THURSDAY, FEBRUARY 22, 1872

THE ROCK THERMOMETERS AT THE ROYAL OBSERVATORY, EDINBURGH

THE whole of the observations made with these instruments (reading to hundredths of a degree Fahrenheit) from 1837 to 1869 having been reduced on a uniform plan, and found to exhibit some well-marked supra-annual cycles, a paper on the subject and on their relations to the sun-spot cycles of similar period but diverse shape was sent in to the Royal Society, London, on March 2, 1870.

Since then two eminent astronomers, one of them being Mr. Stone, the newly appointed Astronomer Royal at the Cape of Good Hope, and the other Mr. Cleveland Abbe, Director of the Cincinnati Observatory, have published somewhat similar deductions touching atmospheric temperatures in reference to sun-spots; Mr. Stone basing on thirty years of South African temperature observed by his indefatigable predecessor Sir T. Maclear; and Mr. Abbe on sixty years' temperature observed on the elevated station of Hohenpeissenberg near Munich, under the superintendence of Dr. Lamont, the Bavarian Astronomer Royal; both parties, equally with myself, using the same famous series of observations of sun-spots, as made by M. Schwabe, and discussed both by Prof. Wolf and Prof. Balfour Stewart. More recently still a Canadian writer, employing the returns of the Toronto Observatory for many years past, considers that he has established a connection between the amount of annual rainfall there and the sun-spots; and of these again with the periods and dates of several interlacing streams of circum-solar meteors. And within the last few days the Radcliffe Astronomer announces in his report for 1871 that the *mean* azimuthal direction of the wind at Oxford, rigorously computed from automatic records during the last eight years, varies year by year through a range of 58° on the whole, between *maximum* and *minimum* of visible sun-spots; the tendency of the wind to a westward direction increasing with the number of spots, and with such west wind, it is to be presumed, the amount of rain also.

These results touch closely on the hopes of physicists to render meteorology more of an exact science by getting at its cosmical relations, but they also touch equally close on another point where the highest science is at present completely dumb, although too it is the very point where the utmost amount of benefit might be conferred on the largest numbers of the people, viz., some approximate indications of the character of the seasons for a year or two beforehand; or indeed, very much as I did make a first attempt, for the two winters of 1870-71 and 1871-72, in the paper presented to the Royal Society in the spring of 1870.

How intimately the well-being of the poor generally, as well as of the agricultural classes, depends on those characteristics of weather which no scientific society can at present foretell, and no Ministry prevent in their destructive effects to the national revenue when they do come, the following letter may serve as a better example than anything that I could prepare on theory alone :—

"Webb's Green, Hales Owen, June 12, 1871

"To C. Piazzi Smyth, Esq., Edinburgh

(Copy)

"Sir,—I am a reader of *Chambers' Journal* and a farmer of some 600 acres. In the publication of Messrs. Chambers I read that you had expressed an opinion from certain observations you had made that the late winter would be very severe. For the general run of weather prophets I have very little respect; but every respect for opinions that are the result of scientific induction.

"Consequently I conducted my farming operations with due regard to your prognostication, and as the result has been a profit to me, I write to thank you. Gratitude has been defined as 'a lively sense of favours to come,' and from that view and in consideration of the present weather if you could give me your opinion of the weather that you think likely to prevail for some time to come I should feel much obliged.

"I have not troubled you with this epistle entirely from a selfish point of view, for besides being a farmer I am unfortunately an employer of a very underpaid class of workmen, hand rail makers.

"Now that stocks of wheat are exhausted, meat is a luxury to which railers cannot aspire; and if the season continues ungenial, before the harvest of 1872 there may be absolute scarcity of bread. I want to get up a fund for emigration, but if you could give me any information as to the probabilities of season that would dispel my gloomy anticipations for next winter, I should rejoice.—I am, &c., &c. (Signed) "THOMAS BISSELL"

But I have so little desire to incur responsibility for any weather predictions that I have gladly availed myself of the opportunity of the publication of the 13th volume of the Edinburgh Astronomical Observations to lay before the public by means of the several Plates 11 to 15 inclusive a complete graphical representation of the whole series of Edinburgh rock-thermometer observations, and on which I will merely venture the following explanatory remarks :—

1. The most striking and positive feature of the whole series of observations is the great heat-wave which occurs every eleven years and a fraction, and nearly coincidently with the beginning of the *increase* of each sun-spot cycle of the same eleven-year duration. The last observed occurrences of such heat-wave, which is very short lived and of a totally different *shape* from the sun-spot curve, were in 1834·8, 1846·4, 1857·8, and 1868·8, whence, allowing for the greater uncertainty in the earlier observation, we may expect the next occurrence of the phenomenon in or about 1880·0.

2. The next largest feature is the extreme cold close on either side of the great heat-wave; this phenomenon is not quite so certain as the heat-wave, partly on account of the excessive depth and duration of the particular cold wave which followed the hot season of 1834·8. That exceedingly cold period, lasting as it did through the several successive years 1836, 37, and 38, was, however, apparently a rare consequence of an eleven year minimum occurring simultaneously with the minimum of a much longer cycle of some forty or more years, and which has not returned within itself since our observations began. Depending therefore chiefly on our later observed eleven-year periods, or from 1846·4 to 1857·8, and from the latter up to 1868·8, we may perhaps be justified in concluding that the minimum temperature of the present cold wave was reached in 1871·1, and that the next similar cold wave will occur in 1878·8.

3. Between the dates of these two cold waves there are located, according to all the cycles observed, even including that earlier one otherwise exceptional, three moderate and nearly equidistant heat-waves, with their two intervening and very moderate cold waves, but their characters are quite unimportant as compared with what is alluded to under heads 1 and 2; and with regard to all the waves, it may be just to state that there has been in observation more uniformity, and will be therefore in prediction more certainty for their dates than for their intensities. C. PIAZZI SMYTH

February 1872

DARWIN'S ORIGIN OF SPECIES

The Origin of Species by means of Natural Selection; or the Preservation of Favoured Races in the Struggle for Life. By Charles Darwin, M.A., F.R.S. Sixth edition, with additions and corrections. (London: J. Murray, 1872.)

FEW are the writers, scientific or otherwise, who can afford, in every successive edition of their works, to place side by side the passages which they have seen reason to alter, from a change of view or any other cause. And yet to this point we find especial attention called in each succeeding edition of Mr. Darwin's "Origin of Species." And herein lies the true humility of the man of science. Science is often charged with being arrogant. But the true student of Nature cannot be otherwise than humble-minded. That man is unworthy of the name of a man of science who, whatever may be his special branch of study, has not materially altered his views on some important points within the last twelve years.* The means at our command for obtaining correct views of the laws which govern Nature are ever increasing, and if we only

Let knowledge grow from more to more,

this can but cause that

More of reverence in us dwell,

reverence for the eternal constancy of Nature's laws, with respect to which we even yet know so little. But a false pride more often tempts men to conceal than to avow their change of opinion. Mr. Darwin carries the contrary practice perhaps to an excess. But such a course necessarily disarms criticism of its sting; and if the learner sometimes ventures to point out wherein he differs from the master's conclusions, it is only in the hope that the interchange of opinion may lead to a removal of the difficulties which prevent a complete accord of thought.

The sixth edition of the "Origin of Species" is considerably smaller than its predecessors; but this does not arise from any diminution of matter, but from the use of smaller type. There has been, in fact, considerable addition, and our province will be simply to call attention to those points in which previous editions have been amended or amplified. Already, in the fifth edition, Mr. Darwin had stated that the able criticism of his work which appeared in the *North British Review* had induced him to modify his views with regard to the frequency of the occurrence of characters which are not useful to the individual; we find now, on some other points, a similar modification of opinion.

It has always seemed to us that one of the weakest parts of Mr. Darwin's statement of the theory of natural selection is the emphasis with which he asserts that single instances of departure from the law would prove the theory to be unsound. In the present edition, speaking of the rattle of the rattlesnake—the only effect of which has been stated to be to direct to the snake the attention of its enemies—he goes out of the way to repeat that "if it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate his theory." Why it would annihilate his theory, we must confess we are unable to understand; since Mr. Darwin repeats in this edition even more emphatically than in previous ones that "he is convinced that natural selection has been the main, *but not the exclusive*, means of modification of species." Since then other causes have been at work to cause the evolution of species, why may not some of these causes be able to produce parts beneficial to the race rather than to the species? In the special case, however, under consideration, the rattle of the rattlesnake, an American naturalist comes to the rescue of the Darwinian theory. Mr. Darwin was probably not aware at the time of writing that Prof. Shaler had stated his belief, from the result of observation, that the rattlesnake's rattle is actually beneficial to it, its object being to imitate the sound of the cicada or other insect which forms the food of many birds, thus attracting them within its power, and accounting for the apparent "fascination" of its prey, which must now be consigned to the limbo of travellers' tales.

The greater part of the additional matter in this edition is naturally devoted to a reply to the objections urged in Mr. Mivart's "Genesis of Species." In replying to Mr. Mivart's objection to the theory that "mimicry" has resulted by the process of natural selection, on the ground that the early stages of resemblance would have no useful tendency, the following sentences appear to us to be open to objection, or to be wanting in clearness:—"But in all the foregoing cases the insects, *in their original state*, no doubt presented some rude and accidental resemblance to an object commonly found in the stations frequented by them." "Assuming that an insect *originally happened* to resemble in some degree a dead twig or a decayed leaf." What is meant by the "original state" of an insect? Every insect-form must have been evolved from some previously existing simpler form by a gradual process, and the "rude or accidental resemblance" must be due to the operation of the same causes that produced the finished likeness. We must acknowledge that Mr. Darwin appears to us to fail to grapple with the difficulty in the way of the application of his theory, that either the early stages of the "mimicry" are useless, or that the exact reproduction of figure and pattern in the "mimicing" insect is a mere freak of nature. Mr. Darwin states his belief that "the sight of birds is probably sharper than ours," which would tell heavily against the utility of the first approaches towards resemblance; Mr. Wallace, if we recollect rightly, has expressed a contrary opinion.

Mr. Mivart's objection with regard to the curious fact

* The first edition of the "Origin of Species" was published in 1859.

that in the Pleuronectidæ, or Flat-fish, the eyes are opposite in the young state, and afterwards become placed both on the upper side of the head—that this change must have taken place suddenly, since any small approach to it would not be useful—is met by an ingenious argument, previously advanced by Malm. It is stated that "the Pleuronectidæ, whilst still very young and still symmetrical, with their eyes standing on opposite sides of the head, cannot long retain a vertical position, owing to the excessive depth of their bodies, the small size of their lateral fins, and to their being destitute of a swim-bladder. Hence, soon growing tired, they fall to the bottom on one side. While thus at rest, they often twist, as Malm observed, the lower eye upwards to see above them, and they do this so vigorously that the eye is pressed hard against the upper part of the orbit. The forehead between the eyes consequently becomes, as could be plainly seen, temporarily contracted in breadth. On one occasion Malm saw a young fish raise and depress the lower eye through an angular distance of about 70°."

The objections urged by Nägeli in his "Begriff und Entstehung der naturhistorischen Art," with respect to plants, that the families of plants differ chiefly from each other in morphological characters, which appear to be quite unimportant to the welfare of the speeies, are combated on the ground that we ought to be exceedingly cautious in pretending to decide what structures now are or have formerly been of use to each species. While admitting that in earlier editions he underrated the frequency and importance of modifications due to spontaneous variability, Mr. Darwin points out that many peculiarities of structure, lately supposed to be simply morphological, are now known to be intimately connected with facilities for fertilisation.

On the whole it seems to us that each succeeding edition of the "Origin of Species" lessens the distance between Mr. Darwin and those who believe that the influence of natural selection, though a *vera causa*, has been overrated as an element in the evolution of species. If it is admitted that important modifications are due to "spontaneous variability," that natural selection is not the exclusive means of modification, Darwinians and non-Darwinians have equally before them the problem to discover what these other laws are which are co-efficient in the production of new species, and what part each of these plays in producing the final result. Until this is accomplished we can hardly consider the great problem of the Origin of Species as solved. Towards the solution of it, however, the labours of Mr. Darwin will ever be held as having contributed a larger share than those of any other naturalist. When we look at the title-page, and see that a work which has produced a greater revolution in the scientific thought of the day than any published in this country since Newton's "Principia" is yet only in its eleventh thousand, and reflect that, although this is not a small sale for a scientific work, yet books which contain the germ of no new thought, and contribute not one iota to our sum of knowledge, have sold their hundreds of thousands, we cannot but think that in the coming age, when the people will really care about science, our descendants will regard this unworthy fact in the light that we do the unpopularity of the writings of Milton and Goldsmith during their lifetime.

We must not omit to mention a very useful addition, for the unscientific reader, made to this edition, in the shape of a glossary of the principal scientific terms used, prepared by Mr. W. S. Dallas.

ALFRED W. BENNETT

MAXWELL ON HEAT

Theory of Heat. By J. Clerk Maxwell, M.A., LL.D. (London: Longmans and Co. 1872.)

IT is very seldom that we meet with a book so instructive and delightful as Prof. Maxwell's "Theory of Heat." It has peculiar claims upon the student of Physics, inasmuch as it supplies a want which has been long and widely felt. The point of view is undoubtedly a new one, and to enable our readers to perceive the value of the book, we ought to make a few remarks upon the kinds of text-books that we have hitherto had. In these books the aim has been to inform the student's mind, and the fault to inform it too minutely and too exclusively. They have been of two classes—elementary books, in which the information is given in a popular manner, and advanced books, through the pages of which mathematical formulæ are very liberally interspersed.

In reading such a book the strength of the student's mind is devoted to one or at most two objects. If the book be elementary, he is bent upon acquiring a good knowledge of the facts, along with a knowledge, more or less complete, of the experimental methods by which these facts have been obtained. If, on the other hand, the book be an advanced one, his strength is devoted to grappling with and overcoming its analytical difficulties. But after he has studied both classes of text-books, he rises from their perusal with the belief that there is something wanting before he can have a thorough grasp of the subject, and a clear view of its truth and beauty. He has followed the experimenter only too zealously into his elaborate and accurate calculations, or it may be the mathematician into his profound investigations, and he now begins to realise the truth of the poet's saying—

> He who hath watched, not shared, the strife
> Knows how the day hath gone,

and to sigh for some elevated spot from which he may obtain a clear view of the whole field. He hears vague rumours that the caloric battalions and their allies the corpuscular forces, have lost the day, but he wishes to see their discomfiture more completely with his own eyes.

Such a point of view is afforded by Prof. Maxwell. He has—wisely, we think—confined himself to this one object, to give the student a clear logical view of the whole subject; nor has he broken the unity of his treatment by going into details, whether experimental or mathematical. Every true student of physics should read this book, and he will unquestionably find it a most delightful study. He will, we venture to say, rise from its perusal with a much truer and wider conception of the science of heat; and if he then wants more detailed information upon any branch, he may consult one of the ordinary text-books. Another beauty of the book is the accuracy and completeness of its historical notes. The author has successfully combined the part of historian and that of logician, and has given us very many valuable references to original memoirs, in which we may see for

ourselves the first germs of the various developments. The only thing wanting in this respect is an index, into which the various facts and names of the book might have been collected with much advantage to the reader.

Another point of interest in the book is the prominence given to the graphical method of representing truth. The Isothermal and Adiabatic curves are largely dwelt upon, and their usefulness in leading us to detect new properties of bodies is well pointed out. We are glad to think that the importance of such graphical representations is becoming well recognised in many departments of science. Even in pure mathematics, if we have occasion to calculate a series of numerical values from a formula, by plotting them upon curve-paper we shall discover at once by the eye if we have made a mistake in our calculation. In like manner, if we plot the result of a series of careful experiments after the manner of Regnault and others, we shall probably be able to determine from the appearance of the curve whether or not we may trust to the accuracy of our determinations.

Finally, by a series of lines similar to those exhibited by Prof. Maxwell, we come to see with great ease the relation that exists between the various properties of bodies; for instance, we see at once and as a direct consequence of the definition, that the ratio between the two specific heats is the same as that between the two elasticities.

We cannot close this review without remarking upon the good English in which this excellent book is written; and this, we trust, will go far to convince the scientific public that the most profound and original treatment of physics is not inconsistent with purity of language.

B. STEWART

OUR BOOK SHELF

Queen Charlotte Islands: A Narrative of Discovery and Adventure in the North Pacific. By Francis Poole, C.E. Edited by John W. Lyndon. (London: Hurst and Blackett, 1872.)

MR. POOLE enjoys the distinction of being the only educated Englishman who has ever lived on Queen Charlotte Islands, where he spent two years in an endeavour to develop the mineral resources of the country. The volume therefore necessarily possesses the interest attaching to a narrative of a residence in an almost unknown country. We miss, however, those touches which add so much to the charm of books of travel, which indicate that the writer has visited many men and many cities, and is capable of contrasting the natural products or the habits of the people of one part of the world with those of another. The attraction for the author to these islands was the presence of copper, to work which a company was formed in 1862. There can be little doubt that copper-veins, and probably other minerals, do exist in the islands in quantities that would amply repay the investment of labour and capital in their working. The climate appears to be equable and agreeable, the harbours are magnificent, and the soil is rich and productive, so that we may hope that at some future time Queen Charlotte Islands will become a valuable dependency of the British Crown. If Mr. Poole's volume succeeds in drawing to their capabilities the attention of those who are competent to develop their resources, it will have performed good service.

Hints and Facts on the Origin of Man, and of his Intellectual Faculties. By Pius Melia, D.D. (London: Longmans and Co., 1872.)

THE writer of this little book states in his preface that "he has brought together systems, facts, statements, and reasons, taken from all available sources, with the view of elucidating several important truths about man, which are at the present day either called in question or absolutely denied." The extent to which he has consulted, or the accuracy with which he has quoted from, original sources, we gathered from the fact that he entirely passes over, as unworthy of notice, the systems of Goethe and Oken, and from the statement that the "Philosophie Zoologique" of G. B. Lemarck (*sic*) was published in 1830.

LETTERS TO THE EDITOR

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

A Zoological Station at Torquay

THE article on "The Formation of Zoological Stations," by Anton Dohrn, which appeared in NATURE of the 8th inst., was read at the meeting of the Torquay Natural History Society on the 14th inst., and was the subject of an animated conversation. I am happy to add that the scheme met the warm approval of the members, and that if a station be established at Torquay, the cordial co-operation of the society may certainly be reckoned on.

W. PENGELLY, Hon. Sec.

Museum, Torquay, Feb. 17

The Chicago Observatory

A LETTER, signed by one of the Professors of the University of Chicago, commenting on the impoverished state of the Chicago Observatory since the great fire in that city, having had an extensive circulation through the Press, I have to request the favour of the insertion in your columns of the following statement on the subject, just received by the Secretary of the Royal Astronomical Society from the Director of the University, Prof. T. H. Safford.

EDWIN DUNKIN,
Hon. Sec. to the Royal Astronomical Society

Royal Observatory, Greenwich, February 22

"Dearborn Observatory, Chicago, Jan. 29, 1872

"DEAR SIR,—As the enclosed article from the London *Daily News* (see also London *Times* of January 9) might convey the impression that the Observatory is to be closed, permit me to state exactly the facts.

"The Observatory—whose funds are separate from those of the University—has, during the few years of its existence, accumulated a large stock (perhaps too large) of unpublished and only partially discussed observations, especially upon stars between 35° and 40° of declination, in conection with the German Astronomical Society, on Argelander's plan. A few months before the fire arrangements had been in progress by which it would gradually acquire the means to discuss and publish these observations, and these arrangements have been interrupted.

"So far, then, as the City of Chicago is concerned, nothing further is to be expected for the present, and, perhaps, the coming year; but as business has revived, it is expected that the difficulty of providing means will not be permanent.

"For the present it is necessary for me to give a portion of my time to geodetic and geographic-astronomical work for the United States engineers, who are conducting large operations in the central portion of the country; and the publication of our observations will be in consequence delayed.

"It is but fit that I should here acknowledge the indebtedness of the Observatory to the Hon. J. Young Scammon, at whose sole expense the Dearborn Tower and the Meridian Circle Room were built, and upon whom the support of the Institution has mainly depended.

"Our thanks are especially due to those scientific friends who have so kindly given their works. Were it not for the Greenwich and other star-catalogues received by past donations, I should have found myself in no condition to accomplish the work which I am now doing for support.

"T. H. SAFFORD,
"Director of Dearborn Observatory
"To the Secretary of the Royal Astronomical Society."

Composition of Vibrations

WHILE holding one of König's large polished tuning-forks in my hand, I happened to give it a swaying movement on the plane on which its vibrations were being performed, and immediately noticed that the space through which the fork swung was occupied by a series of bright straight lines arranged in a fan-like form. The lines spread out, or drew together, as the rate of movement impressed on the fork increased or diminished. The case was clearly one of composition of vibrations, the bright lines being merely the edges of the prongs seen in positions of instantaneous rest, where the proper motion of a prong was equal and opposite to that communicated to it by the hand.

By taking forks of different pitch, and causing them to swing with equal velocities, the dependence of pitch on the number of vibrations performed in a given time was easily exhibited.

In case this simple observation has not yet been made or described I ask its insertion in NATURE.

Trinity College, Cambridge SEDLEY TAYLOR

Eclipse Photography

MR. J. BOESINGER, in the last number of NATURE, expresses his surprise at the ignorance of the photographers attached to the late expeditions, and favours them with hints, observations, and instructions still more surprising. Because he cannot see their reasons for employing equatorial stands, plates in separate frames, and long exposures, he concludes these were unnecessary; and affirms "there must have been a great want of balance in their chemicals." No doubt there is a want of balance somewhere, and I diffidently submit the probability that Mr. Boesinger has lost his.

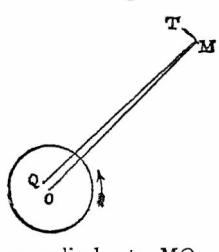
I would briefly state to those few of your readers who may have been misled by this correspondent, that equatorial stands driven by clock-work are absolutely necessary in the production of the best results, either by short or long exposure of photographic plates; a picture "not perfectly sharp but valuable as a memorial," was what Mr. Boesinger aimed at (and I sincerely hope he obtained it), but the expeditions had higher aims and greater expectations. Single large plates were exposed separately, that should a corona extending many degrees be actinically present, it might find ample room to put in an appearance; in such a case had "repeating backs" been used to give many pictures on one plate, there would have been great danger from the corona of one picture over lapping that of another, to the ruin of all. Comparatively long exposures were found necessary to secure impression from the faint extremities of the rays.

HENRY DAVIS

Tidal Friction according to Thomson and Tait

I AM so afraid that this letter will convict me of hopeless stupidity that I conceal my name. For I am going to confess that I do not understand, and even feel inclined to dispute, the reasoning of Thomson and Tait, on pp. 191-194 in their great work, respecting the effect of tidal friction on the motion of the earth and moon. It will be a convenience to your readers if I quote the passage at full length:—

"Let us suppose the moon to be a uniform spherical body. The mutual action and reaction of gravitation between her mass and the earth's will be equivalent to a single force in some line through her centre, and must be such as to impede the earth's rotation as long as this is performed in a shorter period than the moon's motion round the earth. It must therefore lie in some such direction as the line MQ in the diagram, which represents, necessarily with enormous exaggeration, its deviation, OQ, from the earth's centre. Now, the actual force on the moon in the line MQ, may be regarded as consisting of a force in the line MO towards the earth's centre, sensibly equal in amount to the whole force, and a comparatively very small force in the line MT perpendicular to MO. This latter is very nearly tangential to the moon's path, and is in the direction *with* her motion. Such a force, if suddenly commencing to act, would, in the first place, increase the moon's velocity; but after a certain time she would have moved so much farther from the earth, in virtue of this acceleration, as to have lost, by moving against the earth's attraction as much velocity as she had gained by the tangential accelerating force."



The consequences are then shown to be that the moon's distance would be increased in the ratio 1 : 1.46, and her periodic time increased, and the earth's period of rotation lengthened.

This reasoning perplexes me; for if the effect of a certain amount of fluid friction is to throw the line of action of the force from MO to MQ, a fluid friction is conceivable which should throw it outside the earth altogether. Moreover, the line of attraction of the earth on the moon would be in a line not passing through the earth's centre, a result I cannot understand, especially if the fluid friction were increased as just suggested. Nor can I see that a force in MQ, the centre of the earth being free, would tend to stop the rotation of the earth.

As I view the matter, fluid friction generates a *couple* tending to stop the rotation of the earth, and it is impossible to combine this couple with the force in MO, and represent the resultant by a single force. The energy lost in the form of momentum of rotation of the earth is gained in the heat devolved by the fluid friction, which is ultimately dissipated. And the final result would be that the orbit of the moon would not be appreciably altered, while the period of rotation of the earth is gradually lengthened.

Am I wrong, for the thousandth time in my life? and if so will some one try and enlighten me. Perhaps Prof. Tait will spare a few minutes to an old friend. M. A.

Circumpolar Lands

IN NATURE (Feb. 8) Mr. Murphy seems to admit the soundness of the reasoning by which I endeavoured to show (Jan. 25) that the earth's form is probably undergoing a slow progressive change, but he thinks that the statements in the first and last parts of my letter are contradictory.

If Mr. Murphy will be good enough to read again the paragraph immediately following the one which he quotes, I think he will find that there is no contradiction. "Transmission of pressure towards the poles" must tend to elevate the land in those regions. How that pressure is produced and transmitted I have endeavoured to show in the same paragraph.

However, the main proposition which I sought to establish in my paper of 1857, before alluded to, is that any spheroid of equilibrium, whether earth, sun, or any other, in motion about an axis, in cooling from a fluid state, undergoes a change of form, and with this proposition Mr. Murphy seems to agree.

Mr. Murphy has inadvertently omitted part of a sentence in making his quotation from my letter, thus representing me as speaking of a *ratio* with *one* quantity only.

Queen's Coll., Liverpool, Feb. 16 GEORGE HAMILTON

The Spheroidal State of Water

I HAD the pleasure a few days ago of visiting Messrs. Johnson's celebrated iron wire manufactory in Manchester. There may be seen a series of furnaces and rolling mills which in twenty-four hours can convert a truck load of the best Swedish iron into the bright and polished galvanised wire which is now being so extensively employed to complete our very perfect system of Post Office telegraphs. Every stage of the process passes beneath the eye of the observer; the melting of the pigs, the formation of the billets, the puddling of the bloom, the shingling of the balls, the rolling of the bars, and their subsequent extension by further rolling, and drawing into telegraph wire.

The bars are cut off into 10ft. lengths, and are placed in a Siemen's regenerative furnace, where they are raised to a brilliant white heat. They are then drawn out of the mouth of the glowing furnace, and pass through a series of consecutive rollers of varying dimensions, and rotating with varying speed, ultimately flowing out in a continuous stream of iron wire. In fact, the metal is at such a high temperature and so plastic that the curves it takes in falling convey the idea of a thin, fine unbroken jet of liquid matter.

The rollers are kept cool by the constant play upon them of jets of water. The first pair of rollers is fixed close to the mouth of the furnace, which is partially closed by a moveable screw that is only raised when the attendant sprite requires to direct another bar to the attenuating process of the continuous rollers. The jet of water that cools the first pair of rollers in one furnace fell in a broken shower upon the foot-plate of the mouth of the furnace, which, from its proximity to the fire, was raised to a

very high temperature, and therefore converted the drops of water into the spheroidal state. There they bounded and danced and rolled about like pith balls under an excited electrical receiver. Their constant rotation and well-known rippling motion gave them an opaque appearance which caused them to resemble closely a fine fall of hail. In fact, those to whom I pointed out the phenomenon likened their appearance to a fall of dusty snow at the mouth of a furnace. The sight was very striking and interesting. The workmen had taken these spheroids to be particles of scale and dust swaying about in the currents of air at the mouth of the furnace.

I have seen many times the experimental illustration of "Leidenfrost's phenomenon" at the mouth of a furnace, but I had never before seen its practical, though accidental, development, and in the incident which I have narrated above the interest chiefly attaches to the great antithesis of the fact and its appearance—snow at the mouth of a fiery furnace.

W. H. PREECE

The American Eclipse Expedition

I DEEM it but proper and just that I should correct a mistake that has just met my eye in Dr. Schellen's excellent work on Spectrum Analysis.

On page 332 of the 2nd German edition we find "Die erstere Expedition wählte unter der Anführung von Professor Morton die Stationen im Staate Iowa.

"(1) Burlington mit den Beobachtern Professor Mayer, Kendall, Willard, Phillips, und Mahoney, denen sich der als gewandter Spectroskopist bekannte Dr. C. A. Young, Professor am Dartmouth College (Hanover), und *Dr. B. A. Gould für die photographischen Aufnahmen hinzugestellen.*"

In the English translation, edited by Mr. Huggins, the above reads, "The first expedition, under the guidance of Professor Morton, selected stations in the State of Iowa as follows :—

"(1) Burlington, where its observers were Professor Mayer, and Messrs. Kendall, Willard, Phillips, and Mahoney, together with Dr. C. A. Young, Professor of Dartmouth College (Hanover), well known as an experienced spectroscopist, and Dr. B. A. Gould, to whose charge the photographic department was committed."

Dr. Gould had no connection with the photographic expedition, but placed himself under Professor Coffin's general organisation, so that he could have facilities for making observations on the corona, and in searching for the suspected intermercurial planet.

The Burlington station of the Philadelphia eclipse expedition was placed under the direction of Dr. Mayer, and the photographs pointing page 337 of Dr. Schellen's work are two of the five plates secured by him during totality.

Also the diagram on page 338 is from Dr. Mayer's report on the eclipse (published October 1869), an abstract of which, with accompanying copies on glass of the original negatives, was presented by M. Delaunay to the Institute of France. The Rev. T. W. Webb laid them before the Royal Astronomical Society, when the report and the photographs were discussed at length at the meeting of November 12, 1869.

HENRY MORTON, President
Stevens Institute of Technology, Hoboken, New Jersey

Mr. Spencer and the Dissipation of Energy

WILL you permit me to inquire, for the instruction of the many who are familiar with Mr. Herbet Spencer's "Doctrine of Evolution," and especially in regard to "First Principles," sec. 58, referred to by Mr. Spencer in his paper in your number for February 1, if the theory of the "Dissipation of Energy" does not upset a very considerable and significant portion of Mr. Spencer's "First Principles"? WILLIAM SMYTH

Maidstone, February 12

THE AURORA OF FEBRUARY 4

ON Sunday, the 4th inst., was witnessed one of the most magnificent displays of aurora which have been seen in Europe within the past twenty or thirty years. To most observers in this country it appeared equal in magnificence to the two fine auroræ seen on Oct. 24 and 25, 1870, and which were especially grand in England; but foreign observers could only compare it with those seen in 1831 and 1836. But if we take all the attendant phenomena into consideration, it will appear that, whilst others may have equalled this one in grandeur and beauty, there is not one which can compare with it either as to the wide extent of country over which it was visible, or as to the strangeness of many of the phenomena by which it was accompanied. The numerous letters which have appeared in these columns the last two weeks show how universally it was noticed in England, Scotland, and Ireland; but in addition to these, the letters and telegrams which have appeared in the daily and weekly papers—both English and foreign—show that it excited attention over a still larger area. It is difficult to trace the exact limits of this area; but when we mention Paris, Cologne, Berlin, Malta, Constantinople, Egypt, and India, it will be seen what a large extent of country is embraced. So far we have seen no account of it as having been visible in the extreme north of Europe, as in Iceland, Norway, Sweden, St. Petersburg, &c., where most auroræ boreales are so well displayed; but, on the contrary, many of the cities in which it was noticed are those which are commonly supposed to be too far south for such phenomena to be seen. The importance of this point will appear later on.

To take England first. Mr. Allnatt sends to the *Times* a long description of the appearance of the aurora as seen by him at Frant, which shows that it was first noticed at 6 P.M. in the S.W., and that by 7 o'clock it had reached the zenith. It disappeared at 7.45, but reappeared for a short time at 10.50 in the N.; but "at 7.30 P.M. the whole heavens were pervaded by this abnormal southern aurora, that had now expanded universally and dipped its supplementary bands into the northern horizon." He also writes :—"The earth's electricity was so powerful, that the gold leaves of the electrometer remained diverged for a considerable time!" Other correspondents describe it as seen at Blackburn, in Lancashire, at 7, "embracing the whole southern sky from N.E. round to W.;" from Faversham, in Kent, as visible between 9 and 10 o'clock;" from Cambridge as having its maximum intensity about 10; at Swindon as commencing at 10 minutes past 7 and lasting till 10 o'clock, "and giving as much light as a full moon, every object being clearly visible." But many observers had noticed it at times considerably earlier than those just mentioned: thus, "J.S.H.," writing in NATURE last week from Gloucester, "observed it at 5.30, just in the twilight, but it was then confused with the rays of the setting sun; but as the darkness deepened the aurora came out alone, and was then extremely beautiful." But still earlier was it observed at Hartlepool, whence a correspondent writes, at 5 o'clock :—"The whole of the southern sky was tinged with a most beautiful rose colour, which, as darkness set in, extended towards the zenith, where it culminated in a brilliant corona." This very early manifestation of the aurora partakes very much of the nature of a "day aurora," the possibility of which has been so much discussed in these columns (*vide* NATURE, vols. iii. and iv.) To us there does not appear much difficulty in believing that these grand meteorological phenomena, whatever their cause may be, are independent of merely relative time, and that the reason why they are mostly observed at night is because the purely local circumstances are then most favourable to their observation. That an aurora should wait till night-time before it manifests itself hardly seems probable, whilst, on the other hand, that the more brilliant light of the sun should prevent auroral displays being seen in the day-time is not only probable but is borne out by what we know of the light of the stars and planets. No one believes that stars only shine at night-time, why then should there be a belief that auroral displays take place only at night-time, especially when it is remembered that the experiences of polar travellers in their sunless regions are distinctly against it? But this is a digression arising from the fact that in comparative daylight we have distinct and independent evidence of this aurora having been observed. In addition to those already

given, from Worcester we learn that it was noticed "shortly before 6 o'clock in the twilight, when thin fleecy clouds of a bright rose colour were observed in the South and East," whilst correspondents of the *Kölnische Zeitung* state that it was first noticed at Cologne about 6 o'clock, and at Bonn about "half-past 5, gradually becoming more and more marked till 6 o'clock, when do doubt was left as to its true auroral character." While there is thus clear evidence that the phenomena had commenced some time before 6 o'clock, there is, as might be expected, great diversity as to the time when it was last visible. That this should be the case is only natural, and is entirely dependent on purely local circumstances—the state of the weather, the cloudiness of the sky, &c. Thus, whilst in some the aurora first appeared at 6 o'clock, to others it was not visible till between 7 and 8; and whilst in some places it disappeared about 8 or 9, it was then in others in its most brilliant state. But, taken as a whole, it appears to have lasted the whole evening until quite late at night; thus a correspondent, writing to the *Pall Mall Gazette* from Autun, states that "at midnight the East was crimson, and it was so light that I could tell the time easily, although my watch has gold fingers, and strong shadows were cast in rooms whose windows faced the East."

We have thus evidence of the aurora having commenced about 5 o'clock, and continuing at least till midnight, and probably later. But before proceeding to notice the other attendant phenomena, we would direct attention to a passage in the letter of the correspondent of the *Pall Mall Gazette*, before alluded to, which confirms the hypothesis that the accounts of "showers of blood," &c., mentioned in ancient chronicles were in reality only auroral displays. He writes, "all these signs and wonders produced a considerable effect upon the peasantry, who see in them warnings of a coming war; they always connect the idea of a red aurora with bloodshed." Comparing, then, all the varied accounts to which we have referred, we find very general agreement with regard to certain phenomena, some of which are of very remarkable character. The first of these is that when the aurora was noticed by those who observed it early in the evening, it appeared in the Southern and South-Western horizons, thence it seems gradually to spread, and finally appeared later on in the evening in the Northern and Eastern horizon. That this was the case is shown by the agreement of the accounts, some of which we have already quoted, and many more of which might be given. Thus at Bonn, "nothing remarkable was to be noticed on the northern horizon, whilst on the southern lay the dense, greyish bank of clouds, whence auroral streamers shortly ascended." There can also be little doubt that during the middle of the evening, and towards midnight the chief seat of the display was to the north and east, as shown in the letters of those who first observed the phenomena at about 7.30 to 9 o'clock, and continued to do so till towards midnight. The second well-marked phenomenon was that between 7 and 8. There appeared a brilliantly-coloured arch, extending across the heavens from S.W. towards the north and east. Thus at Autun we have described "a splendid and perfect arch, spanning the sky from a point on the south-eastern horizon to one on the south-western, and which lasted, more or less continuously, for two hours, whilst from 10 to 12 the sky became gradually less luminous in the south, and grew more and more splendid overhead. Till about 11 the two eastern and western auroras united in a vast arch overhead, with tongues of green flame darting through a suffused crimson." Similarly other accounts, with merely local variation. The third well-marked phenomenon appears to have been the formation of a "corona," nearly, if not quite, in the zenith, whence auroral rays streamed out in all directions. At some places this was more marked than at others, but is more or less universally noticed, both by English and foreign observers. Thus at Cardiff it is reported that "a corona, having rugged, sharply-defined edges, stood out prominently in the zenith, apparently on a parallel plane to the earth, and having its centre almost immediately over the head of the spectator, rays from which extended to the N.E. and N.W. horizons." If one may venture to say so, most auroræ visible in our latitudes appear to commence in general by an accumulation of cloud masses towards the magnetic north, then coloured masses slowly appear, and afterwards rays, or streamers, are sent up from this northern horizon towards the zenith. Sometimes the coloured masses themselves rise toward the zenith, and there the streamers pass in all directions. But in this aurora of the 4th of February, all the most marked phenomena are directly contrary to our ordinary experience, and should therefore be carefully noted. It is an extremely interesting inquiry to ascertain whether on the evening of the 3rd or 4th instant a brilliant Aurora Australis was visible in the southern hemisphere. If we consider the wide extent of country over which the aurora which we are describing was visible, the probability becomes very great that this will be found to be the case. The question then arises, Was the aurora of Feb. 4th, appearing as it did first in the southern horizon, an Aurora Australis or not? It is impossible to answer this question definitely; but we would throw out the following suggestion:—Knowing the ultimate connection that there is between northern and southern auroræ, and the fact that one of any magnitude rarely happens without the other, may we not have seen the last traces of a grand Aurora Australis, which gradually died away, whilst at the same time an Aurora Borealis was in process of formation, and which appeared in its full brilliancy in the northern and eastern horizon towards the latter part of the evening? We would make this suggestion with all due deference, but it seems to us to account in a fairly satisfactory manner for most of the very unusual and peculiar phenomena noticed, viz., the first appearance of the aurora in the south, the grand arch, the corona in the zenith, and the final disappearance in the north. We must also remember that in what is called the correspondence of northern and southern auroræ, there must be at least twelve hours difference as regards time. So that if there was an Aurora Australis on the same day, it would be dying out at the time our display was commencing.

In conclusion, the wide extent of country over which this aurora of the 4th February was visible, is easily shown. In Paris a "magnificent aurora" is reported, at Nancy and Chaumont there was a "brilliant display," while the Franco-German telegraph lines were greatly disturbed. At Constantinople one telegram states that "a splendid aurora, extending over half the heavens, was visible for several hours;" whilst another states that it was seen "from 10 till half-past 1." From Alexandria we hear that "a large space of the skies was illuminated for five hours." That it was visible at Malta, Suez, and Bombay, the following interesting account shows. It is supplied by Mr. George Draper, of the British Indian Submarine Telegraph Company, under date of Feb. 5th, and it also shows how powerful were the "earth currents" which were noticed in connection with this most brilliant aurora. He writes:—

"It may interest your readers to know that the brilliant aurora which was visible in London last night was also visible at Bombay, Suez, and Malta. Our electrician at Suez reports that the earth currents there were equal to 170 cells (Daniell's batteries), and that sparks came from the cable. These electrical disturbances lasted until midnight, and interrupted the working of both sections of the British Indian cable between Suez and Aden, and Aden and Bombay. Since Thursday last the signals on the British Indian cables have been very much interfered with by electrical and atmospheric disturbances, causing considerable delay in the transmission of messages, which all efforts failed entirely to overcome. Our superintendent at Malta also reports that there was a very severe storm there yesterday morning, so much so that they were compelled to join the cable to earth for several hours. He also reports the aurora as very large and brilliant. The electrical disturbances on the cables in the Mediterranean, and on those between Lisbon and Gibraltar, and Gibraltar and the Guadiana, were also very great. The signals on the land line between London and the Land's End were interrupted for several hours last night by atmospheric currents."

Taken, then, on the whole, this aurora of February 4th was one of the most brilliant, most interesting, and most widely visible which has been witnessed for many years past, and is probably one that will cause renewed attention to be paid to the still unsolved problem of their causes.

J. P. EARWAKER

[We have also received the following from J. W. Spengel of Berlin:—"At Berlin, the sky being covered by clouds, no one could see anything. But a young astronomer of our observatory told me that he had recognised the existence of a mighty aurora by means of the spectroscope. The magnets were also vehemently disturbed, and all the telegraphs fai'ed for several hours. The following appears in the *Leipziger Allgemeine Zeitung* for Feb. 8:—'Freiberg, Feb. 6. The aurora observed by many on the evening of Sunday caused here a complete interruption of communication through the telegraph wires for some time. The intensity between 5·40 and 6·45 overcame the strength of the battery at this station, so that it was not possible to change the oscillations of the magnetic needle caused by the earth-stream. After the northern light had become fully developed the oscillations became stronger, and followed one another at short intervals until the phenomena entirely disappeared about 7 P.M.' At Warmbrunn in the Riesengebirge, the aurora was seen magnificently from 6 to 8·30. Towards 10 it had almost disappeared. The thermometer indicated 0° C., with a violent storm from the south-west. About 11 the storm suddenly subsided; the thermometer fell to − 1·5°, and the aurora appeared for the second time in the same manner and with the same uninterrupted play of colours as at 6. After 11·30 the storm recommenced, and the aurora disappeared soon after 12. The play of the aurora on the snow-covered mountains is described as one of the most magnificent sights that can be conceived."—ED.]

REFERENCE SPECTRUM FOR THE CHIEF AURORA LINE

WHILE Nature herself seems to delight in surrounding some questions with triple difficulties and mysteries almost inscrutable, there are other questions which she has made the easiest of the easy if men will only use the means which she has prepared. And amongst such easy questions, no more signal example can be quoted than the exact spectrum place, within very narrow limits indeed, of Angström's yellow-green aurora line, whenever any aurora at all appears.

This chief aurora line coinciding precisely (as I believe I may say from my own observations, though by means of the roughest of home-made apparatus) with the second line, at W.L. 5579, of the citron band of the blue base of flame, from any and every material used for artificial illumination by man, and having immediately on one side the 1st line, of the same strength with itself, at W.L. 5630, and on the other side the fainter 3rd line, at W.L. 5535, of the same citron band; the smallest variation of spectrum place in the aurora line can be instantly perceived by the eye on this chemical scale, without the aid of any mensuration apparatus.

And yet in your last impression a respectable spectroscopist, after much labour, informs the Academy of Sciences in Paris, on Feb. 5, that Angström's yellow-green aurora line is somewhere close to Fraunhofer's solar line E, *i.e.* W.L. 5269; and in your previous impression a returning Indian observer considers the same Angström line to be somewhere near F, or W.L. 4860. Now, not only are these statements in error to the extent of from 30 to 70 times what they need be, but they cruelly drag us backwards in what should be the always onward course of science, and cause men to flounder once again in that slough of confusion they were immersed in a couple of years ago, when the chief solar corona line, at W.L. 5316, and Angström's grand aurora line, at W.L. 5579, were stated to be one and the same line, in the same place.

Excuse may, indeed, be proffered for these two observers, that they did not know of such a convenient night reference-spectrum as that which I have now alluded to; and then comes the question as to whose fault was that.

A full description of the method (after extensive trial for several months) was sent by me to the Royal Astronomical Society on May 30, 1871, with the particular request that the paper might be read at their June meeting and printed in the June Monthly Notice. This was mainly with the hope of supplying some possibly useful hints to the intending eclipse-corona-observers of December. The paper, however, though taken in, was neither read at the June meeting (if I am rightly informed) nor did it appear in the June Monthly Notice; but was handed over to secret referees, who simply sat upon it during six long months—or until the eclipse was safely past, and then they began to hint about possible objections being likely to be taken against some parts of the paper.

Of course I could not allow so admirable a society to run any risks of which they were afraid on my account; so I withdrew the paper thereupon, and am now engaged in publishing it myself, sustained in so doing by the hope that, although the eclipse for which it was mainly intended is irretrievably gone, its pages may yet be useful to some spectroscopists of aurora; and, in fact, that through their influence certain of both French and English observers will cease to attempt comparing the faint aurora's chief line with a bright solar spectrum, which they can never see in combination therewith (and if they could it has no coincident lines), but with a cheaply-procured chemical spectrum, which only comes well into view under the darkness of night, and is gifted by Nature in the spectroscope with an easily recognisable line in apparently absolute coincidence with the cosmical line of Angström.

C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, Feb. 16

*AMERICAN DEEP-SEA SOUNDINGS**

UNDER the title at foot a pamphlet of thirty-three pages, accompanied by a large chart, and illustrated by several diagrams and tables, has been issued. The school-ship *Mercury* is a vessel belonging to the commissioners having in charge the hospitals and prisons of New York city, and is employed for the purpose of training boys, committed by the magistrates for vagrancy and slight misdemeanours, to become thorough seamen. Instead of growing up to be a curse to the community, such boys are made into valuable men. The adventurous life has a special charm for them.

An essential feature of the discipline on this ship is to make long cruises, by which the boys are fitted quickly to enter into the service of the navy or mercantile marine. Of 258 boys carried out on this voyage, 100 were on the return of the ship, in the opinion of the captain, capable of discharging the duties of ordinary seamen.

The commissioners, in addition to the above object, desiring to advance the interests of science as far as lay in their power, instructed the captain, P. Giraud, to obtain a series of soundings on the line of or near the equator, from the coast of Africa to the mouth of the Amazon, to observe the set of the surface currents and the temperature of the water at various depths. He was also directed to bring home specimens of water and of the sea bottom.

The ship sailed on December 20, 1870, and arrived at Sierra Leone on February 14. On February 21 she left

* Cruise of the school-ship *Mercury* in the Tropical Atlantic, with a Report to the Commissioners of Public Charities and Correction of the City of New York on the chemical and physical facts collected from the deep-sea researches made during the voyage of the nautical school-ship *Mercury*, undertaken by their order in the Tropical Atlantic and Caribbean Sea, 1870–71. By Henry Draper, M.D., Professor of Analytical Chemistry and Physiology in the University of New York.

Sierra Leone, and the soundings and other observations were continued till she reached Havanah, April 13, 1871.

The papers, together with the various specimens, were placed in the hands of Professor Henry Draper, of the New York University, for examination. His report commences by stating "that much attention has recently been given to deep-sea researches in consequence of the investigations made by the United States government on its coast, and by Dr. Carpenter. Mr. Gwyn Jeffreys, and Prof. Wyville Thomson, in the North Atlantic and Mediterranean Sea. Not only have many of the facts so ascertained been corroborated by this voyage of the *Mercury*, but the commissioners, by authorising it, have added much that is new and interesting to our knowledge of the physical condition of the deep sea."

Then follows a discussion of the barometric variations, in which it is show that they were very small in crossing the ocean, the minimum being only $\frac{13}{100}$ below, and the maximum $\frac{8}{100}$ above the mean. In a general manner the pressure increased on nearing the American coast.

The currents varied from south near the African coast by south-west to west near the American coast, and their velocity was on an average above half a knot.

Some general remarks on the sounding apparatus (Brook's detaching apparatus) and water-collecting cylinder are next made, attention being more particularly directed to the incorrect conclusions that the latter is apt to lead to. "The constitution of the water as it exists at great depths is not correctly represented by the sample thus obtained. A considerable portion of the gases dissolved therein may escape under the relief of pressure as the cylinder is drawn to the surface, and hence examinations of such samples as regards their gaseous ingredients are liable to be deceptive. Even the saline ingredients will suffer disturbance when they are held in solution by gases that will thus escape; for instance, this is the case with carbonate of lime." Table iv. shows the specific gravities of the samples of sea water from the surface and at various depths to 420 fathoms; Table v., the air temperature between Sierra Leone and the Florida capes; Table vi., the temperature of the air, sea surface, and of the water at various depths. The thermometer was of Six's form, without index error when compared with a standard Kew instrument, but not protected on the Miller-Casella plan.

A diagram of the bed of the Atlantic Ocean at the twelfth parallel of latitude is introduced, based on fifteen soundings. It shows that "parting from the African coast the bed of the ocean sinks very rapidly. A couple of degrees west of the longitude of Cape Verde the soundings are 2,900 fathoms. From this point the mean depth across the ocean may be estimated at about 2,400 fathoms, but from this there are two striking departures—first a depression, the depth of which is 3,100 fathoms, and second, an elevation at which the soundings are only 1,900,—the general result of this being a wide and deep trough on the African side, and a narrower and shallower trough on the American. It may be that this peculiarity is a result of the river distribution on the two continents respectively, there being, with the exception of the Senegal and Gambia, no important streams on the African side, whilst on the American there are many, and among them pre-eminently the Orinoco and the Amazon, these vast rivers carrying their detritus far out to sea and helping to produce the configuration of the ocean bottom in question. However this may be, it is doubtless through these deep troughs that much of the cold water of the north polar current finds its way."

"In accordance with this we perceive, on examining the temperature of the water after the African verge of the greater or eastern sea trough is reached, that there is a difference in temperature between the surface and that at a depth of not more than 200 fathoms exceeding 25° in many cases. This decline of temperature increases as the depth increases, one observation giving an additional fall of 4° at an additional depth of 200 fathoms. It is not, however, intended to affirm that the mass of cold water is restricted to these deep troughs, since even in the West India seas at similar depths low temperatures are observed, and this though the heat of the surface water had become very much higher. In those seas while the surface temperature was 84° the thermometer at depths of 400 and 500 fathoms marked 48°; and these it must be remembered were the indications of an uncompensated instrument which was bearing a pressure of at least half a ton on each square inch of its surface, and hence registering degrees that were higher than the truth. This accords with the observation of Mr. Barrett that in the deepest parts of the sea near Jamaica there exists a temperature not far above that of the freezing point of fresh water." Accompanying these remarks is a diagram showing the curves representing the temperature of the air, surface of the water, and deep water during the voyage, and that is followed by a diagram of the specific gravity of surface and deep water.

"The general conclusion which may be drawn from these results as to temperature and specific gravities is that there exists all over the bottom of the tropical Atlantic and Caribbean Sea a stratum of cold water—cold since its temperature is below 50°. This is the conclusion to which Dr. Carpenter has come as respects the Atlantic in higher north latitudes; and in this important particular the cruise of the *Mercury* must be considered as offering confirmatory proof of the correctness of the deductions drawn from the cruises of the *Lightning* and *Porcupine*."

"There are reasons for supposing that, so far from this water being stagnant, its whole mass has a motion towards the Equator, whilst the surface waters in their turn have a general movement in the opposite direction."

An analysis of the gaseous ingredients was not attempted, because the specimens had been kept too long and for other reasons that are specified; but in relation to organic matter it is stated: "I made some examinations of the organic matter contained in these waters both by incinerating the solid residue and by the permanganate test. . . . . It needed no especial proof that organic matter was present in every one of these samples, for the clearest of them contained shreddy and flocculent material, some of them quantities of sea-weed in various stages of decomposition. With these vegetable substances were the remains of minute marine animals. As bearing on this subject I found on incinerating the solid residue of a sample of water taken from 200 fathoms, that the organic and volatile material was not less than 11 per cent. of the whole. Though the quantity of organic substance diminished as the structure under examination was deeper, there still remained a visible amount in the water of 400 or 500 fathoms. It is probable therefore that even at the bottom of the ocean such organic substance may exist, not only in solution affording nutriment to animals inhabiting those dark abysses as Prof. Wyville Thomson has suggested, but also in the solid state. Plants of course cannot grow there on account of the absence of light."

"In order to determine whether any hitherto unknown element existed in these waters, I subjected the solid residue to examination with the spectroscope, volatilising the substances by the aid of a voltaic current and induction coil. A careful examination did not reveal the presence of any spectral lines other than those belonging to the well-known elementary substances in sea-water."

"The specimens of the bottom, obtained by attaching to the sounding line quills or wooden tubes, I have transmitted to Dr. Carpenter, who has kindly consented to examine them. In a letter recently received he says, 'As far as I can see they consist of the ordinary Atlantic mud, chalk in process of formation, with the ordinary types of deep-sea foraminifera.'"

THE RECENT AURORA, AND A NEW FORM OF DECLINOMETER

ON Sunday night, the 4th of February, we saw here the magnificent coloured Aurora Borealis, which has been described in NATURE, in the newspapers, and which, I see from telegrams, has been observed at very distant stations. Indications of the aurora were noticed here soon after sunset; but about 6.45 P.M. the whole eastern portion of the sky became illuminated with red light, at first faint, but rapidly becoming more and more intense, while yellow streamers began to shoot up from the north-eastern arc of the horizon nearly to the zenith. About the south-west there was also much red and yellow light; it was spread over a large apparent area, but was not so intensely bright or so strongly coloured as that which lay to the north-east. It too, however, possessed splendid broad, yellow streamers. The display lasted in full beauty till about 7.20, but long after that time much red and yellow light with occasional streamers was to be seen.

It is strange that the phenomena of the Aurora Borealis still remain so little understood. It would add much to our knowledge, if those who witness these displays would make sketches of the appearances at the time when very definite forms of the streamers are observed, noting also the time of the observation very carefully, and the position of well-known stars and constellations. A comparison of such sketches, and of notes that might accompany them, would give us most important data, and might lead to the determination of the locality of the discharge

Simultaneous observations, at widely different stations, of the disturbances of terrestrial magnetism that always accompanied the aurora might, if compared, give us useful information as to the direction and velocity of the electric discharge; and would probably at least help us to decide whether it is to the discharges themselves, or to earth-currents, or to both combined, that these disturbances are due.

I wish to describe an instrument planned by Sir William Thomson, which may be easily constructed, and with which the variations of the horizontal component of terrestrial magnetism can be determined with great accuracy.

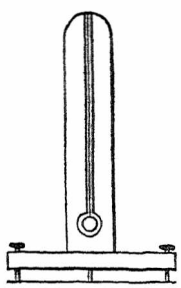


FIG. 1

A flat wooden support, seven or eight inches high, is fixed on a convenient foot furnished with levelling screws, and in the face of it a groove, rather more than four inches long and about $\frac{3}{16}$ of an inch deep, is cut. From a point at the top of this groove, a very light mirror with magnets attached—such as is used in Thomson's reflecting galvanometer—is suspended by a single silk fibre about four inches long; and in front of the groove there is fastened, if the mirror be concave, a slip of plate glass to keep off currents of air: or, if it be a plane mirror, a lens is fastened in front of it, and the remainder of the groove is covered up with a slip of glass or in some other way. A lamp is placed in front of the mirror, and the reflected image of it is received on a scale. The motions of the reflected light upon the scale indicate the deflections of the magnet.

Suitable mirrors and lenses are constructed by Mr. White, instrument maker, Glasgow. In making the mirrors, a large number of the lightest circular glasses used for covering objects on slides for the microscope are silvered; and from these those which give an image perfectly free from distortion are selected by trial. Many of the mirrors formed are much twisted and quite unfit for use; but mirrors are obtained by this plan of selection by trial far superior in lightness and in freedom from distortion to any that can be made by expending extreme care in the glass-work. To the back of each mirror four small magnets are attached; an arrangement which has been found by trial to give the best result. The object is to make the mirror with its magnets sufficiently light, and to give it at the same time the greatest possible magnetic moment. The mirror is three-eighths of an inch in diameter, and weighs not more than one-third of a grain.

Plane mirrors are generally used in Glasgow, and the lens is of such power that a lamp placed at a distance of one metre (about 40 inches) gives an image at the same distance from the mirror. The lamp is placed behind a screen, and in the screen an oval hole is cut and a vertical wire * is stretched across it. The image of this wire is received upon a scale. The scale may be set at a distance of 40 inches (one metre) from the mirror; that is to say it

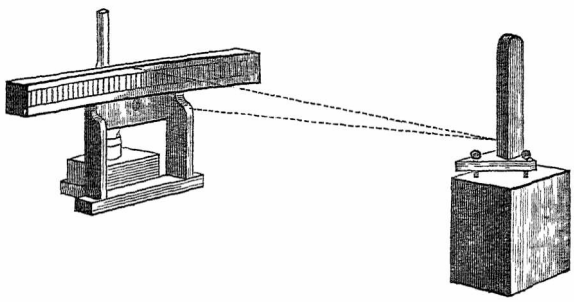


FIG. 2

may be attached to the screen between the mirror and the lamp; or it may be put much farther away, at, say two or three times that distance. The lamp and screen, with its slit and wire, must then be brought near enough to the mirror to throw back the conjugate focus sufficiently. This arrangement gives of course increased sensibility. We use for it a paraffin oil lamp, of which the reservoir is a very shallow rectangular vessel. The slit in the screen is slightly above the horizontal plane through the centre of the mirror, and the scale slightly below that plane. The reflected ray passes below the reservoir of the lamp to the scale beyond.

Our scales, which are also obtainable from Mr. White, are divided into fortieths of an inch, and are generally attached to a piece of wood, cut out so that its curvature corresponds to that of a circle described with the distance of the mirror as radius. Thus, by dividing the number of scale divisions by the distance of the mirror in fortieths of an inch from the scale, the angle is obtained to which that number of scale divisions correspond. At a distance of 60 inches we can easily read the position of the image of the wire on the scale to less than half a scale division, which, since the angle turned through by the reflected beam of light is twice that turned through by the mirror, corresponds to an angular deflection of about 20″.

The great advantage which the arrangement that I have just described possesses over any that are ordinarily used for observing rapid variation in magnetic declination lies in the lightness of the mass moved. The heavy declinometers employed in observatories are unable, through their great inertia, to follow accurately the sudden variations that occur during a magnetic storm.

JAMES THOMSON BOTTOMLEY

The College, Glasgow

* A simple vertical slit was formerly used, but the vertical wire in the middle of the slit, a suggestion of Prof. Tait, is a great improvement, as it enables us to use plenty of light, while it gives increased precision to the reading on the scale.

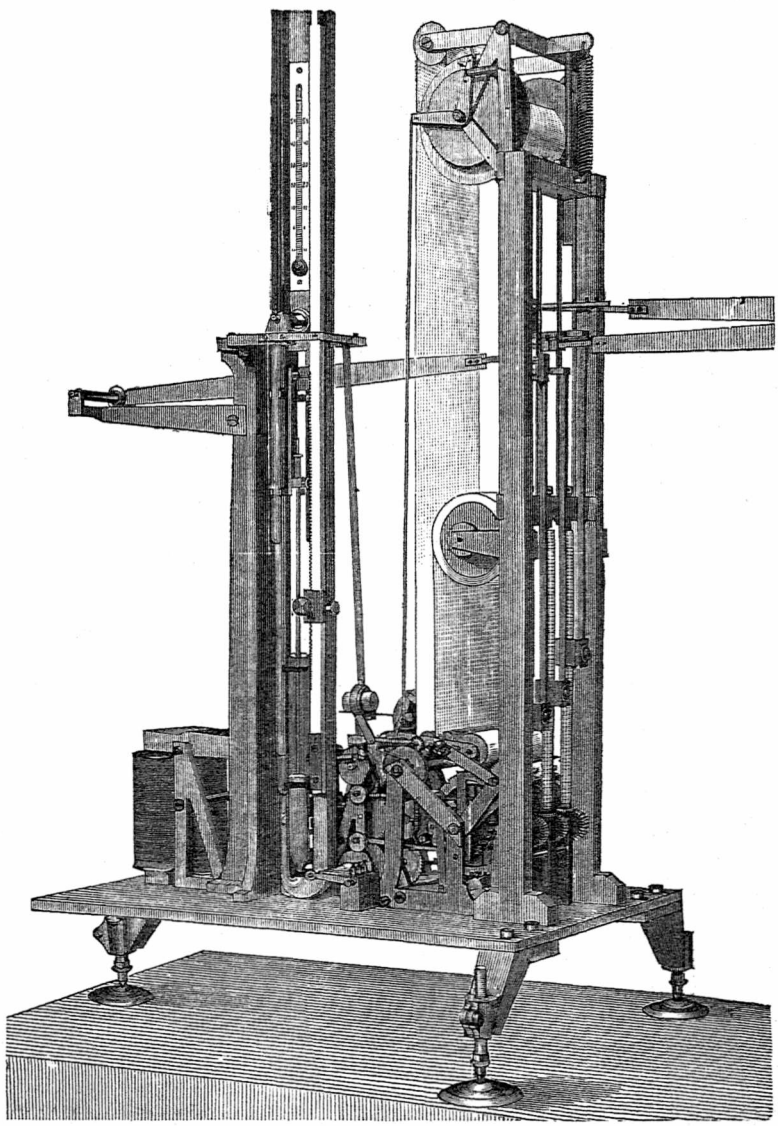
THEORELL'S PRINTING METEOROGRAPH

SOME time since brief mention was made of the above instrument (devised by Dr. A. G. Theorell, of Nybrogatan, Stockholm, Sweden—NATURE, vol. iv. p. 466)—with reference to its having been exhibited at the London International Exhibition, 1871.

Being in possession of a detailed description of the same, I have thought that a copy with additional remarks may be of interest in the pages of NATURE.

I have no hesitation in saying that this instrument does not occupy the prominent position to which it is justly entitled, neither do I think that it is so well known—whether taken in the light of a wonderful piece of mechanism, or of excellent workmanship—as it well deserves to be.

The following is a description of the instrument in accordance with the original, excepting only that I have made a few slight alterations in order to render it more intelligible, the original having been, as I suppose, translated from the Swedish language, and not well expressed :—



"Meteorological observations are by this instrument delivered in tables printed on a slip of paper. Of the four tabular columns the *first* gives the hours, the *second* the temperature, the *third* the degree of humidity according to August's method, and the *fourth* the atmospheric pressure; this last (atmospheric pressure) is given in millimetres, but the first figure, being always a 7, is suppressed. The degrees of the thermometer employed are those of the centigrade scale, and negative degrees are expressed by their complements to 100.

"The registration takes place by means of electrical currents, which are closed by contact between the mercury in the various meteorological instruments and steel wires that descend into their tubes. These steel wires are connected, by means of levers and three vertical screws, each with its respective system of brass wheels with numerical type engraved on the edges, in such a manner that the rotation of the wheels causes an upward or downward motion of the steel wires, so that the point of the scale on which the lower extremity of the wire is situated, is neces-

sarily that indicated by the number appearing at the same moment uppermost on the corresponding wheels.

"The wheels containing the figures are governed by an electro-magnetic motor, which, for each observation, sets the three wheel systems successively in motion, until the corresponding wires have reached the mercury in their respective meteorological instruments, when the above-mentioned electrical current instantly arrests the motion, so that accordingly all three steel wires stop with their lower extremities each in contact with the surface of the mercury in its respective instrument. The numbers therefore that stand uppermost on the numbered wheels are just those which indicate the height of the barometer and of the two thermometers, and now the same electro-magnetic motor operates upon a printing apparatus which, after having deposited colour on the type, presses the slip of paper against them. This being done, the steel wires are drawn up again by the motor, which stops as soon as a certain distance from the mercury is attained, and all is ready for the next observation.

"The interval between the observations is a quarter of an hour."

Attention is then directed to the following considerations:—

"The instrument delivers the observations in a form in which they may immediately, and without further modification, be used by the meteorologist in his work.

"A very large number of very carefully made comparisons have shown that the observations registered by this method possess an accuracy equal to that which is generally attained by ocular observation.

"The zinc vessel, in which the upper ends of the thermometers are enclosed, is so air-tight that it is found possible, by means of chloride of lime and caustic potash, to keep the enclosed air always free from damp and carbonic acid, a precaution which it will be easily understood is necessary in every climate where the temperature is liable to sink below the freezing-point, but is still further necessary to protect both the mercury and the steel wires from oxidation, and thus preserve the galvanic contact.

"A meteorograph of this construction has for two years and three-quarters been in use at the Upsala Observatory, executing six observations every hour, without any perceptible alteration of the surface either of the mercury or the steel wires, that could in any way affect either the free efficiency of the instrument or its degree of accuracy, which throughout the whole time has been found to be that above named.

"As the clock which determines the time of the observations does not require winding up—the instrument itself restoring the tension of the mainspring every quarter of an hour—it continues to go as long as the driving force, *i.e.*, the electrical current, is maintained; and, as the slip of paper applied lasts fully three months, it is clear that that is the period for which the instrument may be left to itself. The work then requisite is little more than to take out, cut, and sew up in order the paper of observations, and replace it with another slip. We thus see that this instrument requires but very little time and labour of the person who takes charge of it.

"It is entirely for special reasons that the construction of the instrument has been limited to the registration of thermometrical, psychrometrical (hygrometrical), and barometrical observations, for the method may be applied advantageously to observations of the course of any phenomena whatever, provided they can be indicated by an index admitting of galvanic contact. It is, therefore, applicable for all the now usual kinds of meteorological observations, and nothing prevents the same instrument executing and printing them all in one and the same table."

The following is an extract (giving one hour's instrumental readings) of one of the printed forms referred to in the first paragraph:—

1	57	130	673
	57	1305	6725
	57	132	672
	57	133	673
2	57	1335	672

The width of the slip of paper used in these observations is 4·25 in.

In the Exhibition meteorograph, the timekeeper (referred to in the eighth paragraph) is merely a watch-movement of moderate size.* In the place of the ordinary minute-hand there are four, fitted on the same centre and projecting from each other at right angles in the form of a cross; in other words, the points (one of which resembles what is technically termed a *spade* hour hand, and indicates the time) are 15 min. apart. Every time one or other of the hands comes opposite the figure III. it depresses a small steel lever which, through suitable mechanism, completes the circuit.

I am indebted to Dr. Theorell for a very courteous letter, dated from Upsala, respecting the block used in the original description, also to Messrs. Norstedt and Son, printers to the Swedish Government, for supplying me with an electrotype copy of the same through the Swedish Consulate.

JOHN JAMES HALL

ON SLEEP†

PROFESSOR HUMPHRY commenced his lecture by giving a brief account of some of the changes that take place in the tissues when their function is active, and explained that during this time a slight deterioration of structure takes place, which, affecting the voluntary system, the muscles and hemispheres of the brain, causes the sense of tiring, and necessitates a period of rest for the restoration of the tissues to their former condition. In the case of the muscles this rest is provided for by periods, quickly alternating periods, of action and cessation of action. But in the case of the brain, the actions upon which consciousness, volition, &c., depend cannot be thus frequently suspended. Their continuance is needed for the safety of the body during long periods, through the whole day, for instance; and longer periods are therefore required for repair. These are the periods of sleep.

He next took a cursory glance at the different parts of the nervous system, explaining that the upper regions of the brain are those which minister to consciousness and volition, the intellectual operations, &c. He showed that the functions of these regions not only can long be suspended without interfering with the action of the lower parts of the brain, which are more immediately necessary to life; but that they are very easily suspended—slight causes, such as a jar, or a shock, or an alteration in the blood current, being sufficient to stop the action of these parts and deprive the person of consciousness. The spontaneous stopping of their action, consequent on the slight deterioration of their structure from the continuance of their functions during the day, is the proximate cause of sleep during the night; and the periodic recurrence of sleep is in accordance with the periodicity observed in several of the nutritive functions, and, indeed, witnessed in many of the other operations of nature.

After observations upon the condition of the brain during sleep, the circumstances that conduce to sleep, the time that should be allotted to it, and other points, the Professor entered at some length into the subject of dreams. These he regarded not, as has been supposed by some, to be a necessary attendant on, or feature of, sleep, but rather to be the result of an abnormal condition. In the natural state we should pass from wakefulness to complete uncon-

* On the other side of the instrument to that seen in the engraving.

† Abstract of a Lecture delivered at the Royal Institution, on Friday, February 9, by Prof. Humphry.

sciousness, and *vice versâ*, quickly, almost instantaneously, and many persons habitually do so. But the transition period is sometimes prolonged, and stages are observable. The first thing that occurs is the lowering, or cessation, of that control over the mental processes which is the highest of our powers, the one requiring the greatest effort, and the one most easily lost. In this condition the thoughts ramble unchecked, chase one another confusedly over the mental field, and give rise to all sorts of incongruities of the imagination. At the same time, being unrestrained, they are excited, and evince efforts of memory and even of combination, of which, in the regulated state of wakefulness, they are quite incapable. In this way the images of persons and places events, and items of knowledge, long forgotten in the ordinary state, are recalled with distinctness, and we fancy that new information has been acquired when it is only forgotten facts that are recalled. He did not agree with the physiologists who conceive that dreaming depends upon an inequality in the condition of different parts of the brain, some being excited or wakeful, while others are quiescent or asleep. He rather took the view that all the parts of the cerebral hemispheres combine in each of the efforts of control, consciousness, memory, and other mental acts, that all suffer alike from those efforts, alike need the restoring changes which take place in sleep, and, together, *pari passu*, pass through the stages on the way to and from sleep, in which dreaming, sleep-walking, &c., occur.

NOTICE OF THE ADDRESS OF PROF. T. STERRY HUNT BEFORE THE AMERICAN ASSOCIATION AT INDIANAPOLIS *

IN a brief notice of the recent address of Prof. Hunt, it is stated that, while the discussions show learning and research, and his review of the progress of opinions with regard to the Taconic and associated rocks is an able presentation of the subject, its conclusions are throughout open to doubts and objections. Since it is fairer to an author to make special, rather than general, criticisms, I propose to state here a part of the objections referred to in that remark. They are as follows :—

1. That, while accepting the ordinary views with regard to most "pseudomorphs by alteration" (crystals chemically altered without a loss of form), he rejects them with respect to those that are silicates in composition ; that is, he denies that the crystals of serpentine having the form of chrysolite, pyroxene, dolomite, &c., are pseudomorphs ; and the same of those of steatite, having the form of hornblende, pyroxene, spinel, &c. ; of those of pinite having the form of nephelite, scapolite, &c. ; and so in other cases :—notwithstanding that (1) they bear positive evidence of change in having ordinarily no polarising properties, and no other interior features or qualities conforming to the external form ; that (2) the crystalline forms are just those presented by the species after which they are supposed to be pseudomorphs, and the idea of their being real forms of a single polymorphous species is wholly inadmissible, as pronounced by every crystallographer who has written on the subject ; that (3) the pseudomorphs show all stages in the process of change from incipient to complete alteration, in the latter case not a trace of the original mineral remaining.

In this assumption, for it is little better, he opposes the views of every writer on pseudomorphs, excepting one—Scheerer ; and Scheerer's chemical speculations, which are at the basis of his opinions, he rejects, like all other chemists.

This unwarranted assumption has a profound position in the system of views on metamorphism which Prof. Hunt holds, and gives shape and intensity to his opinions of the views of others.

2. That, in commencing a paragraph with the sentence, "The doctrine of pseudomorphism by alteration, as taught by Gustaf Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof, and many others (meaning thereby other writers on pseudomorphism), leads them, however, to admit still greater and more remarkable changes than these, and to maintain the possibility of converting almost any silicate into any other"—he grossly misrepresents the views of at least Rose, Haidinger, Blum, Rammelsberg, Dana ; and that he completes the caricature in the closing sentence of the same paragraph, in which he says, "In this way we are led from gneiss or granite to limestone, from limestone to dolomite, and from dolomite to serpentine, or more directly from granite, granulite or diorite to serpentine at once, without passing through the intermediate stages of limestone and dolomite ;" part of which transformations, I, for one, had never conceived ; and Rose, Haidinger, Rammelsberg, and probably Blum and the "many others," would repudiate them as strongly as myself. Next follows a verse from Goethe, that is made to announce his personal vexation with their "sophistries ;" *alias* absurdities, as the context implies.

Prof. Hunt's rejection of established truth alluded to under sec. 1 here manifests its effects in leading him to misrepresent—although unintentionally—the views of writers on pseudomorphism ; and to add to his misrepresentation by means of the strange conclusion, that, because such writers hold that crystals may undergo certain alterations in composition, therefore they believe that rocks of the same constitution may undergo the same changes ; as if it were not possible that external or epigenic agencies might reach and alter crystals under some circumstances of position, when they could not gain access to great beds of rock. Haidinger, the eminent crystallographer, mineralogist, and physicist of Vienna, and one of the most prominent writers on pseudomorphism, never wrote upon the subject of the alteration of rocks at all, and this is true of others, against whom the above charge is made by Mr. Hunt.

With a little clearer judgment, part at least of that vexation of spirit which required the help of a great German poet, and the German language, adequately to express, might have been avoided.

3. That he charges me with the opinion of Bischof, that "regional metamorphism is pseudomorphism on a grand scale :" when I make no such remark, neither express the sentiment, in my Mineralogy of 1854, in which I give an abstract of Bischof's views and make my nearest approach to them ; and when, if there was any occasion for a notice of my opinions, a critic of 1871 should have referred to the formal expression of them in my "Manual of Geology," first published in 1863. The reader will there find the "diagenesis" of Gümbel, which Mr. Hunt takes occasion to commend, applied, as had been done by others, although Gümbel had not then announced it ; and also other points discussed, with but a brief allusion to pseudomorphism.

The above remark by Mr. Hunt is not made with special reference in his address to magnesian silicates, or any other particular class of siliceous minerals ; but, as the context shows, to rocks in general. I have held to views respecting the origin of serpentine which Prof. Hunt rejects, and have sustained them on the ground that the pseudomorphous crystals of serpentine show what transformations are chemically possible, and that hence they may possibly illustrate the changes which beds of rock have undergone. I have not applied this principle in accounting for the origin of ordinary metamorphic rocks, because, as above observed, crystals may often be reached by agencies which can never reach or affect rock-formations, and for various other reasons against it. But the case of serpentine has been regarded as somewhat

* Prof. Hunt's address has been published in the "American Naturalist" for September, 1871, and, since then, in part, in NATURE, Vol. v. Nos. 105, 106. 107. Prof. Dana's reply is reprinted from advance-sheets of *Silliman's Journal* forwarded to us by the author.

different; and I have believed, and still believe, that extended beds of rock have been turned into this mineral by a method analogous to that which takes place in pseudomorphism. Had Mr. Hunt's statement been made a special one, restricted to this case, I should have had little objection to it. I may add that the method of origin for serpentine which I have deemed most probable (though perhaps not the only method) is one which he once advocated—that of the alteration of beds of dolomite, or magnesian carbonate of lime, by waters containing alkaline silicates in solution; and it has appeared to me that the facts (1) that serpentine is commonly associated with beds of limestone or dolomite, (2) that chrysolite crystals are sometimes found in these rocks, and (3) that the forms of crystals of both dolomite and chrysolite occur among serpentine pseudumorphs, give strong support to this view.

Prof. Hunt's opinion on this point in 1857 he thus expressed in a letter to the writer, sent for insertion in "Silliman's Journal," where it appears in volume xxiii. (1857) at p. 437, as a conclusion to his brief statement.

"Suppose a solution of alkaline silicate, which will never be wanting among sediments where feldspar exists, to be diffused through a mixture of siliceous matter and earthy carbonate, and we have, with a temperature of 212° F., and perhaps less, all the conditions necessary for the conversion of the sedimentary mass into pyroxenite, diallage, serpentine, talc, rhodonite, all of which constitute beds in our metamorphic strata. Add to the above the presence of aluminous matter, and you have the elements of chlorite, garnet, and epidote. We have here an explanation of the metamorphism of the Silurian strata of the Green Mountain range, and I believe of rock metamorphism in general." Again, in a letter dated July 6th, published in volume xxiv., at page 272, he says:

"I have already in a previous note indicated the manner in which I suppose these siliceous and argillaceous magnesites and dolomites to have been in certain parts of the formation transformed by the intervention of solutions of alkaline carbonates into silicates, such as talc, serpentine, chlorite, pyroxenite, &c. A further development of my views of the metamorphism of sediments, with the results of the investigation of a great many altered rocks, will appear in the Report of Progress of the Geological Survey of Canada for the last three years—now in press."

It should be added, that Prof. Hunt acknowledges his change of opinion in his address. But, in view of it, some moderating of his positiveness of assertion would have been reasonable.

4. That he attributes the origin of *beds* of serpentine and steatite,—here following nearly Delesse,—to the alteration of beds of different hydrous magnesian silicates related to sepiolite (meerschaum), formed in the surface waters of an era—Palæozoic or earlier—while fossiliferous rocks were in progress:—when, as a matter of fact, no such sepiolite-like beds are known to occur anywhere in *unaltered* stratified formations of Palæozoic or pre-Silurian time, and they are found of limited extent only in some strata of comparatively recent origin. The hypothesis, although deserving of consideration, is therefore without any solid foundation. The doubts that have been recently thrown about the Eozoön affect unfavourably the hypothesis, since these supposed fossils have been made prominent in its support. The view, if true, would, as Prof. Hunt implies, bring the making of serpentine and steatite rocks under the kind of metamorphism styled by Gümbel diagenesis, instead of that of epigenesis; making them a result of change without an addition of ingredients from any external source, like most other metamorphism, instead of through the agency of outside ingredients. But it wants facts to rest upon.

5. That he attributes an origin similar to that for serpentine and talc to beds of chlorite and hornblende; notwithstanding the fact that chlorite schist and hornblende schist—the purest forms of any large beds of these minerals—are always more or less impure, and often graduate into clay slate on one side, and mica schist on the other; and that these schists are thus so involved with others, that if one is derived from ordinary sedimentary beds, all must be.

6. That he devotes some pages to a "theory of envelopment" as a method of accounting for the silicate pseudomorphs referred to, beginning a paragraph with the sentence:—

"By far the greater number of cases on which this general theory of pseudomorphism by a slow process of alteration in minerals has been based, are, as I shall endeavour to show, examples of the phenomenon of mineral envelopment, so well studied by Delesse in his essay on Pseudomorphs."

While, in fact, this theory has almost nothing to do with the subject, since pseudomorphs of serpentine, steatite, and other species, with regard to which there is the dispute, consist often of *pure* serpentine, steatite, &c., and therefore have no enveloper, and are not cases of envelopment. This theory supposes the material of the so-called pseudomorph to be an impurity taken up into a crystal in process of formation—a thing of common occurrence; and, if satisfactory, would account for the want of conformity between internal qualities and external form. It is unfortunate for it that, as just shown, it does not apply where it is wanted.

7. That he makes Delesse the author of the "theory of envelopment:"—when Delesse has not proposed any such theory for cases of ordinary pseudomorphism, but has simply commenced, and very judiciously, his work on Pseudomorphs (1859) by distinguishing the examples of mere impurity, or envelopment, in crystallisation, in order to clear the way for the actual facts; and then gives a long list of admitted pseudomorphs, including in it nearly all kinds so recognised by other authors, and all that affect the question discussed by Prof. Hunt; serpentine occurring in the list as forming pseudomorphs after chrysolite, hornblende, garnet; steatite after pyroxene, hornblende, epidote, scapolite, mica, topaz, magnesite, dolomite, &c. In his work on metamorphism (1861), Delesse takes back none of his views on pseudomorphism; and in his late "Reviews of the Progress of Geology," down to the last just out (1871), he reiterates the ordinary views with regard to pseudomorphism, and mentions the occurrence of other pseudomorphs consisting of talc, serpentine, &c.

8. That he cites Naumann as sustaining the "theory of envelopment:"—when this learned crystallographer and mineralogist has only commended Delesse's chapter on the envelopment of minerals in crystals, and presents in his "Mineralogy" (the last edition of which, that of 1871, is now before me) the subject of pseudomorphism in the usual way, with nothing whatever on the theory of envelopment; and, under the description of the species serpentine, he speaks of "large pseudomorphous crystals of serpentine from Snarum which still contain a nucleus of altered chrysolite."

There is hence no foundation for Mr. Hunt's statement that his views are "ably supported by Delesse," or any occasion for the "no small pleasure" he derived from Naumann's letter; or any warrant for the remark (p. 47) that Delesse and Naumann hold the "view" "that the so-called cases of pseudomorphism, on which the theory of metamorphism by alteration has been built, are, for the most part, examples of association and envelopment, and the result of a contemporaneous and original crystallisation." These men of science are not to be counted upon for aid, countenance, or comfort; though claimed as friends, it has not been their fault, as they have always avowed the opinions of Haidinger and the "many others." It is a strange fact that, neither these claimed friends, nor the many announced opponents, with one or two exceptions,

hold the views which Prof. Hunt has attributed to them in his address. We are glad to know that this is not the usual American method of dealing with authorities.

Gümbel and Credner are the other two claimed supporters of his views. They have sustained Mr. Hunt's opinions as regards the Eozöon and the origin of the serpentine constituting it. But whether they disagree with Haidinger and all others as to pseudomorphs of serpentine, and of other hydrous silicates, I cannot say.

9. That while setting down the Taconic rocks, and rightly, as Lower Silurian in age, he denominates the micaceous gneisses, diorites, epidotic and chloritic, steatitic and serpentinous rocks, talcoid mica schists, quartzites, and clay-slates (which are always without staurolite or andalusite), in fact, the whole range of metamorphic rocks, with small exceptions, between the Connecticut river and the great limestone formation of the Green Mountains (admitted to be Lower Silurian), as the *Green Mountain Series*, and makes the whole "*pre-Cambrian*" in age, although the region has not been examined by any one stratigraphically with the care necessary for a positive opinion; and, although there are gneisses, mica schists, and chloritic talcoid (or mica) schists in the Taconic series, and therefore of admitted Lower Silurian origin, which are closely like those of his Green Mountain Series.

10. That he denominates, in like manner, the gneisses, mica schists (said to be richer in mica than those of the Green Mountain Series), hornblendic gneisses and schists, micaceous and clay-slates containing andalusite, cyanite, or staurolite, and certain limestones, existing east of the Connecticut river, as a *White Mountain Series*, and makes these a newer "pre-Cambrian" than the Green Mountain Series:—when there is the same want of stratigraphical evidence as to age as in the former; and when Prof. C. H. Hitchcock's discoveries of Helderberg corals (Lower Devonian, according to Billings, or else upper beds of the Upper Silurian), at Littleton, not far north of the western extremity of the White Mountain Series, makes it more probable that part of the White Mountain Series of beds are of Helderberg age rather than pre-Silurian; and his discovery of labradorite rocks on the south-western margin of the White Mountains, wholly unlike any of the so-called White Mountain Series, shows further that a vast amount of study in the field is needed before the dictum of any one respecting the age of New Hampshire rocks is worth much.

It is now proved that there are labradorite rocks in Waterville and Albany, N.H., on the borders of the White Mountain region, which are *probably* of *Laurentian* age; that on the other side of the White Mountain line, but 25 miles to the north-northwest, there are fossil-bearing, metamorphic rocks of the *Helderberg* (upper or lower) period; that 100 miles south-southwest, in Bernardston, Mass., or central New England, there are other fossil-bearing metamorphic *Helderberg* rocks, some of the well-preserved crinoidal stems (as the writer has seen, as well as read of in the account of Prof. Hitchcock) *an inch in diameter*. Who then knows whether all, or any, of the long intermediate periods of geological time, from the Laurentian to the Devonian, are represented in the New Hampshire metamorphic rocks lying between these limits? When observation has given positive knowledge, we may then have several "White Mountain Series."

11. That he has relied, for his chronological arrangement of the crystalline rocks of New England and elsewhere, largely on lithological evidence, and commends this style of evidence, when such evidence means nothing until tested by thorough stratigraphical investigation. This evidence means something, or probably so, with respect to Laurentian rocks; but it did not until the age of the rocks, in their relations to others, was first stratigraphically ascertained. It may turn out to be worth something as regards later rocks when the facts have been carefully tested by stratigraphy. A fossil is proved, by careful observation, to be restricted to the rocks of a certain period, before it is used—and then cautiously—for identifying equivalent beds. Has anyone proved by careful observation that crystals of staurolite, cyanite, or andalusite, are restricted to rocks of a certain geological period? Assumptions and opinions, however strongly emphasised, are not proofs.

It is no objection to stratigraphical evidence that it is difficult to obtain; is very doubtful on account of the difficulties; may take scores of years in New England to reach any safe conclusions. It must be obtained, whatever labour and care it costs, before the real order and relations of the rocks can be known. Until then, lithology may give us guesses, but nothing more substantial.

Mr. Hunt's arguments with reference to the White Mountain Series, as urged by him in 1870, will be found in *Silliman's Journal*, ii. 1. 83. Both there, and in his address, may be seen the kind of evidence with which he fortifies, or supplements, that based on the character of the rocks. Direct stratigraphical investigation over the region itself, in which all flexures, faults, and unconformabilities have been thoroughly investigated, is not among the foundations of opinion which he brings forward.

He endeavours to set aside the objections to his views suggested by the existence of Devonian or Helderberg rocks in central and northern New England; but he presents, for this purpose, only some general considerations of little weight, instead of definite facts as to the extent and variety of the metamorphic strata that are part of, because conformable to, these Helderberg beds. Had he studied up these stratigraphical relations with the care requisite to obtain the truth, and all the truth, perhaps he would no longer say—it is "contrary to my notions of the geological history of the continent to suppose that rocks of Devonian age could in that region have assumed such lithological characters." Notions often lead astray.

JAMES D. DANA

NOTES

THE Royal Horticultural Society has taken a step which may prove very advantageous to the interests of science, namely, the appointment of a botanical Professor, who, by lectures, answers to personal inquiries, and other means, shall assist in establishing a more correct knowledge of the principles of botany and horticulture, and of the names of plants, among those of the Fellows and their gardeners who are desirous to profit by the opportunity. Among the duties of the Professor of Botany will be to conduct the scientific business of the society, both horticultural and botanical; to enter into communication with horticultural and botanical establishments at home and abroad; to conduct the meetings and edit the publications of the society; to give courses of lectures on scientific botany to the gardeners and others; and to have a general superintendence of the gardens at Chiswick. The appointment to this office of Mr. W. T. Thiselton-Dyer, late Professor of Botany at the Royal College of Science, Dublin, is a guarantee that the cultivation of scientific botany will not be neglected.

DR. DAVID FERRIER has been appointed Professor of Forensic Medicine at King's College, London, *vice* W. A. Guy, M.B., resigned.

THE Secretary of State for India has appointed Mr. A. G. Greenhill, Fellow of St. John's College, Cambridge, Professor of Applied Mathematics at the Civil Engineering College, Cooper's Hill. Mr. Greenhill graduated as Second Wrangler in 1870, and was bracketed equal with the Senior Wrangler for the Smith's Prize; he also gained a Whitworth Scholarship while an undergraduate.

THE Radcliffe trustees at Oxford, anxious to aid one or more advanced students in the scientific study of preventative or curative medicine, offer 10*l.* a month, for three months, to a student of St. Bartholomew's, Guy's, or St. George's Hospitals, desirous of working for that time in Oxford. He will have opportunities of studying physics, chemistry, geology, the higher parts of biology, clinical and sanitary medicine. Candidates must be recommended on intimate personal knowledge by the Dean or secretary of their medical schools, and will not be submitted to an examination. The first election will be in the last week of February. There will be an election of another student in April.

A GENTLEMAN named Millard has bequeathed to the President and Fellows of Trinity College, Oxford, 8,000*l.* for the advancement of mathematical and general science.

THE University of St. Andrew's has conferred the degree of LL.D. on Mr. Archibald Cunningham Geikie, Professor of Mineralogy and Geology in Edinburgh University.

THE Royal Irish Academy have granted from the fund at their disposal for scientific research, the following:—50*l.* to C. R. C. Tichborne, for Researches on the Dissociation of Salts in hot solutions, and on the History of the Terebenes; 30*l.* to E. T. Hardman, for Chemico-Geological Researches; 25*l.* to Prof. R. S. Ball, for Researches in the Motion of Vortex Rings; 25*l.* to Prof. S. Downing, for Researches on the Motion of Water through Curved Tubes; 50*l.* to P. S. Abraham, for Biological Researches on the Coast of Madeira.

ROBERT PATTERSON, F.R.S., died at his residence, Belfast, on the 14th. He was born in April 1802. Educated at the Belfast Academy, in his early days he contemplated the Irish Bar as a profession, but finally devoted himself to mercantile pursuits. At a very early age he was an ardent student of Natural History, and in 1821 he joined with a few others to form the Natural History Society of Belfast. Among the first papers read before this Society were a series by Mr. Patterson on the insects mentioned by Shakespeare, which were afterwards published. His most important contribution to biological literature was, perhaps, his "Zoology for Schools," the first part of which appeared in 1846. This little work proved a great success. It was adopted by the Commissioners of Irish National Schools, and also by the Committee of Education in England, and most certainly gave a great impulse to the study of Zoology among the school classes of Great Britain. This led to the issue in 1853 of "Zoological Diagrams," large coloured plates which have proved of material assistance to both the teacher and the taught. He was a member of the British Association in its early days, and we believe that the daily printed "Journal of Proceedings" was an idea that originated with him. Of the different positions of honour and trust held by Mr. Patterson in his native town, we need not here speak. He was elected a member of the Royal Irish Academy in 1856, and a Fellow of the Royal Society in 1859. His genial and kindly presence will be missed by very many of his old and young friends.

Harper's Weekly notes the death of Mr. W. Harper Pease, at Honolulu, about the last of July, 1871. This gentleman was an American, born, we believe, in Pennsylvania, and was occupied for a long period in natural history pursuits. During the Mexican war he visited that country, under the protection of the American army, and made extensive collections of birds, which were deposited in the Academy of Natural Sciences at Philadelphia, among them some new species described by Mr. Cassin. About the year 1853 he visited the Sandwich Islands, and occupied himself for a time as a surveyor, and was sufficiently well pleased with the climate and country to remain there, marrying a native, and adapting himself to the customs of the people. During the whole of his residence in Polynesia he was engaged in studying the mollusca of the Sandwich Islands, and gradually extended his research to the species of all the Polynesian group, making collections either directly or through the medium of Mr. Garrett and others. Numerous communications from his pen upon Polynesian conchology have appeared in the *Journal de Conchologie* of Paris, the *Conchological Journal* of Philadelphia, the Proceedings of the Zoological Society of London, and elsewhere, and he has long been recognised as a thorough naturalist and reliable author. He had accumulated around him at Honolulu a very large library of conchological works, which, indeed, lacked few if any of the more important treatises. He enriched the principal cabinets of America and Europe by furnishing extensive collections, by which means he obtained, in part, the facilities for procuring the books needed for his investigations. He was for several years in ill health, and his death by consumption was not at all unexpected by his friends.

WE also learn from *Harper's Weekly* of the death in Reading, Pennsylvania, on December 26, 1871, of Mr. Charles Kessler, in the sixty-sixth year of his age. Mr. Kessler was known as an ardent and successful student of entomology, devoting himself to the lepidoptera, or butterflies, and bringing together a very large collection of insects of this order. We have not heard what disposition is to be made of this collection, but we presume it will ultimately come into the possession of some one of the natural history museums of the country.

PROF. WYVILLE THOMSON has been prevented from lecturing to his students for the past fourteen days, owing to a mild attack of continued fever. He hopes, however, to be able to begin again on Wednesday next. Dr. Christison has also been laid up for some days, owing to an attack of ephemeral fever.

WE learn from the *Academy* that the African traveller and botanist, Dr. Schweinfurth, has happily returned in safety to Europe, and though he has suffered the loss of the greater part of his invaluable collections and drawings, he has brought back a harvest of information and experience which places his journey among the most successful of modern times. After his great journey west of the Upper Nile, in the country of the Niam-Niam and Monbuttu, he made a short excursion from his headquarters, the Seriba Ghatta, westward to Kurkur and Danga, positions formerly visited by Petherick, and returning, planned a much more extended journey, when a fire broke out in the Seriba Ghatta on the 2nd of December, 1870, which not only destroyed the station, but with it the whole property of the traveller. Fortunately, a portion of his collection was at that time already on its way to Berlin. Provided with a few necessaries at Seriba Siber, the headquarters of the Egyptian troops, the indefatigable traveller made a tour in a part of Fertit hitherto unvisited by Europeans from December 1870 to February 1871, during which he found that the Bachr-el-Arab is unquestionably the main stream of the basin which mouths in the Nile at the Bachr-el-Ghazal. Having been deprived by the fire of every instrument by means of which any mechanical reckoning of the distances traversed during this journey could be made, the explorer, with an energy perhaps unexampled, set himself the task of counting each step taken, and in this way constructed a very satisfactory survey of his route.

Harper's Weekly announces the receipt of advices as late as the 5th of November from Mr. William H. Dall, whose return to Alaska under the auspices of the Coast Survey we have already chronicled. Mr. Dall is well known for the encyclopædic work published by him some time ago upon Alaska, the result of several years' residence in that region. His present position gives him unusual advantages for observation and research, and will doubtless be made the most of in gathering an important mass of

information. He is now stationed at Iliuliuk, in Unalaska, and engaged in surveying harbours and taking soundings, and generally in gathering such information as to the shores and their adjacent waters, the tides and currents, as will be to the interest of commerce and navigation. He is also using his opportunities in dredging for marine animals, and in making collections of natural history, of which he has already accumulated quite a number.

THE first contribution to science from the *Hassler* expedition, under Prof. Agassiz, appears in the form of a letter addressed to Prof. Peirce, dated St. Thomas, December 15. In this it is stated that, in the course of the frequent examination of the floating Gulf-weeds made daily, for the purpose of collecting the marine animals that usually inhabit them, a mass of this weed was found, the branches and leaves of which were united together by fine threads, wrapping it in every direction into the form of a ball. The threads forming the connecting material were elastic, and beaded at intervals; the beads being sometimes close together, sometimes more remote, a bunch of them occasionally hanging from the same cluster of the threads. From the accounts of the professor it would appear as if a globular mass had been formed by wrapping up a small quantity in the thread, and then adding more, and continually wrapping it up, until a ball of considerable size was produced. A careful examination of these beads showed that they were in reality the eggs contained in the substance of the threads, and in some the embryo was sufficiently far advanced to prove that they belonged to a fish. The mass was preserved and watched until some became detached and were free in the water; and by a very interesting process of critical investigation, the fish itself being too small for identification, it was ascertained, mainly through the structure of the pigment-cells, that they belonged to a small species, quite common in the Gulf Stream, known as *Chironectes pictus*. In this genus the pectoral fins are supported on arm-like appendages, giving them the power of hands; a somewhat similar structure in some allied forms enabling them, when thrown on the shore, to walk or crawl back leisurely into the water. It is somewhat remarkable that these eggs should have been found in the month of December, when the great majority of species lay their eggs in early spring. It is possible that *Chironectes pictus* may be an exception to the general rule. A scarcely less interesting peculiarity is seen in regard to the eggs of the goose-fish, or the common fishing-frog, of the Atlantic coast. This is an extremely hideous-looking species, shaped like a much-depressed tadpole, with an enormous head and huge mouth, and sometimes weighing from fifty to one hundred pounds. It is known to naturalists as *Lophius americanus*. The eggs of this species are contained in an immense flat sheet of mucus, sometimes thirty or forty feet long, and twelve to fifteen wide, which, when floating along the surface of the ocean, resembles nothing so much as a lady's brown veil. The mucus is so tenacious as to admit of being wrapped around an oar and dragged on board a vessel, but is extremely slippery, and readily escapes from one's grasp. The eggs, or embryos, are disseminated throughout this sheet at the rate of ten to twenty to the square inch, and by their brownish colour tend to give the impression just referred to. The number of eggs in one of these sheets is enormous, in some instances exceeding a million.

THE *Gardener's Chronicle* inquires whether the physicians or the lecturers on Botany at St. Thomas's Hospital and King's College Hospital, London, respectively, have been consulted as to the planting that has been lately carried on in the enclosures facing the buildings we have mentioned. We can hardly suppose that these gentlemen can have had any voice in the matter, since they must be too good physiologists not to know what must be the inevitable result of such operations. At St. Thomas's the expenditure for evergreen shrubs must have been very considerable. There are scores of such things as *Libocedrus decurrens*, *Cupressus Lawsoniana*, *Thujopsis borealis*, *Wellingtonia*, and the like, which are certain to die. The selection of evergreen shrubs for the Thames Embankment (north) is sufficiently unfortunate, but for reckless planting commend us to the Hospital of St. Thomas. At King's College Hospital the planting has been more modest, the victims consisting merely of cherry laurels. Surely we might have looked for a little common sense in such establishments as we have alluded to.

AT the Wisbech District Chamber of Agriculture, held on February 1st, some very interesting remarks were made by Mr. S. H. Miller, advocating the establishment of a County Agricultural Laboratory in which chemistry, botany, and agricultural meteorology might be prosecuted, in which young farmers might get a scientific training, and to which soils and manures might be sent for analysis. The warmth with which the proposal was received by those present augurs well for the manner in which subjects of this kind are now taking hold of the agricultural and commercial mind. We heartily commend the subject to the attention not only of Chambers of Agriculture, but of Chambers of Commerce throughout the country.

EQUALLY satisfactory was the reference made at the half-yearly meeting of the Scottish Meteorological Society, held on January 25th, by Mr. Milne Home and Mr. Melvin, to the extent to which this country is lagging behind in its endeavours to increase our knowledge in scientific agriculture. The following resolution was passed at the meeting:—"This meeting having had explained to it a scheme proposed by Commodore Maury, of America, for obtaining reports from all countries of the state of growing crops, and also of the weather in the districts where these crops are growing, so as to warrant correct estimates of these crops as regard both quantity and quality; and having learnt that an influential agricultural society in America has approved of the scheme, and applied to the United States Government to carry it out, and to invite the co-operation of the Governments of other countries, agree to express a general approval of the scheme, and remit to the Council to make a favourable answer to Commodore Maury's communication."

AT the recent annual *conversazione* of the Sheffield Literary and Philosophical Society, the annual address was delivered by Mr. H. C. Sorby, F.R.S., as president. Among the remarkable inventions of the year he referred especially to the honour done to the town by Mr. Earnshaw's new method of integrating partial differential equations, and to the invention of the Moncrieff gun-carriage, where, by a simple application of mechanical principles, the force of the recoil is utilised, and made instrumental in protecting the men and the gun, and employed to raise it into a position for the next shot.

THE pages of the "Public Ledger Almanack" are filled with far more sensible matter than usually finds its way into similar publications. We find articles on the atmosphere, on the various descriptions of weather signals, and on the United States Coast Survey.

M. QUETELET reprints a eulogium on the late Sir John Herschel, spoken before the Academy of Science of Brussels, of which he was an associate.

MR. FAIRGREVE, the proprietor of Wombwell's No. 1 Menagerie, is retiring from business, and is going to dispose of the stock. The horses requisite for the dragging of the vans were sold the other day in Edinburgh, and realised over 1,400*l*. This sum gives a slight insight into the large capital invested by the owners of menageries. It is not known what he is going to do with the animals. There is some talk of another Zoological Garden being formed in Edinburgh, but nothing definite.

AERIAL NAVIGATION IN FRANCE*

THERE has been a most interesting sitting at the Academy of Sciences, at which M. Dupuy de Lôme read a report on his newly tried and apparently successful system for steering air balloons. M. de Lôme is one of the most eminent—if he is not the most eminent—of living French engineers. He was the first to apply steam to ships of war, and he was one of the earliest designers of ironclad frigates. The piercing of a tunnel under the English Channel is another of M. Dupuy de Lôme's long-cherished projects, and he is one of the engineers who are about to commence that gigantic enterprise. During the siege of Paris by the Prussians, M. Dupuy de Lôme offered to construct a balloon which should have steering powers of its own, and so not be totally at the mercy of the winds. That some sort of guiding power was required for the balloons which were despatched from Paris during its investment by the Germans is shown by the fact that, out of sixty balloons sent out during that period, no less than fifteen failed to carry their contents to a place of safety, some falling into the sea and several into the hands of the Prussians. After much tiresome delay, M. Dupuy de Lôme's plans were accepted by the Government of National Defence, a credit of 40,000 francs (1,600*l.*) was opened for him, and he began to construct his balloon at the Palais de l'Industrie, in the Champs Elysées. So great was the difficulty, however, in constructing an immense balloon on a totally new system, in a city completely cut off from the rest of the civilised world, that M. Dupuy de Lôme's huge machine was not ready until just four days before the capitulation. When that event took place, the balloon had to be packed up and hidden away from the prying eyes of the Germans when they partially occupied Paris. Then came the Commune, and all the disorganisation which followed. It was only after much difficulty that M. Dupuy de Lôme obtained permission to make use of the buildings of the Fort Neuf at Vincennes, whence, on the 2nd inst., he started on his trial trip. Before proceeding to quote from M. Dupuy de Lôme's most interesting report, it may be as well to say a few words as to the end which the eminent aëronaut has proposed to himself. He does not pretend to be able to make independent progress in the teeth of the wind, but only to deviate from the direct set of the wind when running before it. He does not hope ever to be able to beat to windward, but only to tack to right or left with the wind. A sailor would say that M. Dupuy de Lôme wanted to be always running free with the wind on the quarter. So if the wind set straight from Paris to Brussels, an ordinary balloon could only land at some point between Paris and Brussels, or else beyond the Belgian capital. But with a balloon constructed on M. Dupuy de Lôme's system, the aëronaut might steer his course either on the port or starboard tack, and might descend at London or Cologne, as he saw fit.

Having said this much, let me try to describe the balloon which M. Dupuy de Lôme makes use of. Let your readers imagine a gigantic egg of inflated silk, the longer axis being horizontal; to this egg is attached an oblong car, something the shape of a punt. The motive of the inventor in choosing the ovoid form was at once to obtain greater stability for the car than could be hoped for with the old balloons, and at the same time to give the least possible hold to the wind. The diameter of the balloon is about two-fifths of its horizontal length from point to point. I take the following dimensions from M. Dupuy de Lôme's highly interesting report, read before the Academy of Sciences, only changing French metres into feet for the convenience of English readers.

Total length from end to end	118 ft. 6 in.
Diameter at the point of greatest circumference	49 ft. 2 in.
Diameter of the screw	29 ft. 6 in.
Number of blades	2
Number of turns of the screw in a minute, when the balloon is going eight kilometres (five miles) an hour faster than the wind	21

M. Dupuy de Lôme thus describes the rudder by which his balloon is steered :—"The rudder is a plain triangular surface. It is made of unvarnished calico, and is kept in its place by a horizontal yard six metres long at its lower extremity. It can turn easily on its forward extremity. The height of the rudder is five metres, and it has a superfices of fifteen metres." The car is next described—it is of wicker work, and of sufficient size to contain comfortably the windlass for the screw, and eight men to work it; the ventilator with which to manage the small balloon—we shall have to speak of this presently—and the man who attends to it. In all, fourteen persons can be carried in the car. The driving screw is directly carried by the car. The shaft of the screw is a hollow steel tube. This shaft is constructed so as to allow of the screw being easily dismounted when a landing is effected. The rudder is fixed to the balloon itself, and the screw, as we said, is below it, and immediately attached to the car. Two blades only are used in the screw instead of four, because when the ground is touched the two blades can be placed horizontally. so as to escape injury. Were there four blades, the screw would be almost certain to be broken whenever a landing was effected. The windlass which turns the screw is worked by four, or, if necessary, eight men, in a similar manner to the steering wheel of a ship—only the wheel is placed parallel to the axis of the car, instead of at right angles to it, in order to lessen the rolling occasioned by the movements of the men working the windlass. The material of which the envelope of the balloon is composed is white silk, weighing 52 grammes, not quite 2 oz. to the square metre, and a coarser lining weighing 40 grammes the square metre, and seven coats of india-rubber, which together weigh 180 grammes, a little over 6 oz. the square metre. Thus the whole weight of the external web of the balloon is 272 grammes, about 9 oz. to the square metre. In order to render the web of the balloon totally impermeable to the hydrogen gas with which it is inflated, the silk was painted over with a sort of gelatinous compound, invented by M. Dupuy de Lôme. The total weight of the two balloons when ready to start was 570 kilogrammes, or rather more than half a ton. The web of the balloon was reckoned to be capable of supporting a pressure of over 2,000 pounds to the square yard. I have mentioned the smaller balloon; this is, more correctly speaking, only a division as it were of the larger balloon. It is formed by means of an inner skin, separating the bottom of the balloon from the rest. This compartment occupies about one-tenth of the whole cubic space of the balloon, and serves to keep it stiff, and of the required shape. By these means M. Dupuy de Lôme has attained the two ends he proposed to himself, viz., first, permanence in the shape of the balloon; and, secondly, he has been able to give the whole apparatus an axis decidedly parallel to that of the force of propulsion.

Having thus endeavoured to give some account of the new aerial navigator—no easy matter without diagrams—it only remains for us to say a few words about M. Dupuy de Lôme's first experimental trip. There was half a gale of wind blowing at the time he started, and the screw had been slightly damaged. The spirited inventor did not hesitate, however, to make his contemplated ascent. The end justified his confidence; for not only was he able to land near Noyon, in the Department of the Oise, some seventy miles north-east of Paris, but his balloon more than answered his expectations. The screw, when worked by four men, drove the balloon eight kilometres (about five miles) an hour quicker than the rate at which the wind was blowing; so that M. Dupuy de Lôme not only "went like the wind," but actually went faster than the wind. By the use of the rudder the course of the balloon could be altered eleven degrees either way from the set of the wind, making a total deviation of twenty-two degrees. This is, of course, the greatest and most noteworthy result obtained by the new aerial machine. It may possibly be asked, What is the use of the screw when the wind carries your balloon at the rate of fifty-four kilometres, or nearly forty miles an hour? The answer is, that without the screw the rudder would be of little or no use. Every one knows that a ship without way on her steerage-way, as it is called, is nearly impossible to steer. And a balloon which has not, like a ship, a second element for the rudder to work on, is still more at the mercy of the wind. The next question is whether the screw cannot be turned by steam instead of by manual labour. But fire and hydrogen gas are bad neighbours, and the introduction of a steam-engine into the car—although it was hazarded some twenty years ago by one of our countrymen, Mr. Henry Giffard—would expose the aeronauts to the dangers of an explosion, followed by a descent to the earth, doubling in rapidity every sixteen feet, in accordance with the law of gravitation. Even with a steam-engine on board, there does not seem much cause to fear the "airy navies" of the inventor of ironclad ships just at present.

* Reprinted from the *Daily News*.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, February 15.—Mr. G. Bentham, F.R.S., president, in the chair.—Prof. Wyville Thomson, F.R.S., Prof. Allman, F.R.S., and Prof. W. T. Thiselton-Dyer were elected Fellows.—"On the Habits, Structure, and Relations of the Three-banded Armadillo," by Dr. J. Murie. This animal is distinguished from the other members of the order Edentata by its habit of rolling itself into a ball like a hedgehog. The three bands act as hinges, by means of which this rolling-up is effected. It is also peculiar in walking on the points of its toes, instead of, like other armadillos, on the whole foot. It may be considered as a connecting link from the armadillo to the extinct glyptodon, and thence to the megatherium, and so on to the pachyderms.—"On a Chinese Artichoke-Gall," by A. Müller.—"Comparative Geographical Distribution of Butterflies and Birds," by W. F. Kirby. The total number of species of birds is stated by Dr. Sclater as 7,500, and that of butterflies is about 7,700, showing a remarkable closeness. If the surface of the globe is marked off into the divisions proposed by Dr. Sclater, we find in the Palæarctic region (Northern Europe and Asia), including about 14,000,000 square miles, 630 species of butterflies and 630 of birds; in the Indian region, including Asia south of the Himalayas, about 1,200 butterflies and 1,500 birds; in the Australian region 725 butterflies and 1,000 birds; in the Nearctic or North American region, 480 butterflies and 660 birds; in the Neotropical or South American region, 4,200 butterflies and 2,250 birds; thus, in five divisions there is a preponderance of birds, which is balanced by a very large excess of butterflies in the sixth region.—An interesting discussion followed, in which Mr. A. R. Wallace, Mr. Sharpe, Mr. Stainton, and others took part, and it was shown that if Dr. Gray's estimate of the number of species of birds is taken, viz., 10,000, which is no doubt more correct than Dr. Sclater's, the apparent parallelism vanishes; that in limited districts, as the British Isles, there is no resemblance between the number of butterflies and of birds; that in Mr. Kirby's paper no reference is made to the number of birds in each region that are migratory, a most important consideration; and that the conditions of the natural features of the country, as the prevalence of forests, may be favourable to the abundance of insects, and unfavourable to that of birds.

Chemical Society, February 15.—Dr. Frankland, F.R.S., president, in the chair.—Prof. Roscoe, F.R.S., gave an account of some of his recent researches on the element tungsten, under the title "On the study of some tungsten compounds." The author, after giving a short *resumé* of the labours of other chemists on those compounds of tungsten which he had been investigating, proceeded to describe their properties, and the methods of preparation he had employed to obtain them. As the result of his labours he has definitely settled that the metal tungsten is a monad element with the atomic weight 184, and has also showed the cause of the error of the French chemist Persoz, who assigned 153 as the atomic weight. A collection of very fine specimens of tungsten compounds was exhibited by the Professor.

Royal Geographical Society, February 12.—Sir H. C. Rawlinson, K.C.B., president, in the chair. The President announced that the expedition for the search and relief of Dr. Livingstone left England on Friday last, and was at that moment probably crossing the Bay of Biscay *en route* for Zanzibar. The subscriptions from all sources, including the balance of the Government grant lying at Zanzibar, amounted to nearly 5,000*l*. Of this sum about 2,800*l*. will have been expended by the time the expedition leaves Zanzibar for the interior; the remainder would be held in reserve for contingencies very likely to occur. He read also to the meeting a letter from Earl Granville to the Sultan of Zanzibar, stating the great interest the Government and people of England took in Dr. Livingstone, and recommending the expedition organised by the Royal Geographical Society of England to his Highness's good offices; and another to Dr. Kirk, Acting Consul at Zanzibar, authorising him to apply 654*l*., the balance of the Treasury grant of 1870, to the purposes of the expedition. So far everything connected with the expedition had been most satisfactorily and expeditiously carried out; and a message ordering the preparation of escort and porters at Zanzibar, sent as far as Aden by telegraph, would reach Zanzibar in the unprecedentedly quick space of fourteen days. Letters had been received from Dr. Kirk of so recent a date as Dec. 16, and they informed us that no news whatever had been received since September from the interior, but that the war between the Arabs and the people of Unyamwezi would be continued. This would necessitate the adoption of an entirely new route by the expedition now on its way.—Letters were then read concerning Sir Samuel Baker's expedition. The President stated that he had received from the Prince of Wales the original letters of Sir Samuel, copies of which his Royal Highness had sent to the *Times*. A letter, three days later in date, contained the news that a fertile portion of the Bari territory beyond Gondokoro had been acquired, and that Lieut. Baker would have charge of the steamer for the navigation of Lake Albert Nyanza.—A paper was then read by Sir Harry Parkes (British Minister at Japan), entitled "Captain Blakiston's Journey round the Island of Yezo." Sir Harry explained that his office with regard to the paper was that of reducing into readable bulk the voluminous journals which Captain Blakiston had communicated through him to the Society, and of adding some necessary explanations. Yezo was the northernmost island of Japan, larger by 3,000 square miles than Ireland, and rising in importance from its position and its great fertility and mineral wealth. Captain Blakiston, the well-known explorer of the Yang-tsze-Kiang, since resident in Hakodadi in the south of Yezo, had enjoyed the peculiar advantage of travelling with the privileges of a Japanese official. He went by sea to Akis Bay, on the south-east coast, and thence by land almost entirely along the sea coast (the interior being without roads or Japanese settlements) round the island to Hakodadi. The native inhabitants are the singular isolated people called Hairy-men, or "Ainos," a robust race, apparently of Aryan extraction, and nearest allied to certain sections of Sclavonians, distinguished by the thick growth of hair on the body, as well as head and beard.

Photographic Society, February 13.—The officers and council for the ensuing year were elected, and the accounts of the society explained by the treasurer, who reported the society free from debt and with a satisfactory balance in hand. The report of the council was read and adopted.—Dr. Anthony read a paper "On various modes of Plate-cleaning." He stated that his experience went to show that the employment of cyanide of potassium was better than any other agent for the purpose, the plates being treated for a very brief period in the cyanide solution and then washed in water. He found mechanical methods generally rendered the bath unclean, and for this reason also deprecated the application to the glass plate of an albumen substratum. The specimens of Niepce de St. Victor were exhibited.

EDINBURGH

Royal Society, February 19.—Sir Alexander Grant, Bart., vice-president, in the chair.—1. "Remarks on Contact-Electricity," by Sir William Thomson. 2. "On the Curves of the Genital Passage as regulating the movements of the Fœtus under the influence of the Resultant of the Forces of Parturition," by Dr. J. Matthews Duncan. 3. "On a Method of Measuring the Explosive Power of Gaseous Combinations," by James Dewar. 4. "Note on Modification of Sprengel's Mercurial Air-Pump," by James Dewar. 5. Prof. Alexander Dickson exhibited a series of Abnormal Fir Cones, with remarks.

PARIS

Academy of Sciences, February 12.—MM. Delaunay and Serret protested against the insertion in the *Comptes Rendus* of a note by M. Renou relating to asserted inaccuracies in the publications of the Paris Observatory.—The controversy on fermentation and heterogeny was continued by M. Pasteur reading a reply to M. Fremy, and M. Chevreul a communication on the history of ferments after Van Helmont. M. Engel also presented a morphological investigation of the various kinds of alcoholic ferments, which he describes as forming two genera, *Saccharomyces* (Meyen) with seven species, and *Carpozyma* (*gen. nov.*) with one species. The characters of these forms were illustrated with outline figures.—M. Bertrand presented the solution of an arithmetical question by M. Bougaev; M. Serret a note by M. E. Combescure on some points in the inverse differential calculus; and a note by M. A. Mannheim on the determination of the geometrical connection which exists between the elements of the curvature of the surface of the principal centres of curvature of a given surface.—M. de Saint-Venant presented an elaborate report upon a memoir by M. Kleitz, entitled, "Researches upon the molecular forces in liquids in motion, and their application to hydrodynamics."—M. de Pambour read a note on the theory of hydraulic

wheels, relating to the reaction-wheel.—M. Saint-Venant communicated a note by M. Boussinesq on the equation of the partial derivatives of the velocities in a homogeneous and ductile solid undergoing deformation parallel to a plane.—M. Serret presented a note by M. de Tastes in reply to a recent note by M. Ciotti on the employment of vibrating elastic plates as a means of propulsion. M. de Tastes stated that the elastic plate propeller is his invention, communicated by him to M. E. Ciotti.—M. E. Dubois presented a reply to M. Ledieu's objections to the employment of the marine gyroscope.—M. Delaunay presented a note by M. C. Wolf on the reflecting power of mirrors of silvered glass, and their application to astronomical purposes.—A note by M. D. Genez on the absorption-bands produced in the spectrum by solutions of hyponitrous, hypochlorous, and chlorous acids, was communicated by M. H Sainte-Claire Deville.—A note by M. Baudrimont on the recent experiments of M. Poëy with regard to the influence of violet light upon vegetation was read, in which the author stated that he had arrived at totally different results, having found that violet light was fatal to vegetation.—A great number of communications from all parts of France, and also from Belgium, Switzerland, and Algeria, relating to the aurora of February 4, were laid before the Academy; they included notices of magnetic disturbances observed in the telegraphic lines.—M. Delaunay presented a paper by M. E. Stephan containing a list of nebulæ discovered and observed at the Observatory of Marseilles.—M. E. Vicaire read a reply to Father Secchi's observations on the temperature of the solar surface.—Some remarks were read by M. Harting on the saccharine matter observed by M. Boussingault on lime trees, which he ascribed to the action of aphides in accordance with the commonly received opinion. He stated that the saccharine secretion produced by those insects consists in great part of cane sugar. M. Boussingault in reply said that in the case observed by him the saccharine exudation appeared before the aphides, and that it contained cane-sugar, grape-sugar, and dextrine.—M. Le Verrier also read an extract of a letter from M. Follie on this subject.—M. Bussy presented a report upon a memoir by M. Louvel, describing a process for preserving grain *in vacuo*. The author suggested storing grain in air-tight granaries, in which a partial vacuum may be produced by a powerful air pump; he described the construction of the apparatus, and stated that a granary such as he proposed of the capacity of ten cubic metres (about 370 cubic feet) and containing 100 hectolitres of wheat would cost 750 francs. He stated that by this process the ravages of insects are effectually stopped.

BOOKS RECEIVED

ENGLISH.—The Origin of Species, 6th edition: C. Darwin (Murray).—Transactions of the Society of Biblical Archæology, Vol. i., Part 1. (Longmans).—Index of Spectra: W. M. Watts (H. Gillman).—Recollections of Past Life: Sir H. Holland (Longmans).—New Theory of the Figure of the Earth: W. Ogilby (Longmans).

PAMPHLETS RECEIVED

ENGLISH.—Eighth Annual Report of the Belfast Naturalists' Field Club for 1871.—Italy in England.—Five Speeches on the Liquor Traffic: G. O. Trevelyan.—Description of a new Anemometer: J. E H. Gordon.—Psychic Force and Modern Spiritualism: W. Crookes.—On the Mechanism of Accommodation for Near and Distant Vision: Dr. R E. Dudgeon.—Address of Thos. Hawkesley on his Election as President of the Institution of Civil Engineers.—The Reflecting Media of the Atmosphere a Natural Law; J. Shaw.—Preliminary Report of the Scientific Exploration of the Deep Sea in H.M. surveying vessel *Porcupine*.—Report of the Ladies' National Association for the Repeal of the Contagious Diseases Act.—Contributions to the Flora of Berkshire: Jas. Britten.—A Grave Question for Englishwomen.—What is the Shape of the Earth: Scævola.—On the Elevation of Mountains by Lateral Pressure: Rev. O. Fisher.—Meteorology of West Cornwall and Scilly, 1871.—Journal of the Iron and Steel Institute, Jan. 1872.—On Teaching Universities and Examining Boards.—Child's Public Ledger Almanac, 1872.—Every Saturday, No. 1.—Pauperism and Crime: Robert Hill.—The Mining Magazine and Review, No. 2.—The Quarterly Journal of Education, Jan. 1872.—Righthandedness: D. Wilson.—Address at the Anniversary Meeting of the Entomological Society: A. R. Wallace.—Proceedings of the Geologist's Association, Oct. 1871.—The National Church, No. 1.—The Scottish Naturalist, No. 5.

AMERICAN AND COLONIAL.—Lippincott's Magazine for Jan. 1872.—Australian Vertebrata, Fossil and Recent Mammals: G. Krefft.—Catalogue of the Meteoric Collection of C. U. Shepard.—Proceedings of the Asiatic Society of Bengal, 1871, Nos. 10, 11.—Appleton's Journal, No. 143.—Proceedings of the Academy of Natural Sciences of Philadelphia, April-Sept. 1871.—A Letter concerning the Deep-Sea Dredging, addressed to Prof. B. Peirce by L. Agassiz.—Annual Report of the Secretary of the Interior for the year ending Oct. 1871.—Report on the Geological Structure of Prince Edward's Island: Prof. Dawson.—Nitro-Glycerine, as used in the Construction of the Hoosac Tunnel: G. Mowbray.—Cruise of the School-ship *Mercury* in the tropical Atlantic Ocean.—Correspondence relative to Deep-Sea Dredging.—The Indian Antiquary, No 1: Edited by Jas. Burgess.—Monthly Notices of the Meteorological Society of Mauritius.—The School Laboratory of Physical Science, Nos. 3 and 4: G. Hinrichs.

FOREIGN.—La Belgique Horticole, Dec. 1871–Feb. 1872.—Bulletin de l'Académie Royale des Sciences de Belgique, No. 12, 1871.—Verhandlungen der k. k. geologischen Reichsanstalt zu Wien, No. 16, and No. 1, 1872.—Anzeigen der k. Akademie der Wiss. math.-naturforsch. Classe, 1871, No. 1–29.—Bulletin de la Société d'Anthropologie de Paris, June and July, 1870.—Sitzungsberichte Isis in Dresden, July-Sept. 1871.—Die geographischen Verbreitung der Coniferen u. Gnetaceen: R. Brown.—Zeitschrift für Ethnologie, Heft 2.—Journal général de l'imprimerie.—Notice sur Sir J. F. W. Herschel: Ad Quetelet.—Jahrbuch der k. k. geologischen Reichsanstalt zu Wien.—Memoire della Societa dei spectroscopisti Italiani, No. 1.—Un expérience relative à la question de vapeur vésiculaire: F. Plateau.—Récherches expérimentales sur la position du centre de gravité chez les insectes: F. Plateau.—Annali di Chimica, No. 1, 1872.

DIARY

THURSDAY, FEBRUARY 22.

ROYAL SOCIETY, at 8.30.—On a New Hygrometer: W. Whitehouse.—On the Contact of Surfaces: W. Spottiswoode.

SOCIETY OF ANTIQUARIES, at 8.30.—The Roman Villa at Holcombe: Capt. Swann, F.S.A.—The Kirkham Chantry, Paignton, Devon: Sir W. Tite.

LONDON INSTITUTION, at 7.30.—On South Africa and its Diamond Fields: Prof. T. R. Jones, F.G.S.

FRIDAY, FEBRUARY 23.

ROYAL INSTITUTION, at 9.—On Social Influence of Music: Mr. H. Leslie.

QUEKETT MICROSCOPICAL CLUB, at 8.

SATURDAY, FEBRUARY 24.

ROYAL INSTITUTION, at 3.—On the Theatre in Shakespeare's Time: Wm. B. Donne.

SUNDAY, FEBRUARY 25.

SUNDAY LECTURE SOCIETY, at 4.—On the Education of Women: Mrs. Fawcett.

MONDAY, FEBRUARY 26.

GEOGRAPHICAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 4.—Elementary Chemistry: Prof. Odling, F.R.S.

TUESDAY, FEBRUARY 27.

ROYAL INSTITUTION, at 3.—On the Circulatory and Nervous Systems: Dr. Rutherford.

WEDNESDAY, FEBRUARY 28.

SOCIETY OF ARTS, at 8.30.

THURSDAY, FEBRUARY 29

ROYAL SOCIETY, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

ROYAL INSTITUTION, at 3.—On the Chemistry of Alkalies and Alkali Manufacture; Prof. Odling, F.R.S.

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NOTICE

We have received a letter signed "M.," which we hold over till informed (in confidence) of the name and address of the writer. Anonymous communications can in no case receive attention.