst forth from all parts.

The third epoch is that in which the temperature, being sufficiently lowered and below 100°, the quantity of water formed would increase so much the more as the temperature was less elevated. This primordial water contained, probably, carbonic, sulphuric, and other acids which would saturate bases; it is to the reactions produced that must be attributed the formation of the great masses of limestone found in various parts of the earth's crust.

I have been led also in my work to treat of the calorific state of the earth in the first phases of its formation, as also of the volcanic phenomena of the same epochs.

As a consequence of the subjects discussed, I have been led to show that atmospheric electricity had a solar origin, and is the cause of the aurora and probably of the luminous phenomena which are produced beyond our atmosphere. I here limit myself to the indication of the consequences to which the study of the forces of nature has led me.

From what precedes it will be seen that the study of the constitution of the sun requires the conjunction, not only of astronomy, but of observers having general knowledge in Physics, Geology, and Chemistry, and possessing a thoroughly practical knowledge of the spectroscope.

R. M.

REAPPEARANCE OF ENCKE'S COMET

IT is quite possible that before the close of the next period of absence of moonlight in the early evening hours, the comet of Encke may be again detected with the large telescopes now to be found in our observatories. The mean motion determined by Glasenapp for the last perihelion passage at the end of December 1871 would bring the comet to the same point of its orbit about 1875, April 11.5, which was very nearly the date of passage through perihelion in 1842. When it was last in aphelion, in the middle of August 1873, I find its distance from the planet Jupiter would be 10·02, and that from Saturn 7·3, so that the perturbations during the present revolution are likely to be small; the comet still approaches near the orbit of Mercury in heliocentric longitude 123·7° and latitude 6·8° N., but it has not encountered that planet since November 1848. Assuming, then, that the least distance from the sun will be attained at midnight on the 11th of April next, we have the following positions of the comet during the period I have named :—

AT 12H. GREENWICH TIME

1874-75.	R.A. h. m. s.	N.P.D. ° ′	Distance from Earth.	Distance from Sun.
Dec. 22 ...	22 50 31	88 19·9	1·945	1·919
„ 26 ...	22 53 12	88 9·9	1·958	1·874
„ 30 ...	22 56 18	87 57·2	1·968	1·828
Jan. 3 ...	22 59 47	87 41·6	1·976	1·781
„ 7 ...	23 3 38	87 23·2	1·981	1·733

An acceleration or retardation of four days in the time of perihelion passage will not change the geocentric place more than fifteen minutes of arc, so that if the comet be within reach it may be easily found.

It will be interesting to learn what account some of the large reflecting telescopes with which many amateurs in this country have provided themselves, can give of the comet at this return.

The computation of the perturbations and preparation of an accurate ephemeris for 1875 is understood to be in the hands of Dr. von Asten, of Pulkova; but I am not aware that the results have yet been given to astronomers.

J. R. HIND

Mr. Bishop's Observatory, Twickenham, Dec. 14

NOTES

SINCE our last week's note, we understand that the whaling steamer *Bloodhound*, of Greenock, has been purchased as the chief vessel of the new Arctic Expedition. Other whalers have been examined by Sir Leopold M'Clintock, but none have been deemed suitable. The *Bloodhound* is a screw steamer, whose engines are nominally 96 horse-power; she is barque-rigged, two years old, strong, sound, and well appointed, and handy either under steam or canvas. It is announced that the vessel chosen to be the consort of the steam-whaler *Bloodhound* in the forthcoming expedition is Her Majesty's ship *Alert*. She is a five-gun steam-sloop of 751 tons old measurement, and 100 horse-power nominal. The *Alert* has been docked at Portsmouth and will undergo a thorough survey. Active preparations for the equipment of the ships will soon commence, but the start will not be made until the latter part of June of next year, as it is considered merely waste of labour and time to push across the north water until the ice has had time to melt and drift out from Smith's Sound. A request has been made by the Foreign Office that the Danish Government will permit their agents at Disco, Proven, and Upernavik to collect hunters, dogs, and dog-drivers for the Arctic Expedition. Capt. Nares is expected to arrive in this country about the end of January, 1875. The Committee for making arrangements with respect to the Expedition sat on Tuesday and Wednesday at the Admiralty for the purpose of deciding on the provisions and clothing to be supplied to the members of the expedition. They have been occupied hitherto with details as to the route.

Apropos of the possible biological results of the Arctic Expedition, we may recall to recollection a few additional details to those given last week of what was accomplished by the *Polaris*. The northern limit actually reached was 82° 16′. Yet at this extreme latitude fifteen species of plants were collected, five of which were grasses. Twenty-six musk oxen were shot in lat. 81° 38′. Dr. Bessels also made a fair collection of insects, principally flies and beetles, two or three butterflies and mosquitos; and birds of seventeen different kinds were shot in 82°, including two Sabine gulls and an Iceland snipe.

DURING the whole of the past week the members of the French Academy of Sciences have had frequent meetings to receive the telegrams from the several French Transit stations. The first, from Janssen, relieved them of a great anxiety, and was published instantly. The most extraordinary measures have been taken to secure the safe transmission of the results of the observations at French stations. The chief of each station is ordered to make four copies of his observations. One is to be left under a cairn, or a tree (if any in the country), or in an excavation, the site to be described in a letter to the Institute; the second is to be handed over to the captain of the first French ship that is met, with instructions to bring it himself to the Institute; the third is to be delivered to the nearest French consul, agent, or ambassador; the fourth is to be kept by the chief of the station himself.

MM. FIZEAU and Cornu, authorised by M. Leverrier, have been making an experiment of the highest importance at the Paris Observatory, the results of which were to be given at Monday's sitting of the Academy. The two *savants* have been measuring the velocity of transmission of light, by experiments carried on between the Observatory and Montlhéry. The light sent to Montlhéry is reflected and returns to the Observatory, the distance there and back being 22,000 yards. The experiment has never hitherto been made on so grand a scale, nor with such precautions; ten powerful instruments were used.

HER MAJESTY'S ship *Basilisk*, which has just returned to England after a commission of nearly four years, has (the *Times* states) surveyed about 1,200 miles of coast line, added at least twelve first-class harbours, several navigable rivers, and more than one hundred islands, large and small, to the chart; and, lastly, has been able to announce the existence of a new and shorter route between Australia and China. Till these *Basilisk* discoveries were made, a large archipelago of islands (some as large as the Isle of Wight, and densely populated), a rich fertile country, intersected by navigable rivers, and inhabited by a semi-civilised Malay race, remained unknown to us. After the news of this ship's first discoveries reached England, Lieut. Dawson, R.N. (Admiralty Surveyor), was sent out to join her, and she was ordered to complete and follow them up. This has been done with perfect success, and the whole of the previously unknown shores of Eastern New Guinea have been carefully surveyed, and the route above referred to opened up. The principal part of this work of discovery and surveying has been performed by the captain and officers in small open boats, detached from the ship in some instances for many weeks, and among savages who had never before seen a white face. It is stated that two lofty mountains, about 11,000 feet high, facing each other on the north-east coast of New Guinea, have been named "Mount Gladstone" and "Mount Disraeli." This intelligence will have an interest of rather a tantalising kind for naturalists. There is hardly any part of the world more promising to students of the geographical distribution of living forms than that which the *Basilisk* has surveyed. Collections, more especially of the plants, might doubtless often have been made, and would have been of the highest possible value. It is much to be wished that with the existence of such opportunities as these some one might be found to put in a word in aid of purely scientific claims. Doubtless it is pleasant to think that the two rival mountains will be a perpetual memory of frowns frowned elsewhere, but how much more pleasant to know something of the things that grow and live upon them.

THE *Irish Times* states that one of the objects of Sir Stafford Northcote's visit to Ireland is to "examine the sites proposed for the establishment in Dublin of an extensive National Museum of Science and Art, analogous in principle, although not in extent, to that at Kensington."

AFTER the Franco-Prussian war of 1870-71, it is well known that in many districts in France a new vegetation sprang up, evidently the result of the invasion. It was believed that this vegetation would become acclimatised. It is not so, however, *L'Institut* informs us ; at least very few of the species introduced in this way appear likely to continue to flourish on French soil. In the departments of Loiret and Loir-et-Cher, of 163 German species, the half at least have already disappeared, and the surviving species diminish in vigour each year. Scarcely five or six species would appear to manifest any tendency to become acclimatised ; these are, according to M. Nouel, *Alyssum incanum*, *Trifolium resupinatum*, *Rapistrum rugosum*, *Melilotus sulcata*, and *Vulpia ligustica*. On the plateau of Bellevue, where in 1871 many strange species were seen, M. Bureau has been able to find only one—*Trifolium resupinatum*. M. Gaudefroy also, who in 1871 and 1872 found many adventitious plants, has been able to collect only two this year—*Ranunculus macrophyllus* and *Linum angustifolium*.

THE Transactions and Proceedings of the New Zealand Institute always contain a collection of papers of high scientific value, and vol. vi. issued last June is no exception to this rule. It is a bulky volume of some 454 pages, added to which is an appendix of 104 pages more. It may not be known to many of our readers that the New Zealand Institute is composed of the following incorporated societies, each of which includes amongst its office-bearers and members one or more names eminent in science in the colony and well known in this country. The individual societies are, the Wellington Philosophical Society, Auckland Institute, Philosophical Institute of Canterbury, Otago Institute, and the Nelson Association for the Promotion of Science and Industry. On the council of these various societies occur such names as Dr. Hector, F.R.S., Dr. Haast, F.R.S., Mr. W. T. L. Travers, F.L.S., Mr. T. Kirk, F.L.S., &c. Each one of the societies numbers amongst its members the scientific men of its neighbourhood, and amongst the honorary members of the incorporated Institute are such names as Charles Darwin, Prof. Huxley, Dr. Hooker, Sir Charles Lyell, Prof. Owen, Prof. W. H. Flower, &c. These facts are sufficient to show that New Zealand is particularly fortunate in having amongst its residents men eminent in various branches of science. No colony has shown more aptitude for scientific work than New Zealand, and perhaps no other colony can boast of a society approaching so near to our Royal Society, both as regards the value of the papers contributed and the range of scientific investigation. Zoology, Botany, Chemistry, and Geology are all represented by numerous papers in each section. In the first, Dr. Haast contributes an illustrated article "On *Harpagornis*, an extinct genus of gigantic raptorial birds of New Zealand ;" while Dr. J. E. Gray, who is an hon. member of the Institute, supplies a "List of Seals, Whales, and Dolphins of New Zealand," and Capt. F. W. Hutton some "Notes on some New Zealand Fishes." In Botany we find a "List of the Algæ of the Chatham Islands, collected by H. H. Travers, and examined by Prof. John Agardh, of Lund"; "Notes on the Flora of the Province of Wellington, with a list of plants collected therein," by John Buchanan ; and by Mr. W. T. L. Travers a few notes "On the spread of *Cassine leptophylla*." In Chemistry, Mr. W. Skey talks about the Mineral Oils of New Zealand ; and in Geology are papers "On the Formation of Mountains," by Capt. Hutton ; "On the Extinct Glaciers of the Middle Island of New Zealand," by W. T. L. Travers ; "On the Fossil Reptilia of New Zealand," by Dr. Hector ; besides other interesting papers.

THE Cambridge Natural Science Club has held eight meetings this term on Saturday evenings, and some good papers have been read by the members at the meetings in their rooms, usually followed by a discussion. The attendance has mostly been under the average of other terms, on account of some of the members being candidates in the Natural Science Tripos now being held. The following are some of this term's papers :— "The Reniportal Circulation," by Mr. P. H. Carpenter (Trin. Coll.) ; "Vegetation as affecting Climate," by Mr. J. M. F. H. Stone (St. Peter's Coll.); "Tides," by Mr. Arthur Buxton, B.A. (Trin. Coll.) ; "Comparisons of Nervous Systems of Vertebrata and Invertebrata," by Mr. T. W. Bridge (Trin. Coll.) ; "The Influence of Molecular Structure upon some Organic Bodies," by Mr. E. B. Sargant (Trin. Coll.) ; "The Theory of the Identity of Matter," by Mr. P. R. Ogle (St. Peter's Coll.) ; "The Development of Blood," by Mr. S. H. Vines (Christ's Coll.)

IT is gratifying to see a growing tendency in the not professedly scientific press to endeavour to account for the causes of phenomena which it is called upon to notice ; thus, consciously or unconsciously, treating occurrences in a scientific spirit. For example, *The Country*, in speaking of the migration of birds, states that woodcocks have been unusually scarce in Cornwall for the past two or three years, nor is the present season an exception to the rule, for, notwithstanding favourable winds and moonlight nights, they continue *raræ aves* in the county. In attempting to account for this, *The Country* very pertinently suggests that improved agriculture has more or less destroyed the feeding grounds, though, as the same may be said of other parts of the kingdom where such game is not scarce, this cannot be the only cause.

A TELEGRAM to Cairo, dated the 8th inst., from the Governor-General of the Soudan, announces that the entire kingdom of Darfour has accepted annexation to Egypt.

THE American Society of Paris proposes to hold an "International Congress of Americanists" at Nancy, near Paris, on the 22nd of July, 1875, the object being to bring together those who are interested in the history of America prior to its discovery by Columbus, and in the interpretation of the monuments and of the ethnology of the native races of the New World. An exhibition of American Archæology is to be held at the same time. Any American can be enrolled as a member of the Congress by forwarding the sum of twelve francs to Mr. Lucien Adam, secretary of the American Society of Arts, Rue Bonaparte, in Paris.

A SERIES of experiments has lately been made by the Russian Government with reference to the use of electricity for the headlight of locomotives, a battery of forty-eight elements making everything distinct on the railway track to a distance of over 1,300 ft.

A CURIOUS phenomenon frequently met with in the Indian Ocean, the real cause of which has not yet been ascertained, is the existence off Malabar, and in certain spots along the Coromandel coast, of vast mud banks, and of tracts of mud suspended in the sea, wherein many kinds of fish find abundance of food, immunity from much disturbance in the surrounding element, and a locality in which to breed. The exact cause of the existence of these large tracts of sea wherein mud remains in solution is still a mystery, but at any rate the ocean is so smooth that, even during the height of the south-west monsoon, vessels can run for shelter into their midst, and once there are as safe as when inside a breakwater. If the surface is so still, of course so is the water below, and such spots seem to be well suited to the siluroid fishes. These curious patches of sea which appear in a continually perturbed state, and the sea-bottom in the locality, would probably well repay careful scientific observation.

THE manufacture of isinglass, generally supposed to be confined to Russia and North America, or other countries where the sturgeon is found in abundance, is carried on to a considerable extent in India, principally from the air-vessels of several varieties of acanthopterygian fishes, and particularly, different kinds of perch, as well as from other fish. There is room for a great extension of the trade, as isinglass, the purest known form of animal jelly, has, in a measure, had its consumption checked by its high price, and substitutes are employed, such as gelatine, of which it is itself the purest form.

AT the last meeting of the British Association a committee was appointed to investigate the circulation of the underground water in the New Red Sandstone and Permian Formations of England, and the quantity and character of the water supplied to the various towns and districts from these formations. Prof. Hull, M.A., F.R.S., director of the Geological Survey of Ireland, is chairman, and Mr. C. E. de Rance, F.G.S., Scientific Club, 7, Saville Row, London, W., secretary. The following queries have been circulated by the committee for the purpose of eliciting information in connection with the important subject:—1. *Position* of well, or wells, with which you are acquainted. 2. Approximate *height* of the same above the mean sea level. 3. *Depth* from surface to bottom of shaft of well, with diameter. *Depth* from surface to bottom of bore-hole, with diameter. 4. Height at which water stands *before* and *after* pumping. Number of hours elapsing before ordinary level is restored, after pumping. 5. *Quantity* capable of being pumped in gallons per day. 6. Does the *water level* vary at different seasons of the year, and how? Has it diminished during the last ten years? 7. Is the ordinary *water level* ever affected by local rains, and if so, in how short a time? And how does it stand in regard to the level of the water in the neighbouring streams, or sea? 8. *Analysis* of the water, if any? Does the water possess any marked *peculiarity?* 9. Nature of the rock passed through, including cover of drift, with *thicknesses*. 10. Does the cover of drift over the rock contain *surface springs*. 11. If so, are they entirely kept *out* of the well? 12. Are any large *faults* known to exist close to the well? 13. Were any *salt springs* or brine wells passed through in making the well? 14. Are there any *salt springs* in the neighbourhood? 15. Have any wells or borings been discontinued in your neighbourhood, in consequence of the water being more or less *brackish?* If so, if possible, please give section in reply to query No. 9.

WE have received, among the results of the geographical and geological explorations of the Western (U.S.) States, the annotated list of the birds of Utah, by Mr. H. W. Henshaw, containing the names of 214 species, of which 160 were either taken or noted in the expedition. The author thinks that if collections were, as they have not yet been, made during the spring months, several extra species would have to be added to the collection.

COAL is beginning to attract attention in New South Wales, in some parts of which the mineral is being found in abundance, and the pre-eminence which gold and copper have maintained will be assailed by the increasing importance of the newly worked product. A seam, seven feet thick, has been opened at Broughton Creek, near the Shoalhaven River, and not far from the Moss Vale Railway Station; so that every circumstance of locality is in favour of its profitable working.

THE *American Chemist* for August and September, which we have just received, contains a full account of the proceedings at the Priestley Centenary in Northumberland, Pa., on July 31 last. There was then a large and enthusiastic gathering of men of science and others, and several valuable addresses were given. The principal one in the numbers before us is by Prof. B. Silliman, being a long, minutely detailed, and carefully compiled paper on "American Contributions to Chemistry."

WE are gratified to see that the *Geographical Magazine* has been so successful that the price is to be reduced to one shilling.

THE additions to the Zoological Society's Gardens during the past week include a Chamois (*Rupicapra tragus*) from the Pyrenees, presented by Mr. A. Wilson; a White-fronted Capuchin (*Cebus albifrons*) from South America, presented by Mrs. Carpenter; a common Boa (*Boa constrictor*) from South America, presented by Capt. E. C. Kemp; two Barred-tailed Pheasants (*Phasianus reevesii*) from North China, received in exchange.

ON THE STRUCTURE OF STIGMARIA *

AT a meeting of the Manchester Literary and Philosophical Society, held on October 20, Mr. Binney called in question some conclusions at which I had arrived and had published in Part II. of my memoirs on the Structure of the Coal Plants, respecting the organisation of Stigmaria. Mr. Binney further published an abstract of his remarks in Part II. of vol. xiv. of the Society's Proceedings. Believing that Mr. Binney's observations, if allowed to pass unnoticed, may mislead some palæontologists unacquainted with Stigmaria, I feel called upon to reply to them through the same channel as that which he has employed for their promulgation. The general features of the plant known for half a century as *Stigmaria ficoides* have been so well described by Lindley and Hutton, Dr. Hooker, Mr. Binney, and Brongniart, that no one familiar with those descriptions can fail to recognise it without difficulty. That plant consisted of a central medulla, surrounded by a cylinder of scalariform vessels arranged in radiating wedges, very distinctly separated by two kinds of medullary rays (primary and secondary), the whole being enclosed in a thick bark, from the surface of which spring numerous large cylindrical rootlets. The vascular cylinder gives off numerous large vascular bundles of scalariform vessels, which proceed outwards, through the conspicuous primary medullary rays, to reach the rootlets.

The dispute between Mr. Binney and myself resolves itself chiefly into three points: (1), the structure of the medulla of Stigmaria; (2), the source whence the vascular bundles supplying them are derived; and (3), the nature of some vascular bundles which both Mr. Binney and M. Goeppert have figured as existing within the medulla, and one of which is prolonged radially in M. Goeppert's example through a medullary ray. Mr. Binney and M. Goeppert believe that the cellular medulla of Stigmaria contained bundles of very large scalariform vessels, and that those bundles proceeded outwards to supply the rootlets. On the other hand, in my second memoir, referred to by Mr. Binney, I not only expressed my conviction, but demonstrated the absolute certainty, that such was not their origin. I adhere to the same opinion as I previously expressed, and have the specimens on the table which prove its correctness. The fact that these bundles were derived not from the medulla, but from the vascular wedges of the woody cylinder, was illustrated by the figures 43, 44, and 47 of the memoir referred to, figures which accurately represent, not conditions occasionally met with, but those which characterise every specimen of the true *Stigmaria ficoides.* In the memoir I further affirm that immediately within the woody cylinder there exists a delicate cellular tissue, and state that one of my specimens makes it perfectly clear that the entire medulla consisted of similar cells, unmixed with any vascular bundles whatever such as were represented in M. Goeppert's and Mr. Binney's figures, and the accuracy of which is, was, and it appears still is, endorsed by Mr. Binney. After thus endorsing what I believe to be a grave mistake, Mr. Binney proceeds to justify his doing so by appealing to a specimen which I have not seen, but which Mr. Binney's own description convinces me is a plant altogether different alike from the Stigmaria of authors, and from M.

* A paper read before the Manchester Philosophical Society, by Prof. W. C. Williamson, F.R.S., Nov. 17.

Goeppert's and Mr. Binney's own figures. Mr. Binney's describes his new specimen as having a radiating woody cylinder, immediately within which is a second series of large vessels not arranged in radiating wedges, and which Mr. Binney says is "something like a medullary sheath, enclosing a medulla composed of very small and short barred tubes or utricles, in which are mingled large vascular tubes or utricles." Though this use of vague terms renders the sense obscure, I presume that Mr. Binney simply means that in the medulla of his plant a *vascular* cylinder encloses a *cellular* medulla, or, in other words, that his specimen has a Diploxyloid axis. That Mr. Binney possesses a specimen having the above structure, and giving off rootlets from its periphery, I have no reason for doubting, since in the memoirs already quoted I have described a similar structure under the name of *Diploxylon stigmarioideum*, and respecting which I make the following observations:—"It is possible that the plant may, like Stigmaria, prove to be the uppermost part of a root of some of the other forms" (*i.e.* of Lepidodendroid stems), "though I have never yet found it associated with any rootlets, and it may be a fragment from the base where stem and roots united" (*loc. cit.* p. 239). I arrived at the above conclusions because I found in the specimen described, evidence that large rootlet bundles were given off from the woody zone as in the true Stigmaria. But I affirm that out of hundreds of Stigmarian fragments that I have examined, I have only found two possessing this structure, and I unhesitatingly express my conviction that Mr. Binney's specimen is another example of an equally rare type, both being entirely distinct from *Stigmaria ficoides*, to which latter plant alone is referable Mr. Binney's previously published figures, M. Goeppert's description and figures of which Mr. Binney approves, and mine which he rejects.

Mr. Binney proceeds to say: "The size of these large vascular tubes or utricles in the medulla exceeding anything so far as his knowledge extended, hitherto observed in fossil plants, shows that it was easily decomposed, and thus accounts for the general absence of the medulla in Sigillaria and its roots." To this reasoning I must altogether demur. Size has nothing whatever to do with the preservation of the tissues in fossil plants. Vascular structures strengthened by transverse bars of lignine are equally well preserved, whether they are large or small. The medulla of Stigmaria disappeared or became much disorganised because it consisted of an unusually delicate cellular tissue with extremely thin walls. This tendency to decay was more manifest towards the centre of the medulla than at its circumference. Specimens on the table exhibit this peripheral part of the cellular medulla in exquisite perfection, giving off its characteristic cellular prolongations constituting the medullary rays, as described in my memoir. And yet this beautiful cellular tissue occupies the position which Mr. Binney says was occupied by "large vascular tubes or utricles." The specimens referred to showing these conditions constitute unanswerable facts.

Mr. Binney correctly notes the resemblance of the inner vascular cylinder in his specimen to his "medullary sheath." I have already said the same thing in several of my memoirs, and M. Brongniart said it before either of us. But this very homology, if correct, indicates the probability of Mr. Binney's specimen being a fragment derived from the junction of stem and root rather than a true root, since in living plants possessing a medullary sheath, that sheath, as every botanist knows, is never prolonged into the true roots, for the simple physiological reason that its origin is directly connected with that of the leaf formations of the ascending axis.

As I have already observed, M. Goeppert's and Mr. Binney's previous figures represent a structure altogether different from that now described by Mr. Binney. Instead of the continuous inner vascular cylinder of the latter, M. Goeppert's figure displays two detached, unsymmetrically arranged, vascular bundles in the interior of the medullary cavity. I have already affirmed my conviction that these belong to intruded rootlets of a Stigmaria, and are in no respects part of the true medullary axis. On the other hand, Mr. Binney says that "they are certainly not intruded rootlets, as anyone who examines the learned author's plates can satisfy himself." On this point Mr. Carruthers writes to me on Nov. 2: "No one who is accustomed to sections of Stigmaria can fail to see that Goeppert has mistaken the accidental rootlets of Stigmaria penetrating the decayed axis for an organic part of that axis." I may allow this opinion of an experienced botanist, with which I wholly concur, to neutralise that of Mr. Binney, who further says: "It is very improbable that they" (*i.e.*, Goeppert's vascular rootlets) "had ever been introduced into the axis after the pith had been removed." To this I reply that it is an extremely rare thing to find any such axis which does not contain more or less of these rootlets. My cabinet is full of such examples, and in two specimens on the table, one of which has been lent me by Capt. J. Aitken, of Bacup, similar rootlets not only exist in the central axis, but have penetrated the medullary rays as in M. Goeppert's specimen.

Mr. Binney, referring to my comments upon his previous memoir, says that in "that memoir mention is only made of the large vascular bundles found in the axis, without calling them vascular or any other vessels." I do not very clearly understand what this sentence means, but I presume it is intended to imply that Mr. Binney never affirmed that the pith of Stigmaria contained vascular tissues, and that I have misrepresented him in stating that he had done so. I can only answer this by giving Mr. Binney's words:—"The most important circumstance thus developed is the existence of a double system of vessels in Stigmaria, first shown by Goeppert, and the consequent approach in this respect to *Diploxylon*, Corda. In Diploxylon, however, the inner system forms a continuous cylinder, concentric with and in juxtaposition to the wedges of wood forming the outer; while in Stigmaria the same inner system is broken up into scattered bundles, apparently unsymmetrically arranged in the medullary axis or pith of the plant" (*Quarterly Journal of the Geological Society*, vol. xv. p. 17); and on p. 78 of the same memoir, describing the specimen represented by Fig. 2, he says, "The axis is filled with eleven or twelve large vessels of circular or oval form," and the same structures are again spoken of as "vessels" no less than six times in the next seventeen lines, with the further remark that "altogether these angular vessels remind me somewhat of the vascular tissue in the middle of Anabathra" (loc. cit., p. 78). It is true that in two places Mr. Binney applies to these structures the term "utricles," by which, I presume, he means cells; but such a term, applied to such tissues, is equally applicable to all known fibro-vascular structures, and is simply equivalent to saying that scalariform vessels have no existence.

I have entered into these details because by promulgating vague and groundless doubts respecting work already carefully done, Mr. Binney's communication tends to re-introduce confusion into questions that have been virtually settled. It does this through failing to discriminate between things that differ. His introductory remarks refer to the common *Stigmaria ficoides*, whilst his justification of those remarks rests upon a plant of a very different character, and which I am absolutely certain is not the common form of Stigmaria.

VEGETATION OF THE LIBYAN DESERT

IN Dr. Ascherson's report on the vegetation of the Libyan Desert, published in the *Botanische Zeitung*, there are some interesting notes on the fall and renewal of the leaves of deciduous trees. In our climate we have little difficulty in understanding the distinction between evergreen and deciduous trees and shrubs, because the greater part of those that change their leaves cast the old ones in autumn or early winter; and evergreens with flat leaves have them more or less coriaceous. But even with us there is a gradual transition from evergreen to deciduous through *Eunonymus europæus* and *Ligustrum vulgare*, both of which have strictly evergreen congeners in *Eunonymus japonicus* and *Ligustrum japonicum*. Some few years ago Hoffmann started a theory that sempervirence could be artificially produced, and there is no doubt that climate influences to a great extent the length of the period during which really deciduous species hold their foliage; but it appears far more probable that these are physiological peculiarities not altogether dependent upon climate, as we find evergreen and deciduous species growing in the same regions and under precisely similar conditions. Some evergreens do not change their leaves at all, and even retain them for many years or all their lifetime; *Araucaria imbricata*, for example. *Taxodium distichum*, one of the few deciduous Coniferæ, offers a very curious phenomenon, inasmuch as the ultimate branchlets are deciduous. The observations chronicled by Dr. Ascherson agree almost entirely with our own experience. On his outward journey he traversed 25° of lat. in less than a month, which gave him an excellent opportunity for studying the conditions of the same species under very diverse climates. Thus, in the plains of Lombardy many deciduous trees, and especially *Morus alba*, were still partially covered with foliage on the 19th of November, the same species having long previously shed their leaves in Germany. In a similar manner, the fig-trees in Lower Egypt (31° N. lat.) were partially clothed with foliage at the beginning of December, and in Upper Egypt (27° N.) were still in full leaf, whilst already, on the 24th of November, they were quite bare in the Apulian plain (41° N.). On the 11th of December, the pomegranate trees in the gardens of Siout were in yellow leaf, and on New Year's Day, 1874, the apricot trees at Farafreh were still in their prime of green leaf. Hence, one might readily imagine that on approaching nearer the equator these same species would exhibit no interval between the fall and the renewal of the foliage, and thus, to all intents and purposes, become evergreen. But this phenomenon was only verified in the case of the little cultivated peach trees of the oases, in which it may not be constant. Moreover, the peach tree shows the same tendency in mild seasons with us. In the oases, at the beginning of March, when the trees began to blossom and make new growth, the old leaves were still fresh and capable of assimilation. All other deciduous trees and shrubs cultivated in the gardens of Kasr Dghakel (25° 45′ N. lat.), including the grapevine, apricot, apple, pomegranate, plum, fig, mulberry, and willow (*Salix safsaf*), had lost their foliage on the arrival of Dr. Ascherson, or became leafless before the end of January. It should be mentioned that the fall of the leaf in this region does not proceed with the same regularity as at home, for it is not unusual to see quite naked and fully clothed trees of the same species standing side by side. Again, the presence of abundance of moisture has the effect of enabling the trees to carry their old foliage longer and put forth their new earlier than trees growing in drier situations. And some of the willows growing by water were quite evergreen; that is, after the manner of the peach trees mentioned above. But the apricot, one of the most abundant trees, rarely retained even a few scattered old leaves on the appearance of the flowers. The same was observed of the grapevine, fig, and mulberry. By Feb. 20 the apricot trees were in full blossom, and by March 10 in full foliage, so that there was only an interval of four or five weeks between the fall of the old foliage and complete development of the new. The apple and plum behaved in a similar manner, the pomegranate was a little later, the fig next in order, and finally the mulberry; whilst these same things, in the reverse sense, lost their leaves first. From the preceding notes it seems that the fall and renewal of the leaf is an essential constitutional peculiarity, which is modified by climatal conditions, but not entirely subject to them. A more striking illustration of this fact may be found in exotic deciduous trees planted in Egypt. Dr. Ascherson noted more particularly the summer fall of the leaves of *Poinsettia pulcherrima*, a South American shrub, and *Albizzia lebbek*, a native of the East Indies. The former is in the full splendour of its inflorescence in December, and quite leafless in April, remaining so, it is said, until the autumn. The Albizzia is extensively planted as an avenue tree. It sheds its foliage in April, but soon renews it. Both of these plants lose their leaves in their native countries during the dry, and renew them with the opening of the rainy season.

SCIENTIFIC SERIALS

Journal de Physique, III., No. 34, Oct. 1874.—This number commences with the first portion of a paper by M. J. Bertrand, entitled "Demonstration of Theorems relating to Electrodynamic Actions." The object of this paper is to simplify Ampère's demonstrations of the theorems of electrodynamics.—Arrangement for obtaining projections of the metallic rays and their reversal, by M. Boudréaux. Instead of the electric light or oxyhydrogen flame, the author employs a mixture of the chlorate of the metal with one-sixth of its weight of powdered gum-lac. The mixture is inflamed in a carbon crucible placed in a lantern provided with a vertical slit. Reversals of the metallic lines are effected by allowing a beam of white light (Drummond or sunlight) to pass through the deflagrating mixture and analysing the resulting rays by prisms. By allowing the sun-light to fall only on one-half of the slit, the coincidence of bright with dark lines can be shown.—M. Mascart contributes a paper describing two pieces of apparatus for obtaining the phenomena of interference.—On the magnetisation of steel, by M. E. Bouty.—This number contains a translation of Lord Rayleigh's paper on the manufacture and theory of diffraction gratings from the *Philosophical Magazine* for February and March.—Sensibility of silver bromide to rays supposed to be chemically inactive, by H. Vogel, from *Poggendorff's Annalen*.—From the same journal there is a paper by H. Streintz, on changes in the length and elasticity of a wire under the influence of an electric current.—From the Proc. Roy. Soc. there are translations of Prof. Tyndall's paper on the transmission of sound, and Mr. Norman Lockyer's note on a new class of absorption phenomena.

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie. Nov. 15.—In this number Dr. Hann treats of some of the consequences of the laws of change of temperature in air undergoing change of volume. The following are some of the results of his argument, which is full of interest. The rate of cooling of ascending air varies so much with the conditions of time and place that it cannot be expressed by any general law. But both in Germany and in the tropics the mean rate lies between 0°·5 and 0°·6 C. for every 100 metres. Air warmed at the surface of the earth does not continue to rise until it reaches a level where the temperature corresponds with its own (reduced), but becomes thoroughly mixed with other strata before reaching that height. Temperature falls more rapidly with increase of height in bad than in fine weather. In a descending current there can be no condensation of moisture, and so in it the theoretical increment of 1° C. does take place. We would expect this current to clear the sky. But in fact we find that a descending current often brings rain as well as warmer weather. Our moist west winds do not bring their moisture from the tropics, but the Anti-trade, becoming warmer as it descends, collects a fresh quantity of vapour and precipitates it again when cooled by radiation or ascent of mountain slopes. The formation of hail and phenomena of hailstorms are best understood by supposing, with Reye, the lower hot moist strata to rise rapidly to a great height, not the upper air to descend, as it has been shown that this would become much warmer in descending. A cold wind blows first in the higher parts of the atmosphere, and the overheated air below rushes upward with unusual energy to a height where precipitated moisture freezes as it falls. This ascending movement of warm air, and the further impulse given to it by liberation of the latent heat of vapour, appear to play a large part in the production and continuance of falls of rain. Dr. Hann holds the barometric minimum in the middle of a storm area to be a mechanical effect of the whirling movement of the air, and the moving force in cyclones to be the latent heat of vapour.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 3.—"On the Coefficient of Expansion of a Paraffine of high boiling-point," by G. F. Rodwell, F.R.A.S., F.C.S., Science Master in Marlborough College. Communicated by Prof. Stokes, Sec. R. S.

The author, after giving an account of his researches, concludes that paraffine is a body which undergoes a most unusual expansion in passing from its ordinary solid condition to the high boiling-point which it possesses. He does not remember any other substance of a high boiling-point which occupies at the boiling-point a volume which is one-half as large again as the volume at the ordinary temperature. In an accompanying table he has introduced, side by side with the paraffine curve, the expansion curves of mercury, iodide of silver, and terbromide of phosphorus, one of the most expansible liquids known, if we except such bodies as ether, bromide of ethyl, acetate of methyl, &c., the boiling-point of which is below 100° C., and which, therefore, could not be easily introduced into the table for comparison with a body which boils at nearly 400° C.

Dec. 10.—"On the effect of Heat on the Iodide of Silver," by G. F. Rodwell, F.C.S. Communicated by Prof. F. Guthrie, F.R.S.

The author endeavours to prove the following main facts:—

1. That the iodide of silver exists in three allotropic forms, viz. (α) at temperatures between 116° C. and its fusing-point, as a plastic, tenacious, amorphous substance, possessing a reddish colour, and transparent to light; (β) at temperatures below 116° C. as a brittle, opaque, greenish-grey, crystalline mass; and (γ) if fused and poured into cold water, as an amorphous, very brittle, yellow, opaque substance.

2. That the iodide possesses a point of maximum density at or about 116° C. at the moment before passing from the amorphous into the crystalline condition.

3. That if we allow a mass of molten iodide to cool, the fol-

lowing effects may be observed :—(α) at the moment of solidification a very considerable contraction takes place ; (β) the solid, on further cooling, undergoes slight and regular contraction after the manner of solid bodies in general, until (γ) at or about 116° C. it undergoes sudden and violent expansion, passing from the amorphous into the crystalline condition ; (δ) after undergoing this expansion the mass on further cooling undergoes slight expansion, and (ε) the coefficient of contraction diminishes as the temperature decreases (or, otherwise expressed, the coefficient of contraction augments with the temperature).

"On the Multiplication of Definite Integrals," by W. H. L. Russell, F.R.S.

Geological Society, Dec. 2.—Mr. John Evans, F.R.S., president, in the chair.—The following communications were read :—On the femur of *Cryptosaurus eumerus*, Seeley, a Dinosaur from the Oxford clay of Great Gransden, by Mr. Harry Govier Seeley, F.L.S., Professor of Physical Geography in the Bedford College, London. The author described this femur as showing a slight forward bend in the lower third of the shaft, and as having the terminal portions wider in proportion to the length of the bone than in any described Dinosaurian genus. He pointed out its differences from the corresponding bone in Megalosaurus, Iguanodon, and other genera. The length of the femur was stated to be about one foot.—On the succession of the ancient rocks in the vicinity of St. David's, Pembrokeshire, with special reference to those of the Arenig and Llandeilo groups and their fossil contents, by Mr. Henry Hicks. In the first part of this paper the author described the general succession of the rocks in the neighbourhood of St. David's from the base of the Cambrian to the top of the Tremadoc group, and showed that they there form an unbroken series. The only break or unconformity recognised is at the base of the Cambrian series, where rocks of that age rest on the edges of beds belonging to a pre-Cambrian ridge. In the second part the author gave a minute description of the rocks, comparing the Arenig and Llandeilo groups, as seen in Pembrokeshire, with each other and also with those known in other Welsh areas. Each group he divided into three subgroups, chiefly by the fossil zones found in them. 1. The *Lower Arenig* was stated to consist of a series of black slates about 1,000 feet thick, and to be characterised chiefly by a great abundance of dendroid graptolites. 2. *Middle Arenig.*—A series of flags and slates, about 1,500 feet thick, and with the following fossils :—*Ogygia scutatrix*, *O. peltata*, *Ampyx Salteri*, &c. 3. *Upper Arenig.*—A series of slates, about 1,500 feet in thickness, only recently worked out, and found to contain a large number of new and very interesting fossils, belonging to the following genera, viz. : *Illænus*, *Illænopsis*, *Placoparia*, *Barrandia*, &c. 4. *Lower Llandeilo.*—A series of slates and interbedded ash, equivalent to the lowest beds in the Llandeilo and Builth districts, and containing species of Æglina, Ogygia, Trinucleus, and the well-known graptolites *Didymograptus Murchisoni* and *Diplograptus foliaceus*, &c. 5. *Middle Llandeilo.*—Calcareous slates and flags with the fossils *Asaphus tyrannus*, *Trinucleus Lloydii*, *Calymene cambrensis*, &c. 6. *Upper Llandeilo.*—Black slates and flags, with the fossils *Ogygia Buckii*, *Trinucleus fimbriatus*, &c. The Arenig series was first recognised in North Wales by Prof. Sedgwick about the year 1843, and was then discussed by him in papers presented to the Society. The Llandeilo series was discovered by Sir R. Murchison previously in the Llandeilo district, but its position in the succession was not made out until about 1844. The Geological Survey have invariably included the Arenig in the Llandeilo group ; but it was now shown that this occurred entirely from a mistaken idea as to the relative position of the two series, which were shown to be entirely distinct groups, the equivalents of both groups being present in Carnarvonshire, Shropshire, and Pembrokeshire, but the Llandeilo group only of the two being developed in Carmarthenshire. The lines of division in the series were said to be strongest at the top of the Menevian group and at the top of the Tremadoc group, these lines being palæontological breaks only, and not the result of unconformities in the strata.

Anthropological Institute, Dec. 8.—Mr. J. E. Price, F.S.A., in the chair.—Mr. M. J. Walhouse read a paper on the existence of a leaf-wearing tribe on the western coast of India. The author's residence at Mangalore for some years afforded him the opportunity of studying the habits of the native tribes of South Canara, and in the present communication he recorded a few facts concerning the Karagars, a remnant, now numbering only a few hundreds, of the aboriginal slave castes whose distinctive peculiarity was the habit of wearing aprons of woven twigs and green leaves over the usual garment. The custom at present is observed by the women only, who think that abandoning it will bring them ill luck. The author maintained that the leaf was a badge of degradation, and was a survival of a very ancient custom. The unswerving truthfulness of the Karagars is proverbial, and should be remarked as affording a complete refutation of Mr. Mill's assertion, that savages are invariably liars. The paper contained many interesting facts concerning the physical characteristics, traditions, religious rites, and habits of the tribe.—A paper by Mr. Rooke Pennington was read, on some tumuli and stone circles near Castleton, Derbyshire. It comprised a full account of the exploration of the barrow of Elden Hill, measuring 49 feet in diameter, which yielded bones of man, horse, and rat in great abundance, and a red deer's antler that had been worked. A few feet deeper was discovered a grave, containing the skeleton of a young person that had been buried in a contracted position ; no implements accompanied it, but it appeared to have been interred with much barbaric pomp. On the top of Siggett Hill was another barrow of somewhat less dimensions, in which was found a fine skeleton with an inverted urn, of the usual type, containing burnt bones. Evidence was adduced to prove that the corpse was not burnt until after the funeral feast was concluded, and the bones of the animals eaten were cast at the same time and into the same fire with the human body. This was one of those barrows which had led the author to conclude that in Derbyshire, at any rate, no connection can be established between the Neolithic age and contracted burial, and the bronze age and incremation.—Major Godwin-Austen contributed some further notes on the stone monuments of the Khasi Hills.

Mathematical Society, Dec. 10.—Prof. H. J. S. Smith, F.R.S., president, in the chair.—Prof. Cayley gave an account of his paper on the potentials of polygons and polyhedra.—Two notes from M. Mannheim to J. J. Sylvester, F.R.S., were, in the latter gentleman's absence, communicated by Mr. Tucker. The first note contained an elegant geometrical demonstration of the following propositions. $ABCD$ is a quadrilateral whose sides are of invariable magnitude, such that $ab = ad$ and $bc = cd$. The points a and b are fixed ; if m be a point rigidly connected with b, it will trace out, as the quadrilateral changes its form, the pedal of a conic. The problem has been treated in an analytical form by Prof. Cayley in a communication to the Society on the determination of the position of the node of a quartic curve, mentioned also in a paper read before the same Society by Mr. Samuel Roberts as a case of three-bar motion. This Mr. Cayley observed to be the inverse of a conic. Mr. Sylvester calls attention to M. Mannheim's proof as a very beautiful and purely geometrical one.—The second note is connected with a geometrical proof of a proposition thus stated by M. Mannheim :—"Lorsque le quadrilatère a, g, m, d, dont les cotés sont inégaux, mais dont les diagonales sont perpendiculaires entre elles, se déforment de façon que le sommet a décrive une circonférence (o) et les sommets g, d, des circonférences ayant même centre s sur la diagonale ma, le sommet libre m décrira une anallagmatique du 4e ordre." This note relates to seven-bar motion, and is Peaucellier's motion, generalised by substituting for his rhomb any quadrilateral in which the two diagonals are at right angles, one case of which is the kite or spear-head form.

Royal Geographical Society, Dec. 14.—Major-General Sir H. Rawlinson, K.C.B., presided. The principal business was the reading of the report of the Livingstone Congo Expedition, by Lieut. W. T. Grandy, R.N. The President opened the proceedings by announcing the receipt of an interesting letter at the Foreign Office from Lieut. Cameron, the substance of which we have already given. Lieut. Cameron expressed his confident opinion, founded on Arab information, that the Lualaba river was in reality the Congo. Mr. Markham then read Lieut. Grandy's report, which was an itinerary of the writer's journey to the interior up to the date of his recall. Roads were being made by means of which the troops would be able to intercept the transit of slaves to the coast, and encouragement was being given to cultivate the india-rubber tree, of the value of which in Europe the natives had been hitherto ignorant. The chiefs had been exceedingly kind and hospitable to Lieut. Grandy and his party, and had promised facilities for future expeditions. He had found several traces of the Portuguese occupation of Congo, and he described the natives as being civilised, indolent, and exceedingly fond of snuff and tobacco. The palm-tree grew abundantly, but the principal use made of it by the natives was the distillation of the oil into a very intoxicating wine, in the use of which the king and chiefs indulged rather freely. Lieut. Grandy had shown his Majesty specimens of stearine, and he promised to commence its manufacture, instead of wine, from his palm oil. A terrible epidemic of small-pox was decimating Congo as Lieut. Grandy travelled through the country, and it almost entirely carried off his native porters and escort. In conclusion, the report described the Congo as being one of the grandest rivers in the world, and as being navigable for a distance of 110 miles from its mouth.

CAMBRIDGE

Philosophical Society, Nov. 30.—Prof. Humphry in the chair.—A paper on "Lopsided Generations," or "Right-handedness," by Dr. W. Ainslie Hollis, was read, which, judging from the abstract before us, contained little or no new matter. The author laid special stress on the statement that the left side of the brain in man is the larger, and that aphasia is connected with disease of that side; statements which, in the discussion which followed, Prof. Paget justly remarked were not yet in any way proved. The cases of Johnson and Swift were quoted as instances in which the left side of the brain had suffered, and paralysis of the right side had been induced, apparently as a consequence of overwork. Prof. Humphry and Mr. Carver both agreed that right-handedness was much a matter of education.—Dr. Wilson made a communication on the disposition of the peritoneum in man and other vertebrata, directing special attention to the peculiarities of the omental sac, which he showed to be frequently divided into two parts—a gastro-hepatic and a gastro-colic—by a constriction corresponding with the upper border of the stomach. One or more of the hepatic lobes usually project into the gastro-hepatic portion of the sac. In man, and we should therefore expect in others, it is the lobulus spigelii.

DUBLIN

Royal Irish Academy, Nov. 9.—William Stokes, F.R.S., president, in the chair.—Samuel Ferguson, V.P., read a paper on the Ogham-inscribed stone at Montaggart, Co. Cork. The author, in applying to this text, which had been considered undecipherable, the same method of translation adopted by the present Bishop of Limerick in the case of the Camp inscription, read it "Feqreq Moqoi Glunlegget," identifying Feqreq with the name Feachra, as written by Adamnan, and assigning the meaning of "the Kneeler" to Glenlugget, which he took to be a name in religion; and expressed his belief that the monument is Christian.—A letter was read by the Secretary from Mr. R. R. Brash, commenting on Dr. Ferguson's paper.—Mr. H. W. Mackintosh read a paper on the muscular anatomy of *Cholœpus didactylus*.—Alexander Macalister, M.D., read a paper on two new species of Pentastoma. The first of these, *P. imperatoris*, was found in the lung and peritoneal cavity of *Boa imperator*, from South America; the second, *P. aonycis*, in the peritoneal cavity of *Aonyx leptonyx*, var. *B. major*, from the River Indus.—Alexander Macalister, M.D., also read a paper on the presence of a lachrymo-jugal suture in a human skull. Although the relation of the maxillary process of the jugal bone to the supra-orbital edge of the maxilla is subject to a considerable amount of variation, yet in the majority of cases this process ends at a point vertically over the infra-orbital foramen. But in one skull in the collection of Trinity College, Dublin, of which the history is unknown, the author found the maxillary process to stretch over the whole infra-orbital edge of the maxilla, in front of the large external hamulus of the lachrymal bone, with which it forms a suture of about a line and a half in length. The author further gives a sketch of the comparative anatomy of this suture.

PARIS

Geographical Society, Dec. 2.—President, M. Delesse.—Dr. Cosson declared himself decidedly against the scheme of forming a sea in the interior of Africa, on the site of the Tunisian "Chotts." He believes that not only would the climate of the Sahara be modified, but the great source of wealth of these regions—the culture of dates—would be completely destroyed. Moreover, the commercial results would never repay the enormous cost, estimated at about 12,000,000*l*. for Tunis, and a like sum for France. All the existing legitimate commerce is sufficiently carried on by means of caravans. It was, however, suggested that it would be wise to suspend judgment on the subject until the return of the French expedition, which is making preliminary investigations on the spot.—Dr. Hamy announced the discovery of new mines of gold in Australia, and of very ancient caverns which are expected to yield valuable results to geologists.—Dr. Harmand, one of the companions of the unfortunate Lt. Garnier, gave many interesting details concerning Tonquin.

Academy of Sciences, Dec. 7.—M. Frémy in the chair.—The following papers were read:—Memoir on the actions produced by the simultaneous concurrence of the currents from a battery and electro-capillary currents, by M. Becquerel.—Memoir on the intervention of physico-chemical forces in the phenomena of life, by M. Becquerel.—On the carpellary theory according to the Liliaceæ (*Yucca*), by M. A. Trécul.—On the swim-bladder from the point of view of station and locomotion, by M. A. Moreau.—Note on magnetism, by M. J. M. Gaugain.—On trials at acclimatising the "Jesuit's-bark" tree in the Isle of Réunion, by M. Vinson.—On the ureides of pyruvic acid; synthesis of a homologue of allantoïn, by M. Grimaux. In the present communication the author has examined the derivative obtained by the action of urea upon pyruvic acid. This body, named by its discoverer *pyvurile*, is a white crystalline substance of the formula $C_5H_8N_4O_3$. Treated with hydrochloric acid, it is converted into mono-pyruvic ureide, $CON_2H_2(C_3H_3O)$.—Application of illuminating gas to the pyrophone, by M. F. Kastner. The author's experiments show that if two or several isolated gas flames of convenient size are introduced into a tube of glass or other material at a distance of one-third of the length of the tube, reckoned from the lower extremity, these flames vibrate in unison, this phenomenon continuing as long as the flames remain separated, but ceasing immediately on their being brought into contact. The author likewise verifies the formation of ozone in the tubes.—Observations concerning species of the genus Phylloxera, by M. Signoret. The author publishes the following rectified synonymy:—*P. corticalis*, Kaltenb. = *Lichtensteinii*, Balbiani = *Rileyii*, Lich. MSS. Riley.—Method followed in searching for the most efficacious substance to oppose to Phylloxera at the viticultural station of Cognac, by M. Max Cornu.—Despatch from M. Stéphan, director of the Observatory of Marseilles. This refers to the discovery on the 6—7 December of a new comet by M. Borrelly.—Occultation of Venus, eclipse of the sun and of the moon, observed during the month of October at Paris, by M. C. Flammarion.—Solution of the equation of the third degree by means of a jointed system, by M. Saint-Loup.—On two simple laws of the active resistance of solids, by M. J. Boussinesq.—Determination of the analytical relations which exist between the elements of curvature of the two *nappes* of the evolute of a surface, by M. A. Mannheim.—On the solutions of chrome alum, by M. D. Gernez.—On the transformations of persulphocyanogen, by M. J. Ponomareff. Phosphoric chloride appears from the author's researches to have the following action:—

$$C_3N_3S_3H + 3PCl_5 = 2PCl_3 + S_2Cl_2 + PSCl_3 + HCl + C_3N_3Cl_3$$

The action of ammonia has also been studied.—On the transport and inoculation of virus, carbuncular and others, by flies, by M. J. P. Mégnin. The author considers it demonstrated that certain flies, such as Stomoxus, &c., can be agents of transmission of certain virulent maladies—amongst others carbuncle.—During the meeting the perpetual secretary announced to the Academy that they had sustained a heavy loss in the person of Count Jaubert.

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