

THURSDAY, MAY 24, 1883

SCIENCE AND ART<sup>1</sup>

WE stated in our article last week that we should take an early opportunity of noticing some of the pictures in this year's Academy, with especial reference to the points which we then mentioned. The following notes are a fulfilment of that promise. It is to be understood that only those pictures which illustrate, either by their perfection or their defects, the points in question have been referred to. The pictures have been classified according to the particular class of natural phenomena which they portray.

## SKY COLOUR

2. "Homewards," Wm. J. Monkhouse Rowe. Sky not zoned; impossible colour of clouds. Perhaps our word zoned requires some little explanation. When the sun is at such an altitude above the horizon that the blue rays are absorbed, and there begins to be colour on cloud and sky, then the sky at the same altitude is always of the same colour, and the colours gradually modulate from the warmest at bottom to the coolest at top. These remarks with regard to the sky have, of course, nothing whatever to do with the clouds, but whatever the colour of the clouds may be, and this will depend upon various conditions, any true clouds, however they may be coloured, shown on the picture at the same altitude, will be surrounded by the same sky colour, even if the intensity differs in consequence of different distances from the sun's place. For instance, if a painter chooses to put a bright green sky at 5° or 10° elevation on the right of his picture, and then paints a blue sky at the same elevation above the horizon on the left, he is showing something which is impossible: his picture is not perfectly zoned.

76. "Welbourne Hall, Yorkshire," H. A. Olivier. Sky well zoned, but its reflection from water too intense. Reflection will naturally lower the tone of the reflected light, but, all the same, this difference may not come out very strongly, for the reason that the reflection is most frequently and necessarily shown in a darker part of the picture, so that although by mere contrast the tone seems lowered, it will yet appear to be brighter than the tone seen in the region of more general illumination.

132. "To Pastures New," James Guthrie. Sky of impossible colour.

121. "Freshening," A. Harvey Moore. Sky and sea both admirable.

157. "Corrie, Isle of Arran," John MacWhirter, A. Sky admirable; water a little doubtful. It is also not level.

218. "The Dogana and the Island of San Giorgio, Venice," Frank Dillon. Sky colour and water reflection good.

233. "After Sundown," Francis R. Binns. Excellent in sky colour, but zoning might have been more perfect.

242. "Superstition," Everton Sainsbury. Good and bold sky colour, especially the red, but the sky is too much worked to resemble cloud.

246. "Autumn," A. Glendening, jun. Good, and forms of clouds quite excellent.

<sup>1</sup> Continued from p. 51.

247. "The Forgotten Sheaf," F. S. Walker. Zoning gone wrong; green never rests on white, nor on gray.

269. "And the Unclean Spirits went out of the Swine," Briton Riviere, R.A. Bold and perfect sky and clouds.

297. "Windsor," Vicat Cole, R.A. Might have been more evenly zoned.

315. "A Mortally Wounded Bandit Chief Exhorting his Comrades to Return to an Honest Living," J. R. Herbert, R.A. There is no relation between the light, the colour of the sky, and that of the landscape.

327. "Grouse-driving on Bowes Moor, Yorkshire," George Earl. It is difficult to understand by what means the sky to the left is illuminated.

331. "Carting for Farmer Pengelly," J. C. Hook, R.A. Sky and clouds admirable; green on the cliff very striking.

371. "A Silent Pool," Fred. E. Bodkin. A slight change in the colour of the clouds would make this an admirable picture.

394. "November," E. A. Walton. Let us hope this is not true sky colour. Far too deep a green, and there is no reason why the clouds at the top of the picture should not be as intense in their lower portions as the mass of cumulus on the horizon.

398. "Ben Ray," H. W. B. Davis, R.A. There is not sufficient relation between the colour of the sky and the colour of the landscape. (Compare 702.)

700. "Trabacolo Unloading at the Custom House, Venice," Clara Montalba. The sky colour is wrong. There could have been no green where the artist has placed it.

702. "At Kinlochewe," H. W. B. Davis, R.A. (compare 398). In this case the sky is wedded to the landscape, and we have a perfect and harmonious whole beautifully luminous.

713. "A Summer Evening, Folkestone," W. Ayerst Ingram. Nearly perfect zoning of cloudless sky, but the reflection from the water has been a little too much toned down perhaps.

773. "Winnowing Gleanings," H. Gillard Glindoni. Sky and seascape both admirable.

826. "The Boundary of the Heath," J. C. Harrison. Careful study of sky. Bank of trees against it very effective.

793. "Rochester from the River," Charlie W. Wyllie. A pleasing picture—both sky and water good.

1438. "Leaving Labour," E. B. Stanley Montefiore. Impossible green sky.

1503. "Lost Sheep," Robert Page. It is a pity this artist takes the trouble to paint a sky, because it is evident he does not know the difference between sky and clouds.

1483. "A Spanish Aqueduct," Adrian Stokes. Note colour of sky and landscape and effect of heat under tropical sun.

## CLOUDS

225. "On Solway Sands," Thomas Hope M'Lachlan. Blue clouds.

257. "Still Waters run Deep," George Chester. The clouds in this picture are hideous in form and impossible in colour.

339. "Night into Day," Vincent P. Yglesais. This may be a view in Mars. It is fortunately impossible here.

577. "Rye, Sussex," Leslie Thompson. A new kind



of cloud is here represented, one resembling mashed potatoes.

602. "A Calm—Bay of Naples," F. W. Jackson. Good study.

1461. "Between the Showers," Henry Moore. Good study of clouds.

#### DISTANCE AND ATMOSPHERE

279. "Gathering the Flock," H. W. B. Davis, R.A. Perfect distance, toning carefully preserved, and the picture is free from the exaggeration which one so often laments. The illumination of the sheep is, however, too local.

321. "Highlands and Lowlands," William Linnell. Admirably managed distance.

479. "Light in the West: After Rain," Alfred W. Williams. The artist seems to have too strongly contrasted the peaks against the sky. The peaks though high are really distant, and hence there is atmosphere between us and them.

96. "Snowdon," Joseph Knight. Everything that could be desired.

255. "Llyn-yr-Adar on the Adder's Pool, Carnarvonshire," J. W. Oakes, A. This is the picture referred to at length in the preceding article. We repeat that it is a pity the author did not study the rainbow before he attempted to paint it.

843. "Spring Time at Tillietudlam Castle, Lanarkshire," David Murray. The effect of sunshine on grass is here carefully rendered, but there is a little too much colour in the delicate clouds in the centre of the picture.

#### SUNSETS

98. "Parting Day," B. W. Leader, A. Careful study, rifts admirably attempted, but the treatment of them is not quite perfect, especially on the right of the picture. As a rule the clouds must be lower down than they are here represented to give the effect sought to be rendered.

*Note on Rifts.*—These rifts, which have attracted the attention of Mr. Leader, are only possible when the air is very densely charged with aqueous vapour, for the reason that they are a projection upon the distant sky of a cylinder, less illuminated than the surrounding air, owing to the interposition of a cloud low down in the atmosphere. Now, as whatever the condition of the air may be it must obviously get more dense as the earth is approached; the lower the cloud the stronger will be the rift, and the more the cylinder of shadow is directed to the point overhead the more definite will be the rift, for the reason that along the line of sight the greatest distance will then be in shadow. We are really in such a case dealing with a partial eclipse of a long column of air, and as at any one place the conditions of the atmosphere at the same time will be almost, if not quite, identical, if rifts produced by clouds are shown on one side of the picture, the other side of the picture should show rifts produced in like manner. This we think is a point which Mr. Leader has very pardonably missed. The reflection of the clouds in the water is not quite true.

164. "Sunset Fires," John MacWhirter, A. This picture is spoiled because the artist has made no distinction between the colours of the sky and of the clouds.

399. "At Last!" Fred. C. Cotman. Sunset and water reflection; a beautiful picture.

1471. "An Autumn Evening," B. W. Leader, A. This is a very fair sunset sky, but a little too cool in colour, and the reflection in the water is not good. There is however a careful bit of painting in the way in which the top of the cumulus is reflected over the bridge.

#### MOONS, &c.

214. "Tipt with Eve's Latest Gleam of Burning Red," James S. Hill. It is quite impossible that such a moon should be at such a height at sunset, besides which the moon is more shapeless than she should look under the given cloud conditions.

232. "Too Late," Frank Dicksee, A. Everything about this picture when we leave the figures, with which we are not at present concerned, is wrong. We have an impossible moon in an impossible sky. The artist has attempted to paint the old moon in the new moon's arms, one of the most beautiful natural phenomena visible after sunset, but by a strange fatality almost every point where science could have assisted the artist has been neglected. As is known to many children, the appearance of the complete body of the moon on such an occasion as this, when only a very thin crescent is illuminated by the sun, depends upon the fact that the earth reflects light to the moon, hence the term "earth shine," the equivalent of "la lumière cendrée" of the French. Under these conditions we have in fact a thin crescent illuminated by direct sunlight, whilst the rest of the moon is illuminated by light reflected from the earth. One of the points of this earth illumination which Mr. Dicksee has entirely missed is this, that the earth-light must be equally distributed over the whole surface of the moon which it illuminates. Sunlight reflected from the earth to the moon, and then reflected back again from the moon to the earth, must be quite general in its action, and must equally light up each part of the lunar surface. Hence the absolutely equable illumination which is always seen, but which this picture fails to show. The fact that the old moon thus illuminated appears to rest in the new moon's arms depends upon irradiation, by the operation of which a thing very brilliantly illuminated looks larger than when it is dimly illuminated. That part of the moon, therefore, which shines by the brilliant light of the sun, appears to belong to a larger body than that part which receives its less brilliant illumination from the earth. This appearance is so obvious that it has given rise to the old world illustrations, in which the "old" moon is represented as in a boat, because in fact the two horns of the crescent moon extend beyond the old moon as we have said, and appear to form part of a larger circle. This point also Mr. Dicksee has entirely missed. What we have to say touching the colour of the sky is, that neither Mr. Dicksee, nor any one else, ever saw such a colour as he has painted to indicate the place of sunset. At such a height above the horizon a green colour is impossible, it must either be red, or yellow, or grey, according to the state of the atmosphere at the time. We have ventured to speak thus at length with reference to this picture, because we consider it a very typical case, and surely Mr. Dicksee, when he becomes acquainted with the facts to which we have drawn attention, and as to which there can, we believe, be no dispute, will regret that he should have disfigured his



picture by disregarding them. Had these things been correctly painted the picture would have been just as beautiful to the ignorant as it is at present, whilst it would have had the additional advantage of being also pleasing to look upon by those who can in its present form only regard it with regret. It may be said that these are simple matters. Be it so. Simple or not they are typical, and that is the point we wish to urge.

260. "The Ides of March," E. J. Poynter, R.A. The comet is admirably rendered, but does not the little lamp give out rather too much light?

697. "The Dawn of Night," Richard Whatly West. It is not easy to understand this picture, as the title of it is "The Dawn of Night"; but if we have the moon rising then the sky is far too dark.

807. "Can He Forget?" Edward H. Fahey. This artist is to be congratulated on his moon. He has painted it so that any one with a very simple calculation can determine that he is quite wrong. If the young lady asking the interesting question had had a crown in her hand, the coin would have covered the real moon. She herself would hardly cover the false one. So, to one who knows, it looks like Nadar's balloon without the netting being inflated. Further, each part of the moon when she rises is generally under the same atmospheric conditions, so that such a variation in its illumination as is here shown is almost impossible unless a definite cloud is darkening its surface, and no such cloud is here to be seen.

888. "Moonlight," Robert Jobling. It would have been better if in this moonlight scene the artist had discriminated more between the clouds and their background the sky.

546. "Moonlight Bay, Milford Haven," F. W. Meyer. Very careful study of moonlight.

#### WATER

28. "Catching a Mermaid," J. C. Hook, R.A. Colour and forms of waves, and swirl and dash on rocks, good.

133. "Wind against Tide: Rillage Point, Ilfracombe," J. Geo. Naish. Sea and sky excellent in the distance; water a little weak in foreground, both as to colour and forms of waves.

202. "Oyster Dredgers," C. Napier Henry. Water in this picture is not level.

281. "High Tide at Kynance, Cornwall," Sidney R. Percy. The wave at the left is being forced back by nothing, and is altogether too solid.

282. "A Rising Gale: Dunbar Sands, Padstow, Cornwall," Walter J. Shaw. Very bold low front view of breaking waves. Reflection good.

301. "The Gull Rock: off Kynance Cove, Cornwall," Edmund Gill. Colour and form of water quite admirable.

467. "A Travelling Cobbler," Joseph Henderson. Colour of water very true to nature. The distant land deserved more careful painting.

498. "The Last of the Crew," Briton Riviere, R.A. This ice is a careful study of form and colour.

495. "A Fisherman's Garden," Theodore Hines. Water not level.

711. "The Sad Sea Wave," John Francis Faed. Colour of water very brilliant.

695. "Lobster Fishers," Colin Hunter. Form of waves very careful study.

778. "A Haven," C. E. Holloway. Note colour of

water and form of waves. The artist should say where this Haven is, that it may be avoided by all who love the beautiful.

809. "Welsh Dragons," John Brett, A. Note colour of water and rocks. Mr. Brett is again quite perfect in his treatment, but we rather doubt the colour of some of the cumuli that float over the sea, and also their sharp darkened boundaries.

145. "Adrift," R. C. Leslie. An admirable study of water, far too good to be skied.

#### REFLECTION

36. "Love Lightens Toil," J. C. Hook, R.A. The reflection from the water has not been carefully studied. Does not the green of the grass come too low down to the water? The water, too, is not level. Current indicated.

86. "A Quiet Noon," Peter Graham, R.A. The reflection of the clouds from water is not quite in accordance with their form; the clouds themselves are admirable.

83. "The Enchanted Lake," Albert Goodwin. Careful study of reflection. The artist has left out what most artists would incontinently have put in, but there are several blunders; for instance, it is the under side of the umbrella which should have been reflected, and not the upper one, and the colours of the objects reflected are too entirely lost, as if the reflection were from the bottom of the enchanted lake instead of from its surface. The artist's idea has evidently been that there have been two transmissions of the light through the water, in consequence of which its original colour has been lost. This cannot have been so.

123. "On the Marshes," Percy Belgrave. The ordinary laws of reflection do not seem to apply in this case.

162. "Loch Scavaig, Isle of Skye," Sidney R. Percy. Even if the moon had not been veiled by a cloud we could not get this effect, nor with such rough water would the wake alone have been so illuminated; we should have had side reflections as well.

168. "Loch Alsh," Colin B. Philip. Good; distance and water reflection carefully managed

356. "Among the Trawlers, Tarbert, Loch Fyne," Andrew Black. Reflection from water very admirably managed.

508. "Green Pastures and Still Waters," B. W. Leader, A. The reflected images of the trees in the distance are about one and a half times as long as the trees themselves. Still water does not magnify the height of objects when they are reflected in it. One of the branches of the tree to the left has also considerably suffered by the reflecting process.

648. "A North Country Stream," Alfred W. Hunt. Very perfect study of water, the light reflected by the surface being mingled with that coming from the bottom.

688. "Willows Whiten, Aspens Quiver," Keeley Halswelle. Admirable landscape and water, but the colour and shapes of the clouds are unsatisfactory, and the picture would be better without them.

1509. "A Pebbled Shore," Colin Hunter. Note the way in which the cumuli are reflected from the waves beneath them. Glorious picture.

1493. "Toil, Glitter, Grime, and Wealth on a Flowing Tide," W. L. Wyllie. An admirable picture, but we question whether the artist is justified in getting such a brilliant reflection from the surface of the water to the



left, where the sky as indicated does not appear to be a very luminous one.

142. ". . . these Yellow Sands," John Brett, A. In this admirable picture, in which the sea and sky are quite perfect, Mr. Brett has attempted some difficult effects. More transparent water has never been seen on a canvas, and the colour of the yellow sand at its bottom is beautifully mingled with the light reflected from its surface.

626. "Sounding for Shallows at Low Nile," Tristram Ellis. A bold attempt at reflection in the Nile water, but, as a matter of fact, the real colour is not so entirely subordinated by reflection.

#### SNOWSTORMS

764. "The Joyless Winter Day," Joseph Farquharson. The storm must have been very considerate to the artist. In spite of the driving blast there is not a single snowflake to be seen in the first twenty yards.

#### THE LIVING ORGANISMS OF THE ATMOSPHERE

*Les Organismes vivants de l'Atmosphère.* Par M. P. Miquel, Docteur ès Sciences et Docteur en Médecine, Chef du Service micrographique à l'Observatoire de Montsouris. (Paris: Gauthier-Villars, 1883.)

*PLU* occidit aer quam gladius, such is the main idea contained and explained in M. Miquel's very able and interesting book. If the modern theories are true, it must be certainly conceded that although the sword and gun are very murderous tools, air is yet more so. But on the other hand one may say of our atmosphere's murderous propensities what a French writer said when he was told that coffee was a poison: "Well, it may be a poison to be sure, but it must be a very slow one; I have been indulging in it for over fifty years." In fact, if Voltaire and many other men took too much of it, it began to tell on them only very late. Taking it for granted that coffee is murderous, it must be also granted that it is not always so. Such is also the case of the atmosphere we live in.

The influence of infinitely small organisms contained in the air and water, as well as in the body of man and animals, can no longer be denied, at least, in a general manner. Certainly much remains to be done to bring the Microbe Theory to the point it must attain; many inconsistencies and discrepancies yet interfere with its general harmony; but Davaine's and Pasteur's experiments and discoveries have certainly opened new ways in science.

Now that it is granted that the organisms alluded to are to be found and may thrive in the air, it is interesting to know what these are, how abundantly they may be found in the atmosphere, and by what means they may be captured and experimented upon. To these important questions M. Miquel answers in a very precise and interesting manner.

It is not a difficult thing to detect the corpuscles contained in the atmosphere; a mere sunbeam in a room shows hundreds of them dancing in the light. But it is less easy to ascertain the nature of these little atoms; great skill is required to do that. Some are vegetable, some are mineral, some are animal.

M. G. Tissandier has established that a great quantity

of mineral atoms is contained in the atmosphere; the most interesting of these are meteoric iron melted into the form of little globules. Some infusoria are also to be found, but bits of wool and silk, pollen and spores are more abundant. As one may easily believe, all these corpuscles are less abundant in the atmosphere after a fall of rain. For instance, M. Tissandier finds in a cubic metre of air 0.023 gramme of dust after a rainless week; 0.006 gramme the day after a heavy rain.

The description given by M. Miquel of the numerous instruments contrived by himself and by others to collect the corpuscles contained in the air is good and interesting, but is not easily condensed. Another very important chapter of this book is that concerning the nature and origin of the aerial corpuscles among which pollen, flour, and spores are most abundant. For instance, the number of spores to be found in a cubic metre of air is about 14,200. But this number changes very much according to the season. In winter the mean number is 6200; in spring, it is 13,000; in summer, 28,000; in autumn, 9800. The reason of these variations is easy to understand.

However abundant spores and pollen, woollen and silk threads may be in the air, that is a question of little importance when compared with that of the presence of bacteria in the atmosphere. Bacteria are to be found, often in great quantity, in the air. Generally speaking, according to M. Miquel's experiments and observations, bacteria are more abundant when the weather is dry; the reverse is to be observed concerning spores of inferior cryptogams. The direction of the prevailing wind has much to do with the number of bacteria found in the air. M. Miquel shows, by means of a diagram, how the air having passed through part of Paris, before coming to the Montsouris Observatory, contains more bacteria than that which passed only over the suburbs and country around the town. South winds bring from 42 to 77 bacteria to a cubic metre of air; northern ones bring from 108 to 152. Other experiments give the same results. M. Miquel draws from his numerous experiments the conclusion that the air in Paris contains nine or ten times more bacteria than does that outside of the fortifications or close to them. For instance, in the Rue de Rivoli, M. Miquel finds an average number of 760 bacteria in autumn, 410 in winter, 940 in spring, and 920 in summer; that is, a mean annual number of 750 bacteria per cubic metre of air. At Montsouris the mean annual number is 75. The minimum number found by M. Miquel is 45 (winter 1882); the maximum is 3000 (summer 1881) bacteria per cubic metre.

In hospitals, the air contains a much greater quantity of bacteria, as might be expected; the cubic metre contains an average of five or six thousand! In some cases M. Miquel has found ten, even sixteen, twenty-one, and *twenty-eight thousand* bacteria per cubic metre of air. These last numbers are stupendous.

These bacteria in the air, liable every moment to penetrate into our lungs and body, are of many sorts. Some are spherical,—the *sphero-bacteria*; they generally have no power of locomotion; some are coloured red or yellow. M. Miquel remarks that although some of these bacteria must exert a pathogenetic action, he has not been able to produce any disease in ani-



mals by means of these organisms. It may be that the atmosphere kills these bacteria, it may be that the animals experimented upon were not liable to catch the disease; at all events it would seem that no pathogenetic bacteria are to be found in the air. This is a very important conclusion, but it is not yet sufficiently supported by facts. How could scarlet fever, measles, and other diseases be brought by a physician from a patient to a healthy person if the bacteria could not resist the action of the air for some time?

Other bacteria present a more elongated shape: they are called *bactéries en batonnets*. They generally move about, sometimes very slowly, sometimes with great rapidity, in various manners, when they are allowed to remain in a suitable liquid. M. Miquel has remarked that one of these bacteria converts sulphur into hydrosulphuric acid in a very energetic manner; together with another similar bacterium it is the principal agent that converts urine into sulphuret of ammonia. M. Miquel cannot say exactly as to the presence of pathogenetic bacteria in the atmosphere, nor especially as to their precise nature and *modus faciendi*.

Bacilli are also to be found in the atmosphere; they may be long or short; the less they move about the longer they become. One of these bacilli resembles very much the *Bacillus amylobacter* (van Tieghem). Another one seems pathogenetic; it brings on, in animals, a phlegmon that generally terminates—as is the custom of most phlegmons—in suppuration. Of course many other pathogenetic bacilli perhaps exist in the atmosphere, but that question has not been specially discussed by M. Miquel. He shows very well how considerable an influence the rainfall exerts on the number of the bacteria contained in the air. Temperature has little to do with this as diagrams show; rain on the contrary has a great effect. As soon as the weather becomes dry the number of the bacteria increases; when it is rainy this number falls rapidly. This result is one of the most important among those M. Miquel has attained, inasmuch as this *savant* shows that rainy periods are those during which the bacteria multiply.

If the number of these organisms is considerable in the air we breathe every day, one thing must however console us in some degree. If these bacteria are murderous, they are somewhat like the coffee; they kill very slowly in most cases. Many of them must each day come into our lungs and body, and yet we feel none the worse for it generally. This does not mean that they are not dangerous; it means only that they are not always able to act a dangerous part. For what reason, we know not yet. Typhoid fever, cholera, yellow fever, measles, scarlet fever, and a great many other diseases are contagious; but all persons who live with patients suffering from either of these diseases do not catch them. Most doctors and medical students do not catch any contagious disease in the hospitals, and yet they doubt not the nature and danger of these diseases.

Whatever opinion one may entertain as to the Microbe Theory, it must be admitted that M. Miquel's book is exceedingly useful and well arranged. M. Miquel understands the matter thoroughly, and his book will certainly be much read abroad, as it has been in France.

HENRY DE VARIGNY

### ANIMAL TECHNOLOGY

*Animal Technology as Applied to the Domestic Cat.* An Introduction to Human, Veterinary, and Comparative Anatomy. By Burt G. Wilder, B.S., M.D., and Simon H. Gage, B.S. (New York and Chicago: A. S. Barnes and Co., 1882.)

MESSRS. BURT WILDER AND GAGE are not the first anatomists to employ the domestic cat as an introduction to the study of vertebrate anatomy. In 1881 Mr. St. George Mivart published an elaborate treatise on the Cat, as a type for examination and comparison with other vertebrates; and as far back as 1845 M. Straus-Durckheim issued his well-known work in the French language on this animal.

The book now before us differs however in its scope and mode of treatment from its English predecessor. It is not like Mr. Mivart's, a systematic treatise on the anatomy of the cat, both macroscopic and microscopic, with chapters on its development, psychology, specific forms, geographical distribution, &c. But it is a practical treatise written with the object of instructing the student in the methods of dissecting and displaying the structure of this animal.

As preliminary to the anatomical description, the authors have written some short chapters on the instruments employed in dissecting, the modes of using them, the methods of injecting, and the preparation and preservation of anatomical specimens, so as to justify the title of *Anatomical Technology* given to the book. We would especially direct attention to the sections on the maceration of bones and the preparation of skeletons as furnishing the young anatomist with useful hints on these subjects.

Those who are familiar with the papers on *Anatomical Nomenclature* by Prof. Wilder in the *American Journal Science*, and elsewhere, will not be surprised to find that he has in this work again enunciated his views on Terminology, and adopted many but little used, as well as new terms in his descriptions. There can be no doubt that the terms used in anatomical description in many instances would be improved by being altered. No one who is engaged in the comparative study of the anatomy of the human body, with that of other vertebrates, but must constantly feel a difficulty in the use of the terms employed to express position. He has ever to keep in mind that a surface which is superior in man is anterior in any other vertebrate, and that a surface which is posterior in man is superior in vertebrates generally. Hence such terms as dorsal and ventral, cephalic and caudal, are much to be preferred to express corresponding surfaces throughout the vertebrata, whatever may be their direction, than posterior, anterior, inferior, superior. If indeed the recommendations made by the Edinburgh anatomist, Dr. Barclay, in the early part of this century, had been attended to, then anatomical description would by this time have been on a much more satisfactory basis than it is. The delay and difficulty in effecting the necessary reforms are largely due to the works on human anatomy having been for the most part written by men, who are specialists in that department only, and have not had a wide and philosophical training in the whole subject. The introduction, however, of biological study into



the scheme of a general education, and the publication of such books, as the one now before us, as guides to a practical knowledge of the structure of animals, will break up the conservative instincts of the purely human anatomists, and will lead in time to the adoption of a more scientific nomenclature.

To turn now to the descriptive part of this book. The impression we have derived from its perusal leads us to say that it is well adapted to the purpose for which it has been written. The authors have evidently studied the anatomy of the cat, not from the dissection of a single animal, but from numerous specimens. The methods of displaying structure, and preserving the parts for future observation and study are workmanlike and practical. The descriptions are clear and concise. Though at times terms are employed, such as ectal for external, ental for internal, trochiter for the great tuberosity of the humerus, and trochin for the lesser tuberosity, which are novel, and at first require a little thought to gather their meaning, they soon become familiar, and without doubt conduce to give clearness and accuracy to the description.

We ought not to omit to say that, as preliminary to the description of the cat's brain, the authors give an account of the dissection of the brain of the frog and the Menobranchus.

The work is illustrated with 130 figures in the text, and with four lithographed plates of the brain of the cat. The plates are neatly executed; but the figures in the text are in many cases coarse and inartistic. Surely in the United States, where the art of engraving on wood, as is shown in the illustrations to Scribner's and other monthly magazines, has attained such a high order of excellence, the authors ought to have been able to procure a draughtsman and woodcutter who could represent muscles, more like nature, than is given in say Figs. 66, 67, and 72.

#### OUR BOOK SHELF

*Magyarország Ásványai, Különös tekintettel termőhelyeik megállapítására. (The Minerals of Hungary, with Special Regard to the Determination of their Occurrences.)* By Michael Tóth, S.J., Professor at the Gymnasium, Kalocsa. (Budapest, 1882.)

WE have here a contribution to science which reaches us from the far east of Europe, from Hungary. The author has aimed at nothing less than to give a complete catalogue of all the minerals that occur in that country, noting the exact place of the occurrence of each, and adding such statistical and other information as may enable the reader to form a judgment as to the economic value of the subject of the article. Special attention is given to such minerals as are of recent discovery or of such importance as to be likely to affect the future history of the district in which they are found.

Prof. Tóth is, we believe, the first writer who has attempted a complete account of the minerals of Hungary. His work would have been more widely useful had he seen fit to employ some language that is more widely known than his native Hungarian. But in the case of a work like this, which consists largely of names of places and of those technical names of species which are common to all the civilised world, the unfamiliar tongue does not render the book altogether useless. The author would seem to have looked forward to his work being used in England, for he has prefixed an English title-page, and frequently refers to the collections in the British Museum and the Museum of Practical Geology.

#### LETTERS TO THE EDITOR

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

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

#### Natural Selection and Natural Theology

THE amicable discussion between Dr. Romanes and myself, 'endeavouring to help in determining the true position of an important question,' has now (in NATURE, vol. xxvii. p. 527) reached a critical point, one seemingly capable of settlement by scientific inquiry, and upon which a brief note may be pertinent.

I take Dr. Romanes now to agree with me that the physical distinction of the less fit organisms, or, more generally, that the action of the environment, is not in a proper sense the cause of the advantageous variations of surviving organisms; also that natural selection does not explain and has no call to explain the cause of variation. As to this, he says, the theory merely supposes that variations of all kinds and in all directions are constantly taking place, and that natural selection seizes upon the more advantageous. Now if variation in animals and plants is lawless, of all kinds and in all directions, then no doubt the theory of natural selection may be "the substitute of the theory of special design," so as to efface that evidence of underlying intelligence which innumerable and otherwise inexplicable adaptations of means to ends in nature was thought to furnish. If it is not so, then the substitute utterly fails. For omnifarious and purely casual variation is essential to it in this regard. Fittingly it said that "the theory merely supposes" this. For omnifarious variation is no fact of observation, nor a demonstrable or, in my opinion, even a warrantable inference from observed facts. It is merely an hypothesis, to be tried by observation and experiment. I am curious to know how far the observations and impressions of the most experienced naturalists and cultivators conform to my own, which favour the idea that variations occur, in every degree indeed, but along comparatively few lines. That the investigator of any flora or fauna should so conclude as to actual and accomplished variation, is natural, but may go for little, the theory of course supposing that numberless non-occurring forms have failed in the struggle and disappeared. But there is no evidence that all sorts of varieties ever appeared or tended to appear, and there is a *musty maxim* about "*de non apparentibus et de non existentibus*" which is not devoid of application.

Moreover, as to the vegetable kingdom, it would seem that this question of omnifarious variation may be tested in the seed-bed and the nursery, from which Darwin took the idea and the term of natural selection. These indeed are actual experiments—very numerous and extensive—for the testing of incipient variation. If experienced nurserymen, gardeners, and others who raise plants from seed in a large way, usually with eyes watchful for variation, would give their testimony in this regard, they might materially contribute to the settlement of an interesting question.

We need not hold Dr. Romanes to the terms of his fundamental supposition, "that variations of all kinds and in all directions are constantly taking place." He probably means only that incipient variations are wholly vague and irrespective of ends—are as likely to occur in the direction of unfitness as of eventual fitness to the environment and to use, the divinity that shapes the ends—if ends there be—acting only through the surroundings. And we all understand that the particulars in which progeny differs from parent are potential in the germ, or in the cells of which the germ consists, and therefore wholly beyond observation. The up-hot is, that, so far as observation extends, it does not warrant the supposition of omnifarious and aimless variation; and the speculative assumption of it appears to have no scientific value.

ASA GRAY

#### The Fauna and Flora of the Keeling Islands, Indian Ocean

I HAVE only recently been able to obtain my copy of Mr. Wallace's "Island Life," in which I find an estimate of the fauna and flora of the Keeling Atoll in the South Indian Ocean. I had the fortune to visit that outlying spot in the year 1879,



and made a collection both of its plants and of its animal life. With the exception of my birds and a few of the insects, my collections were destroyed by sea water, so that it is now impossible for me to give a definite list, but I may note that rats were in such numbers as to have become almost a plague. A goodly herd of introduced *Rusas*, a cross between the Sumatran (*C. equinus*) and Javan (*C. Hippelaphus*) species, were in excellent condition, and were living wild on Direction Island, where also pigs were living in the same state. Among birds, the *Gallus bangkiva* (introduced) was in considerable numbers; I saw also the nest of the *Ploceus hypoxanthus*, which comes, not every year, but very often to breed there, but the progeny seems either to die or to return to Java (?). I did not see the snipe, but of the *Rallus philippinus* I got several specimens. Egrets, blue and white, abounded and rested on the high trees on some of the islands. Lizards of several species are now found on most of the islands in large numbers. Of insects the number of species is very considerable. Coleoptera were represented by *Melolonthidae*, *Cetoniidae*, *Carabidae*, *Elaterridae*, *Chrysomelidae*, but as I have not my journals of that date by me, I cannot recall other families nor state the number of genera represented. Of Hemiptera I caught a good many species, mostly of small size. Many species of ants were observed. Neuroptera are represented, unfortunately, by the *termite*, introduced some years ago in furniture, it is said, but it occurs now on every islet of the group in myriads. I am told that during the cyclone of a few years ago, the whole surface of the sea was covered with the mangled bodies of dragon-flies for miles out to sea, but that since then very few have been seen. Of Lepidoptera I caught many species both diurnal and nocturnal, some very handsome, of which I sent a small collection to London in 1879. The Atlas Moth is rather common. Orthoptera were represented by the ubiquitous cockroach, and a few *Acridiidae*.

Mr. Ross told me that on several occasions the large fruit bat, called the flying fox, has reached the islands, and once a pair arrived together, but died, from exhaustion apparently, soon after arrival. Under favourable circumstances, as in the case of an unusually strong pair, these may yet become inhabitants of the islets.

There are, I believe, considerable additions to the flora since Mr. Darwin's visit. It is only within recent years that the islands have become so greatly covered with coconut plants. Their original vegetation consisted principally of "iron wood" (*Sideroxylon*?) and other trees, and of low shrubs. These were nearly all burned out by accidental fires, one of which burned for three months.

HENRY O. FORBES

Fatunaba, Timor Dilly, January 21

"Festooned" or "Pocky" Clouds (Mammato-Cumulus)

UNDER one of these names letters have appeared at different times in NATURE, notably on October 19, 1871. These were followed by a paper read before the Meteorological Society by Mr. R. H. Scott in February, 1872, in which he collects all the observations which had then been recorded, and the theories which had been propounded to explain them.

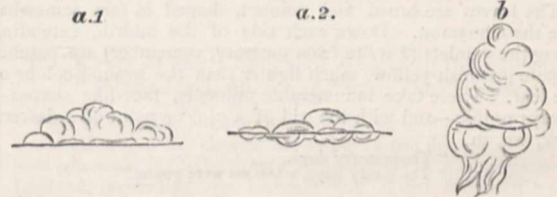
For several years I have been watching this kind of cloud, and I think that its formation is capable of a very simple explanation, partially in agreement with that suggested by Mr. Jevons in the earliest notice of these clouds (*Phil. Mag.*, July, 1857). The name is applied to a peculiar festooned appearance sometimes seen below cumulus and stratus clouds. In Orkney Mr. Clouston has found that it is usually followed by a severe gale; but in Lancashire, where the festoons are called "rain-balls," it is only considered a sign of rain. Other observers in the tropics have also seen it with thunderstorms, and not necessarily with wind. In this country I have observed it both in heavy gales and also in an ordinary summer thunderstorm. The method by which I have endeavoured to discover its origin has been to try and trace its life-history; that is to say, to follow its growth from other forms of cloud and to watch the forms into which it develops.

On one point almost all observers are agreed, that the festoons are frequently seen just before a cloud begins to break up. The first time that I was fairly able to trace the formation of the cloud was one summer evening in London, when towards sunset a flat-based cumulus, like that marked *a 1* in the figure, suddenly became festooned at the base and diminished on the top, as marked *a 2* in the figure. A few minutes afterwards the whole cloud evaporated. The succeeding night was fine. The

explanation which immediately suggested itself was that the ascensional current which had formed the flat-based cumulus had suddenly failed, and that the festoons were simply the masses of vapour falling downwards for want of support.

Another very striking case is marked *b* in the figure, and was observed before a shower. Here a detached cumulus was observed to form first festoons, and then they in turn degenerated into raggy cloud, the whole disappearing very shortly, but was quickly followed by fresh rain-bearing clouds. The impression which the whole conveyed to me was that the festoons were formed by a sudden drop of the cloud, and that the "rag" was produced when the drop was less sudden. The appearance of the "rag" is not very well rendered in the diagram, but it is very difficult to delineate clouds by any engraving.

These are two typical cases of many which I have observed, and always with the same result—that the constant condition necessary for the formation of festoons was the sudden failure of an ascensional current of air. If so, the explanation of its prognostic value is very simple. Before many squalls or showers we are all familiar with the short, abortive gusts which so frequently precede them. Now we have only to assume that the ascensional uptake in front of the main body of the shower is as unsteady as the surface wind, and we have at once all the conditions of the formation of festoons. Almost all observers agree that they are usually formed at the edges of cloud masses. In the case of rain or thunder they ordinarily appear just before or after the rain; but in the case of a gale following some time afterwards, as observed by Mr. Clouston, the festoon must have been formed by some local squall or shower which bore some



relation to the disturbed weather which produced the gale. I once saw festoons in the west of Scotland during the hardest gale I have ever seen in this country. They were formed on the outskirts of a north-westerly squall.

Allied to festooned cumulus we may mention festooned stratus and festooned cirrus. The former is quite common in London during the summer, associated with showers or thunderstorms, while the latter is rare. In both the same idea seems to hold good as for cumulus, that they are formed by the sudden failure of the current, whatever it may be, that forms the stratus or cirrus.

It might appear, at first sight, that a uniform stratus could not fall in lumps; but however uniform it may seem, viewed from below, there is probably no such thing as a uniform stratum of cloud. Some portions are always denser, or composed of larger drops, and these, falling first, give the "pocky" appearance. In many simple cases, which I have been able to follow, there often seems to be a rough correspondence between bosses on the upper surface and festoons on the lower. In *a 2* there is an unsuccessful attempt to depict such a case, which is drawn from nature.

The name of "festooned cloud" has been objected to as suggesting a lengthways arrangement of vapour, like the cloud called "rolled cumulus," with which it has probably nothing in common. Mr. Clement Ley has proposed the name of "tubercled cloud" as more applicable. Prof. Poey, who has also studied this cloud, has proposed the name of "globo-cumulus."

The general conclusion then, is that festoons are caused by a sudden failure of an ascensional current associated with showers or squalls, but whether they portend rain or wind depends on the circumstances under which they are observed.

21, Chapel Street, S.W., April 27 RALPH ABERCROMBY

The Sacred Tree of Kum-bum

PERHAPS the following statement may throw a little light on what was the tree seen by the Abbé Huc:—

On his voyage home from China the Abbé touched at Ceylon. This must have been in 1852 or 1853, as far as I can recollect. I was invited to meet him at breakfast, at the house of my kind



friend, Sir Charles Macarthy, then Colonial Secretary, my zoological and botanical tastes being well known to the latter.

The conversation turning on plants, the Abbé described a wonderful tree which he had seen, on the leaves of which were impressed thousands of likenesses of Buddha. Nothing was said about "Thibetan characters," nor did he lead us to suppose it grew larger than an ordinary cinnamon-tree (*not bush*), as it grows wild. His description was so detailed that, in spite of the florid language of a French traveller, I at once recognised a plant which grew not uncommonly in our gardens, the leaves of which were often placed in the finger-glasses after repasts, as on being crushed, they imparted a delicious fragrance to the hands. Looking up and catching the eye of our hostess, in which lurked an amused smile, I made the motions of dipping hands in a finger-glass. She instantly caught my meaning, whispered her instructions to the servant behind her chair, and each finger-glass—which useful adjunct to a meal was shortly after placed on the table—contained a leaf or two of what we used to call by a variety of names, such as the "profile laurel," or "figure laurel," or "face laurel."

The face of the Abbé was a picture to behold. "But here it is!" he exclaimed. "Where did this come from?" We then explained that it grew not a dozen yards from where he sat, to his great astonishment, and I fancied not a little chagrin, that his wonderful plant should be so well known and common.

The plant is, I believe a *laurel*. It has flashed across me that it *may be a citron*, but the plant is so well known in Ceylon, that if your contributor, Mr. W. T. Thielton Dyer, wishes to ascertain its name, he has but to write a line to the Director of the Botanical Gardens, Peradenia, who will at once recognise it.

The leaves are broad and pointed, shaped in fact somewhat like the cinnamon. Down each side of the midrib, extending along the veinlets (I write from memory, remember) are patches of pale greenish-yellow, much lighter than the ground-colour of the leaf. These take innumerable fantastic, face-like shapes—always profile—and with the aid of a pin, or point of a dessert-fork, we, in

"Those merry days,  
The merry days, when we were young,"

used to put in an eye, and amuse ourselves in trying to find likenesses of our friends and acquaintances. It was a source of much fun among the young people.

The events of that morning were, from a variety of circumstances, deeply impressed on my memory, and I am positive that *then* nothing was said about "Thibetan characters" on the leaves or on the bark, nor of the *great size* of the tree, and the Abbé distinctly recognised the leaves as identical with those he had seen. You will perceive he calls it the "Tree of the Ten Thousand Images" (the italics are mine). This name would well apply to the "profile laurel," for no two faces are ever alike, but does not include *characters*.

Whether the size of the tree and the "Thibetan characters" grew (in the Abbé's brain?) after he left Ceylon, I do not know. The "real article" seems to have vanished. A bungling attempt to deceive by etching in lilac leaves could easily be detected, but "travellers see strange things!" E. L. LAYARD

Brit. Consulate, Noumea, New Caledonia, March 5

### Sheet-lightning

THE correspondence on this subject (NATURE, vol. xxviii. pp. 4 and 54) can scarcely be said to contribute anything in support of the statement that sheet-lightning and the so-called summer or heat-lightning, are nothing else than the reflection of, or the illumination produced by, distant electrical discharges. The table given in the review (NATURE, vol. xxvii. p. 576) is not a record of instances of sheet-lightning, but only the number of hours, sorted according to the twenty-four hours of the day, in which sheet-lightning or heat-lightning was observed at Oxford during the twenty-four years ending 1876. In constructing the table, all those hours were excluded in which thunder was heard, and also the hour immediately preceding and following the hour of occurrence of thunder. Only those hours, therefore, were included during which any thunder that may have accompanied the lightning was at some distance from Oxford.

It follows simply as a matter of statistics that, if all cases of sheet-lightning are nothing else but the illumination produced by distant electrical discharges, the curve of thunder and the curve of sheet-lightning and heat-lightning should be approximately parallel to each other after darkness has fairly set in. The

Oxford observations show that such is not the case. To make this quite clear we give the results for August only:—

	Thunder.	Lightning.
8-9 p.m. ... ..	5 ... ..	0
9-10 ,, ... ..	3 ... ..	3
10-11 ,, ... ..	4 ... ..	6
11-mid. ... ..	3 ... ..	14
Mid.-1 a.m. ... ..	2 ... ..	14
1-2 ,, ... ..	2 ... ..	12
2-3 ,, ... ..	1 ... ..	4
3-4 ,, ... ..	0 ... ..	3

These two sets of figures from 8 p.m. to 4 a.m. furnish two curves quite distinct from each other; and the difference is not to be explained by the degree of facility for recording the observations afforded by each separate hour. It may be added that a similar result is obtained from electrical manifestations in other parts of the globe during the summer months. It is from these facts that it was concluded that no inconsiderable number of the cases of sheet-lightning and heat-lightning are not illuminations produced by distant electrical discharges, but, as suggested by Loomis, are rather to be considered as due to the escape of the electricity of the clouds in flashes so feeble that they produce no audible sound, and they occur when the air being very moist offers just sufficient resistance to the electricity to develop a feeble spark.

THE REVIEWER

### Solar Halo

THE following, taken from vol. i. *Philosophical Transactions*, p. 219, may interest your readers, as the phenomenon appears to coincide almost exactly with the one recorded in NATURE, vol. xxviii. p. 30. I omit the illustration, though it corresponds almost exactly with the one in NATURE, except that there were mock suns.

"An account of four suns, which very lately appear'd in France, published in the French *Journal des Sçavans* of May 10, 1666:—

"The 9th of April of this present year, about half an hour past nine, there appear'd three circles in the sky. One of them was very great, a little interrupted and white everywhere, without the mixture of any other colour. It passed through the midst of the sun's disk, and was parallel to the horizon. Its diameter was above a hundred degrees, and its center not far from the zenith.

"The second was much less, and defective in some places, having the colours of a rainbow, especially in that part which was within the great circle. It had the true sun for its center.

"The third was less than the first, but greater than the second, it was not entire, but only an arch or portion of a circle whose center was far distant from that of the sun, and whose circumference did by its middle join to that of the least circle, intersecting the greatest circle by its two extremities. In this circle were discerned also the colours of a rainbow, but they were not so strong as those of the second.

"At the place where the circumference of this third circle did close with that of the second, there was a great brightness of rainbow colours mixt together. And at the two extremities where this second circle intersected the first, appear'd two parhelia or mock suns,' &c., &c."

In a note to this account it is stated that "Five suns appear'd the 29 March, A., 1629, at Rome between 2 or 3 of the clock in the afternoon." In the illustration given we find two circles similar to those given in NATURE. It seems that two of these suns "which were in the intersection of two circles, appear'd in that of a circle, which passed through the sun's diske, with another, that was concentric to the sun."

The phenomenon of last week was minus the parhelia; can any reason be given for this?

THOS. WARD

Northwich, May 15

IN reply to Mr. Mott's query (p. 30) I beg to say that I measured the halo with a sextant as carefully as possible, and made the semidiameter 25'. [Another halo occurring on the 13th measured 23' 20'.]

With regard to the mock moons, they were perfectly equidistant from the horizon all the time I observed them, and I regret that I did not notice that Mr. Mott had seen them otherwise. I read his letter rather hurriedly and thought the expression "out of place" referred to their position above the moon, and not to a want of parallelism with the horizon.

SM.

Temple Observatory, Rugby, May 17



## Mock Moons

I NEVER noticed that mock moons and mock suns are not always at the same altitude as the moon or sun, but I would point out that when objects are high up, it is very difficult to decide on their relative altitudes. If mock moons are at the same altitude as the moon, then of course they are not on a great circle, but on a small one, and in consequence, except when they are low down, a straight line passing through the mock moons will pass above the moon, and when they are high up, at a considerable distance above it. In such a case, if the observer does not look straight at the moon, he may easily suppose that one of the mock moons is higher up than the other. Is your correspondent (vol. xxvii. p. 606) *sure* that they were not at the same altitude on the occasion he refers to? If, instead of facing the moon and looking straight at it, he looked more at the right-hand mock moon, the illusion would be produced of the left-hand one appearing higher up. The same illusion is caused when the horizontal lines of buildings or of a window cut the line passing vertically through the moon obliquely; so that great care is required in making these observations.

I might add that I observed the mock moons and halos on the evening alluded to (April 16) from Sunderland till after 11 p.m., and that I noticed nothing unusual in their positions or size. The mock moons (or suns) are always outside the ordinary halo when their altitude is considerable. On that occasion there was also visible a considerable part of the horizontal halo passing through the mock moons, forming long tails to them away from the moon; also vertical and horizontal rays proceeding from the moon; forming a faint cross with the moon at its centre. The horizontal rays were narrow, and reached at one time to the ordinary halo, but were much fainter than the "tails" of the mock moons. The vertical rays did not reach quite so far, and were broad and indefinite; otherwise I suppose their character (except as to brightness) would be much the same as that of the "sun pillar" described by several of your recent correspondents.

T. W. BACKHOUSE

Sunderland, May 12

## Helix pomatia

ALTHOUGH this species is decidedly local in this country, yet it is interesting to note that the counties in which it has been recorded are contiguous to one another. Its course of distribution appears to pass through Kent, Sussex, Surrey, Hants, Wilts, Gloucestershire, Berks, Oxon, Bucks, Herts, and Northamptonshire, and this seems to support Mr. Stokoe in his suggestion (NATURE, vol. xxviii. p. 6) that it may be a geologically recent importation from France (to the northern portion of which it is confined in that country).

In Murray's "Handbook to Surrey," p. 70, *Helix pomatia* is stated to abound at Tyting Farm near Guildford, "said to have been introduced from Italy" by an Earl of Arundel, and Bevan's "Guide to Surrey," p. 111, mentions the same locality as the "habitat of the edible snail imported from Italy," &c. I visited this spot in September, 1880, in quest of *H. pomatia*, and mentioned my object to a farm labourer, who speedily produced three specimens from under a log of wood, but told me that they were not at all plentiful there, as the soil was sandy and not chalky, and he said I must look for them on the neighbouring chalk downs, whence his master the farmer procured his for the purpose of adopting the diet, which, when ill, he had been advised to try. Be *H. pomatia* indigenous or not, there is no doubt its presence in England has been assisted by importations, for Mr. Lovell Reeve mentions its being introduced from Italy by an English nobleman in the vicinity of Box Hill and Reigate (cf. also Gray's "Turton," ed. 1840, p. 35).

The *Helix scalaris* referred to in Venables' work on the Isle of Wight is cited in that book as a monstrosity of *H. aspersa*, and Moquin Tandon's figure of the variety *scalaris* is of the usual coloration of that species. The name, however, was originally bestowed by Müller on a variety of *H. pomatia* (Lamk. "An sans vert," second edition, vol. viii. p. 32), and is figured as such by Draparnaud, but Venables' reference seems to apply to a scalariform variety of *H. aspersa* observed by Dr. Gray near Ventnor.

W. C. ATKINSON

Streatham, S.W., May 11

## Cape Bees

I CAN endorse all that Sir J. H. de Villiers says concerning the sense of smell in the wild bees of the Cape. The aver-

they have to sweating horses is well known, as also to the scent of chopped carrots. The following instances of this have come under my own notice:—

A party of young men who had been springbok hunting all the morning, off-saddled their horses during the hottest part of the day, under the shadow of a great krantz (cliff); they had but just tied them to some trees, when the poor animals were attacked in the most vicious manner by an immense swarm of rock bees from the krantz, and so dreadfully were they stung, that, although the thongs that bound them were cut through as quickly as possible to enable the poor things to escape, one beautiful horse was stung to death, and two more of the number were so maddened that they galloped off, and for many days were quite unfit for use.

One of the Hottentot children upon our place, playing in the garden near some hived wild bees, mischievously chewed up a carrot, and spat it into the entrance of the hive; the boy was perfectly naked, and the next few minutes might have been his last, had not the European gardener happened to be near, and hearing his shrieks, hastened to the spot, thrust the child into a newly-dug trench, and quickly covered him with earth; but he had a narrow escape of his life, for he was literally covered with stings.

The precursor of a storm in the Karoo is generally a whirlwind of dust, and our boys used to take advantage of the dislike to storms evinced by bees, to throw up large handfuls of dust into the air, when a swarm was passing overhead, when sometimes the bees would be deceived and settle immediately.

M. CAREY-HOBSON

Late of Graaff Reinet, Cape of Good Hope

The Effect of the Change of Colour in the Flowers of "*Pulmonaria officinalis*" upon its Fertilisers

YESTERDAY I had an opportunity of convincing myself by direct observation that the change of colour in the flowers of *Pulmonaria officinalis* is of the same significance as in *Ribes aurcum* and *Lantana*, according to Delpino and Fritz Müller (compare NATURE, vol. xvii. p. 79).

In a small locality about twenty yards long and two broad, where many hundred flowers of *Pulmonaria* were in all stages of development, its principal fertilisers were the females of *Anthophora pilipes*, F.; they visited almost exclusively the red flowers and those just beginning to change towards blue, but only exceptionally blue ones.

The first individual which I watched when it was flying from flower to flower did so without any exception. Another individual newly alighting on the place at first now and then visited one or some few blue flowers, but the longer it continued its predatory flight the more it neglected the blue flowers and selected only the red ones.

A third female of *Anthophora* which I followed indiscriminately visited (a) red flowers of *Pulmonaria*, (b) large blue flowers of *Glechoma*, both in the following order:—(a) 16, (b) 1, (a) 23, (b) 1, (a) 21, (b) 62, (a) 5 flowers; then it left the place without having touched a single blue flower of *Pulmonaria*.

The fourth and last female of *Anthophora* I followed neglected completely the flowers of *Glechoma*; but when it visited the red flowers of *Pulmonaria* and met for some time only with already emptied ones, it became more and more disturbed and hurried, and then indiscriminately visited blue and red flowers until anew it found honey in a red one. It visited (a) red and (b) blue flowers of *Pulmonaria* in the following order:—(a) 52, (b) 1, (a) 18, (b) 3, (a) 16, (b) 1, (a) 34, (b) 3, (a) 7, (b) 1, (a) 42, (b) 1, (a) 13; in summa (a) 182 red, (b) 10 blue flowers.

It is easy to be seen whether a flower of *Pulmonaria* when visited by *Anthophora* contains some honey or not; in the first case the proboscis of the bee rests at least 1 to 1½ seconds in the corolla tube, whereas in the other case it is instantly withdrawn. All blue flowers of *Pulmonaria* which were visited proved thus to be empty of honey, and in all which I examined with a lens in this locality the stigma was supplied with pollen.

We may, I think, safely conclude from these observations that the blue colour of older flowers of *Pulmonaria*, whilst increasing the conspicuousness of the clusters of flowers, at the same time indicates to such intelligent bees as *Anthophora* to which flowers they have to restrict their visits as well to their own as to the plant's profit.

HERMANN MÜLLER

Lippstadt, May 8



### The Soaring of Birds

IN the discussion about the soaring of birds which has lately been carried on in NATURE, I do not remember to have observed that any one quoted from Mr. Darwin's account of the condor. He says ("A Naturalist's Voyage Round the World," chap. ix. p. 186):—"When the condors are wheeling in a flock round and round any spot their flight is beautiful. Except when rising from the ground, I do not recollect ever having seen one of these birds flap its wings. Near Lima, I watched several for nearly half an hour, without once taking off my eyes: they moved in large curves, sweeping in circles, descending and ascending without giving a single flap. . . . The head and neck were moved frequently, and apparently with force; and the extended wings seemed to form the fulcrum on which the movements of the neck, body, and tail acted. If the bird wished to descend, the wings were for a moment collapsed; and when again expanded with an altered inclination, the momentum gained by the rapid descent seemed to urge the bird upwards with the even and steady movement of a paper kite. In the case of any bird *soaring*, its motion must be sufficiently rapid, so that the action of the inclined surface of its body on the atmosphere may counterbalance its gravity."

Cambridge, May 17

JAMES CURRIE

### Intelligence in a Dog

SOME time since a friend of mine, Mr. J. W. Schaub, a mechanical engineer at the Edgmore Ironworks of Wilmington, Del., informed me of an exceedingly interesting case of intelligence in a black and tan terrier belonging to him. The old mother dog and her playful family entered his bedroom while he was dressing, and one of the pups snatched his stocking as he was in the act of putting it on, running out of the room with it. The mother at once followed the young offender, took the stocking from him, and returned it to the master. Mr. Schaub said that her conduct gave evidence of displeasure at the action of the pup, and she impressed him with the idea that she felt in some way responsible for the conduct of her young. Being greatly interested in the matter, Mr. Schaub contrived to have the offence committed on many successive mornings, the same performance being repeated each time.

St. Louis, U.S., April 24

FRANCIS E. NIPHER

### Mid-height of Sea Waves

CAN any of your readers furnish me with the formula, or other means, for finding the difference between the mid-height of a sea-wave and the sea-level?

W. PARFITT

### A CURIOUS SURVIVAL

THE thirteenth Annual Report of the Deputy Master of the Mint, just issued, contains some interesting information showing how persistently an ancient system of computing the value of bullion has survived in this country. The facts are fully set forth in an appendix to the Report by Prof. Chandler Roberts, who has recently and successfully advocated the adoption of the decimal system in the bullion transactions of the Mint. In order to make the matter clear, it may be well to state that the Troy pound, still used in this country for weighing the precious metals, is believed to have been derived from the Roman weight of 5759.2 grains, the 125th part of the large Alexandrian talent; this weight, like the Troy pound, having been divided by the Romans into 12 ounces. The earliest statute of this kingdom in which the Troy weight is named is the 2 Henry V. st. 2, c. 4, but the Troy weight is universally allowed to have been in general use from the time of King Edward I. The most ancient system of weights in this kingdom was the Moneyer's pound or the money pound of the Anglo-Saxons, which was continued in use for some centuries after the Conquest, being then known as the "Tower pound," or sometimes the Goldsmith's pound. It contained 12 ounces of 450 grains each, or 5400 grains, and this weight of silver was a pound sterling. The Tower pound was abolished in 1527 by a statute of Henry VIII., which first established Troy weight as the only legal

weight for gold and silver, and from this time to the present our system of coinage has been based on the Troy weight, the Troy pound containing 5760 grains.<sup>1</sup>

The bullion transactions of the Mint have hitherto been based on an Assayer's weight termed the "carat pound," the final division of which corresponds with the number of grains in the Troy pound, and side by side with this system a curious method of expressing the 'standard' or composition of ingots or coins of gold and silver has been retained until the present year. For instance, the ordinary conception of the composition of a sovereign would be that it is an alloy or mixture of the two metals gold and copper in definite proportions, and the most simple way of expressing its contents would be to describe them as consisting of 91.66 per cent. of gold and 8.34 per cent. of copper. An assayer or bullion dealer, on the other hand, using the old system, would simply consider the composition of the coin to be gold of 'standard fineness,' that is to say, containing two carats of alloying metal in the pound; and in dealing with any particular alloy of gold and copper would in no way regard its percentage composition, but would consider it as being so much "better" or "worse" than the one definite and legal standard, according as it contained more or less of the precious metal. The French 20-franc piece, which contains 90 per cent. of gold, would thus be described as "worse 0 carats 1 $\frac{1}{2}$  carat grains," and an Austrian ducat, which contains 98.61 per cent. of gold, as "better 1 carat 2 $\frac{1}{2}$  carat grains." The cumbersome nature of this system is evident; it has the disadvantage of being unintelligible to those who employ the decimal system, and who are therefore in the habit of mentally referring to pure gold as 1000. It is even found wanting in clearness by many who are conversant with the ordinary operations of coinage and bullion transactions generally. For instance, the meaning of "worse 0 1 $\frac{1}{2}$  + 1" as the assay report of an ingot is at least obscure, while the equivalent statement that the standard fineness of the ingot is 900 at once suggests that 1000 parts of the metal contain 900 parts of gold.

The ancient system of reporting the results of assays possesses however many points of interest, and Prof. Roberts adds a few details respecting it, taken from a work by Snelling,<sup>2</sup> an authority on the computation of the value of bullion, who, writing in 1766, observes that "by the word SILVER we understand not only the metal so-called, pure and unmixed, but also when in a mass with copper; and if but one-half, two-thirds, or any other proportional part of it be silver, yet the whole bears that name. The same is to be understood of GOLD, when by itself, or in a mass with silver and copper together, or with either of them alone."

"This is the reason that inquiries are not made, what quantity of fine gold or fine silver is contained in any mixture, which seems to be the most natural inquiry, but how much standard it holds." Thus it is that "the Assay Master, in reporting the result of an assay, does not give the absolute fineness or the quantity of fine silver or fine gold present, but only the relative quantity or fineness, that is, how much the mixture is more or less than standard. In the case of gold of 20 carats fine (or 20 parts of pure gold in 24 parts of the alloy) the assayer puts down **Wo.** <sup>car.</sup> <sub>ij.</sub> and if it is 23 carats 3 $\frac{1}{2}$  grains fine,

he puts down **Br.** <sup>car. gr.</sup> <sub>j. ij.</sub> <sup>ob.</sup> The last sign represents an obolus or half of a carat grain, but in modern times the final division has been 1/60th of a carat grain.

It may be pointed out in defence of this complicated system, that, as Snelling proceeds to remark, "the quan-

<sup>1</sup> "On the Abolition of the Troy Pound," the third Report of the Commissioners appointed to inquire into the condition of the Exchequer Standards. Parliamentary paper [c. 30], 1870.

<sup>2</sup> "Doctrine of Gold and Silver Computations," by Thomas Snelling. (London, 1756.)



tity of "betterness" or "worseness" in an ingot being added to or subtracted from the weight of it, gives the quantity of standard metal contained in it," and that therefore the "betterness" or "worseness" affords a ready means of determining the amount of copper or gold required to standardise the whole. Further, if a number of ingots of varying weights and fineness have to be dealt with, a similar result will be arrived at by taking the algebraical sum of the several products of their weights and "betterness" or "worseness." These advantages, however, apply to individual calculations, and become unimportant when standarding tables adapted to the decimal system are available.<sup>1</sup>

In a letter to Mr. Fremantle, Prof. Roberts advocated the abolition of this old system of carats and grains and the adoption of the decimal system. This has accordingly been carried into effect. Gold of the value of two millions sterling has recently been imported for coinage, and the simplicity and accuracy of the new system has been abundantly demonstrated.

The facts above stated may seem comparatively unimportant in themselves, but the Mint may at any time be called upon to coin (as was the case in 1872) fifteen millions sterling of gold in a single year, and extreme care has to be taken to insure accuracy in the standard fineness of the metal. It is curious that the old system described above should not have given place before now to that which has long been adopted in other countries.

#### THE POISONOUS LIZARD<sup>2</sup>

THE Gila Lizard of Arizona and Sonora has anterior, deciduous, grooved teeth, which communicate by ducts with large glands within the angle of the lower jaw—an apparatus so strongly resembling the poison-fangs of serpents as to suggest that this lizard has venomous properties. It is said by the natives of Mexico to be very poisonous, but others again have declared that it is perfectly harmless. One specimen sent to Sir John Lubbock killed a frog in a few minutes and a guinea-pig in three minutes.

The conflicting statements are probably due to the fact that the teeth are very small and easily removed. Some specimens of the creature reach the length of three feet. As experiments made by allowing the lizard to bite animals are untrustworthy on account of the uncertainty of getting the poison equally introduced into the tissues at every bite, Doctors Weir Mitchell and Reichert collected the saliva so as to be able to inject it in known quantities. The saliva was obtained by making the animal bite on a saucer-edge. It dropped in small quantities from the lower jaw, and had a faint and not unpleasant aromatic odour. It was distinctly alkaline, in contrast to serpent venoms, which are all alike acid. Four and a half minims of it diluted with half a cubic centimetre of water and injected into the breast of a large pigeon caused the bird to walk unsteadily after three minutes. At the same time the respiration became rapid and short, and at the fifth minute feeble. At the sixth minute the bird fell in convulsions with dilated pupils, and was dead before the end of the seventh minute. There was not the least trace of any local effect of the poison, as there would have been in the case of crotalus venom. The muscles and nerves were perfectly sensitive to stimulation mechanically or by weak induced currents. The heart was arrested in complete diastole, and was full of firm, black clots. The intestines looked congested. In another experiment it was found that the poison gradually

<sup>1</sup> Tables on the system above described were first published in the year 1651, having been prepared by Mr. Reynolds, Assay Master at the Mint in the Tower. A second edition was afterwards issued with corrections and additions in 1677.

<sup>2</sup> "A Partial Study of the Poison of *Heloderma suspectum* (Cope), the Gila Monster." By Dr. S. Weir Mitchell and Dr. E. T. Reichert of Philadelphia.

lowered the arterial tension and rendered the pulse irregular. Its action on the pulse is not due to any effect upon the pneumogastric nerves, as it is just the same when these nerves are cut. When applied to the heart of a frog it arrests its pulsations in diastole, and the organ afterwards contracts slowly—possibly in rapid *rigor mortis*. The cardiac muscle loses its irritability to stimuli at the time it ceases to beat. The other muscles and nerves respond readily to irritants, but the spinal cord has its power annihilated abruptly and refuses to respond to the most powerful electrical currents.

The authors conclude that "this interesting and virulent heart poison contrasts strongly with the venoms of serpents, since they give rise to local hemorrhages, and cause death chiefly through failure of the respiration and not by the heart, unless given in overwhelming doses. They lower muscle and nerve reactions, especially those of the respiratory apparatus, but do not as a rule cause extreme and abrupt loss of spinal power. Finally, they give rise to a wide range of secondary pathological appearances which are absent from *Heloderma* poisoning."

This distinction between the action of the poison of *Heloderma* and serpent venom is correct as far as regards the poison of the rattlesnake and perhaps also the *Crotalidæ* generally, but the distinction is by no means marked between the poison of *Heloderma* and the venom of the cobra. This venom was found by Sir Joseph Fayrer and Dr. Lauder Brunton to have but a slight local action as contrasted with that of the rattlesnake or of the daboia, and to produce no local hemorrhage. The effect of cobra poison on birds also is very much the same as that of the *Heloderma*; and in the experiments given in this preliminary paper, the effect of the *Heloderma* poison on the heart of the frog is very much like that of cobra poison, the failure of action with subsequent and gradually increasing contraction being almost precisely the same.<sup>1</sup>

In Brunton and Fayrer's experiments on cobra poison, the fall of blood-pressure was less marked, but it still occurred. Paralysis of the spinal cord also is produced by cobra poison, and the experiments in this preliminary paper are too few to enable us to decide whether the paralyzing effect is greater from the poison of *Heloderma* than from cobra venom. We shall look with much interest to the further study of the venom of this curious animal, which the authors intend to make on the arrival of the fresh specimens which they are about to receive.

#### ON THE CONDENSATION OF VAPOUR FROM THE FUMARoles OF THE SOLFATARA OF POZZUOLI

THE fumaroles of the Solfatara of Pozzuoli, and especially the larger fumarole known as the *Bocca della Solfatara*, give a striking illustration of the action of smoke in causing the condensation of aqueous vapour in the manner demonstrated by the experiments of Coulier, and more especially by those of Dr. Aitken.

Persons who have visited the Solfatara will remember that one of the feats by which the *ciceroni* of the place try to excite the wonderment of visitors is to light some paper or a few dry branches, and put the flaming body before or inside the mouth of the principal fumarole, augmenting thus very greatly the volumes of cloudy vapour escaping from the fissure. This phenomenon can be observed in all volcanic fumaroles. A flame is not indispensable, the condensation of the vapour being also produced by the mere smouldering of tinder.

Prof. Piria first tried to explain the phenomenon. He thought that small quantities of sulphuretted hydrogen issued from the soil together with the aqueous vapour:

<sup>1</sup> Brunton and Fayrer on the Poison of Indian Venomous Snakes (*Rey. Sec. Proceedings*, January 22, 1874, p. 126).



on mixing with air and coming in contact with a flame, or an incandescent body, the hydrogen sulphide would be oxidised, and resolved in sulphur and water (with the production of small quantities of sulphur dioxide); the sulphur, minutely divided, would remain long suspended in air, and cause the condensation to cloudy consistency of the aqueous vapour. Piria illustrated his explanation by a simple experiment: if in a vessel containing a mixture of sulphuretted hydrogen and air a lighted taper is introduced, a dense mist is rapidly formed; a similar mist is produced when glowing charcoal, or highly heated lava, or pumice, or glass, or red-hot iron is introduced in the gaseous mixture. When there is a large proportion of  $H_2S$ , the oxidation is very rapid, and the mixture explodes and burns.

Piria's explanation cannot be applied to the *Bocca della Solfatara*, where the presence of  $H_2S$  cannot be detected either by the sense of smell, or by the lead-acetate test-papers. In the "Memorie Geologiche sulla Campania (*Rendiconti della Reale Accademia delle Scienze di Napoli*, 1849, p. 137) Prof. A. Scacchi, after having opposed Piria's opinion, gives the following explanation: "I believe the increase of the vapoury cloud due to the carbonic acid produced in the combustion of the tinder, its affinity for water causing the precipitation of the invisible vapour, and thus producing a mist." According to Prof. Scacchi, in the presence of large quantities of aqueous vapour, and at the temperature of the fumarole, carbonic dioxide would act as hydrochloric acid gas which fumes in ordinary air.

Since 1849 no one (as far as I have gathered) has suggested any new opinion or tried some experiment to explain the phenomenon in question. I thought it would be interesting to test experimentally at the Solfatara the opinion of Prof. Scacchi. I was inclined to believe that, if at the ordinary temperature carbonic dioxide does not condense aqueous vapour from the air, there was very little probability that the condensation would be caused at temperatures as high as those of the vapours issuing from the *Bocca* of the Solfatara (about  $90^\circ$  Centigrade externally); the action of flames or smouldering bodies in augmenting the vapoury cloud appeared to me as chiefly due to the condensation around the minute particles of soot or dust produced during the combustion.

The following experiments were done during a clear day, when abundant vapours were issuing from the large fumarole:—

1. A Wolff bottle (1 litre capacity), from which a constant current of carbon dioxide was obtained (by pouring dilute hydrochloric acid on marble fragments), was placed on the ground inside the fumarole. The cloud of vapour augmented.

2. By means of a caoutchouc tube the  $CO_2$  from the generator was conducted near the hottest invisible vapour. This vapour became interspersed with cloudlets of condensed vapour, and the cloudy pillar outside the *Bocca* greatly augmented.

3. A large bottle (of about 15 litres capacity) filled with carbon dioxide was brought inside the cavity, and the  $CO_2$  poured out. The effect was most striking outside by the voluminous, but not immediate, outbursts of cloudy vapour.

4. With bellows of the kind used for sulphuring vines, I blew sulphur dust inside the cavity. This caused the production of great volumes of visible vapour. The same effects were produced every time that minutely divided bodies (wheaten flour, oxide of magnesia, chalky dust, &c.) were blown, or thrown, inside the cavity or near the invisible vapour.

5. The effect was very striking when the action of the carbon dioxide (from the Wolff bottle) was combined with the action of the sulphur dust.

6. A small alcohol flame augmented the cloudiness of the vapour.

7. The smoky flame of burning naphthalene acted much more powerfully than the alcohol flame.

From these experiments, which (with the exception of 3 and 6) were often repeated, the following conclusions may be drawn:—

1. Carbon dioxide helps to condense watery vapour.

2. Minute bodies suspended in air are a powerful cause (the principal cause, as Coulier and Aitken have shown) in the condensation of aqueous vapour.

3. The action of flames, or of incandescent bodies, in augmenting so remarkably the volumes of visible vapour rising from the fumaroles of the Solfatara must be ascribed both to the carbon dioxide and to the minute carbonaceous particles set free during the combustion.

Of these conclusions the first requires to be confirmed by careful laboratory experiments.

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#### STATE OF THE ATMOSPHERE WHICH PRODUCES THE FORMS OF MIRAGE OBSERVED BY VINCE AND BY SCORESBY

IN 1881, when I wrote the article *Light* for the *Encyc. Brit.*, I had not been able to meet with any detailed calculations as to the probable state of the atmosphere when multiple images are seen of objects situated near the horizon. I had consulted many papers containing what are called "general" explanations of the phenomena, but had found no proof that the requisite conditions could exist in nature:—except perhaps in the case of the ordinary mirage of the desert, where it is obvious that very considerable temperature-differences may occur in the air within a few feet of the ground. But this form of mirage is essentially unsteady, for it involves an unstable state of equilibrium of the air. In many of Scoresby's observations, especially that of the solitary inverted image of his father's ship (then thirty miles distant, and of course far below the horizon), the details of the image could be clearly seen with a telescope, showing that the air must have been in equilibrium. The problem seemed to be one well fitted for treatment as a simple example of the application of Hamilton's *General Method in Optics*, and as such I discussed it. The details of my investigation were communicated in the end of that year to the Royal Society of Edinburgh, and will, I hope, soon be published. The paper itself is too technical for the general reader, so that I shall here attempt to give a sketch of its contents in a more popular form. But a curious little historical statement must be premised.

It was not until my calculations were finished that I found a chance reference to a great paper by Wollaston (*Phil. Trans.* 1800). I had till then known only of Wollaston's well-known experiment with layers of different liquids in a small vessel. But these, I saw, could not reproduce the proper mirage phenomena, as the rays necessarily enter and emerge from the transition strata by their *ends* and not by their lower *sides*. This experiment is by no means one of the best things in Wollaston's paper, so far at least as the immediate object of the paper is concerned. That so much has been written on the subject of mirage during the present century, with only a casual reference or two to this paper, is most surprising. It may perhaps be accounted for by the fact that Wollaston does not appear to have had sufficient confidence in his own results to refrain from attempting, towards the end of his paper, a totally different (and untenable) hypothesis, based on the effects of aqueous vapour. Be the cause what it may, there can be no doubt that the following words of Gilbert were amply justified when they were written, early in the present century:—"In der That ist Wollaston der Erste und Einzige, der die *Spiegung aufwärts* mit Glück zu erklären unternommen hat." For his methods are, in principle, perfectly correct and suffi-



ciently comprehensive; while some of his experiments imitate closely the state of the air requisite for the production of Vince's phenomena. Had Wollaston only felt the necessary confidence in his own theory, he could hardly have failed to recognise that what he produced by the extreme rates of change of temperature in the small air-space close to a red-hot bar of metal, could be produced by natural rates of change in some ten or twenty miles of the atmosphere:—and he would have deserved the credit of having completely solved the problem.

Six months after my paper was read, another happy chance led me to seek for a voluminous paper by Biot, of which I had seen no mention whatever in any of the books I had previously consulted. The probable reason for the oblivion into which this treatise seems to have fallen is a curious one. It forms a considerable part of the volume for 1809 of the *Mém. de l'Institut*. But in the three first great libraries which I consulted, I found this volume to be devoid of plates. In all respects but this, each of the sets of this valuable series appeared to be complete. Without the figures, which amount to no less than sixty-three, it is practically impossible to understand the details of Biot's paper. The paper was, however, issued as a separate volume, "*Recherches sur les Réfractions extraordinaires qui ont lieu près de l'horizon*" (Paris, 1810), which contains the plates, and which I obtained at last from the Cambridge University Library. I have since been able to procure a copy for the Edinburgh University Library. Biot's work is an almost exhaustive one, and I found in it a great number of the results which follow almost intuitively from my methods:—such as the possible occurrence of *four* images, under the conditions usually assumed for the explanation of the ordinary mirage; the effects of (unusual) refraction on the apparent form of the setting sun; &c. But it seems to me that Biot's long-continued observations of the phenomena as produced over extensive surfaces of level sand at Dunkirk have led him to take a somewhat onesided view of the general question. And, in particular, I think that his attempted explanation of Vince's observations (so far as I am able to understand it; for it is very long, and in parts extremely obscure and difficult, besides containing some singular physical errors) is not satisfactory. His general treatment of the whole question is based to a great extent upon the properties of caustics, though he mentions (as the *courbe des minima*) the "locus of vertices" which I had employed in my investigations, and which I think greatly preferable. There can be no doubt, however, that Biot's paper comes at least next in point of importance to that of Wollaston:—though in his opinion Wollaston's work was complete only on the physical side of the problem. "*Sous le rapport de la physique son travail ne laisse rien à désirer.*"

But, if the chief theoretical papers on the subject have thus strangely been allowed to drop out of notice, the case is quite different with several of those which deal with the observed phenomena. Scoresby's *Greenland*, his *Arctic Regions*, and his *Voyage to the Northern Whale Fishery*, are still standard works; and in them, as well as in vols. ix. and xi. of the *Trans. R.S.E.*, he has given numerous careful drawings of these most singular appearances. The explanatory text is also peculiarly full and clear, giving all that a careful observer could have been expected to record. It is otherwise with the descriptions and illustrations in Vince's paper (*Phil. Trans.* 1799). In fact the latter are obviously not meant as *drawings* of what was seen; but as *diagrams* which exhibit merely the general features, such as the relative position and magnitude of the images:—the details being filled in at the option of the engraver. That such was the view taken by Brewster, is obvious from the illustrations in his *Optics* (*Library of Useful Knowledge*), for while one of Scoresby's drawings is there *copied*, one of Vince's is treated in a highly imaginative style by the reproducer.

Scoresby's sketches are composite, as he takes care to tell the reader, so that in the reproduction below (Fig. 1) I have simply selected a few of the more remarkable portions which bear on the questions to be discussed. It is to be

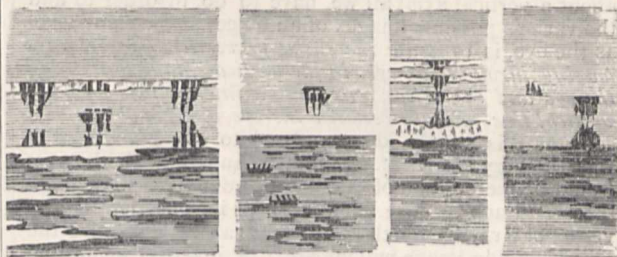


FIG. 1.

remarked that the angular dimensions of these phenomena are always of *telescopic* magnitude:—the utmost elevation of an image rarely exceeding a quarter or a third of a degree.

Because the rays concerned are all so nearly horizontal, and (on the whole) *concave* towards the earth; and because they must also have on the whole considerably greater curvature than the corresponding part of the earth's surface, especially if they happen to have points of contrary flexure; it is clear that, for a preliminary investigation, we may treat the problem as if the earth were a plane. This simplifies matters very considerably, so that definite numerical results are easily obtained; and there is no difficulty in afterwards introducing the (comparatively slight) corrections due to the earth's curvature. But these will not be farther alluded to here.

Of course I began, as almost every other person who has thought of the production of the ordinary mirage of the desert must naturally have begun, by considering the well-known problem of the paths of projectiles discharged from the same gun, with the same speed but at different elevations of the piece. This corresponds, in the optical problem, to the motion of light in a medium the square of whose refractive index is proportional to the distance from a given horizontal plane. Instead, however, of thinking chiefly of the different elevations corresponding to a given range, I sought for a simple criterion which should enable me to decide (in the optical application) whether the image formed would, in any particular case, be a direct or an inverted one. And this, I saw at once, could be obtained, along with the number and positions of the images, by a study of the form of the locus on which lie the *vertices* of all the rays issuing from a given point. Thus, in the ballistic problem, the locus of the vertices of all the paths from a given point, with different elevations but in the same vertical plane, is an ellipse.

Its minor axis is vertical, the lower end being at the gun; and the major axis (which is twice as long) is in the plane of projection. Now, while the inclination of the piece to the horizon is less than  $45^\circ$ , the vertex of the path is in the *lower* half of this ellipse, where the tangent leans forward from the gun; and in this case a small increase of elevation *lengthens* the range, so that the two paths do not intersect again above the horizon. In the optical problem this corresponds to an *erect* image. But, when the elevation of the piece is greater than  $45^\circ$ , the vertex of the path lies in the *upper* half of the ellipse, where the tangent leans back over the gun; and a small increase of elevation *shortens* the range. Two contiguous paths, therefore, intersect one another again above the horizon. And, in the optical problem, this corresponds to an *inverted* image. In symbols, if the eye be taken as origin and the axis of  $x$  horizontal, there will be a direct image for a ray at whose vertex  $dy/dx$  and  $x$  (in the curve of vertices) have the *same* sign, an inverted image when the signs are different.



Hence, whatever be the law of refractive index of the air, provided only it be the same at the same distance from the earth's surface, (*i.e.* the surfaces of equal density parallel planes, and therefore the rays each symmetrical about a vertical axis) all we have to do, in order to find the various possible images of an object at the same level as the eye, is to draw the curve of vertices for all rays passing through the eye, in the vertical plane containing the eye and the object, and find its intersections with the vertical line midway between the eye and the object. As soon as this simple idea occurred to me, I saw that it was the very kernel of the matter, and that all the rest would be mere detail of calculation from particular hypotheses. Each of the intersections in question is the vertex of a ray by which the object can be seen, and the corresponding image will be erect or inverted, according as the curve of vertices leans from or towards the eye at the intersection. Thus, in Fig. 2, let E be the eye, and

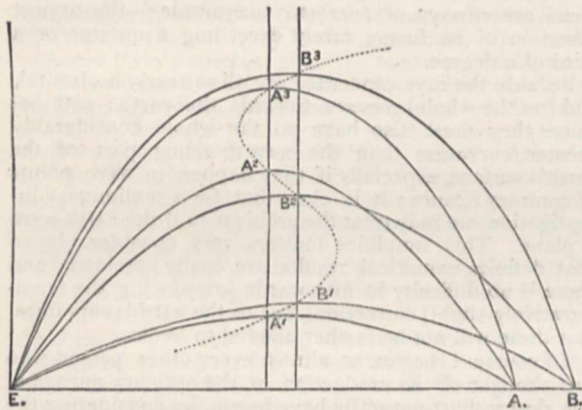


FIG. 2.

the dotted line the curve of vertices for all rays in the plane of the paper, and passing through E. Let A be an object at the level of the eye,  $A^1 A^2 A^3$  the vertical line midway between E and A. Then  $A^1, A^2, A^3$  are the vertices of the various rays by which A can be seen. If we make the same construction for a point B, near to A, we find that whereas the contiguous rays through  $A^1, B^1$  and through  $A^3, B^3$  do not intersect, those through  $A^2, B^2$  do intersect. At  $A^1$  and  $A^3$  the curve of vertices leans from the eye, and we have erect images; at  $A^2$  it leans back towards the eye, and we have an inverted image. And thus, if this curve be continuous, the images will be alternately erect and inverted. The sketch above is essentially the same as one given by Vince, only that he does not employ the curve of vertices. If the object and eye be not at the same level, the construction is not quite so simple. We must now draw a curve of vertices for rays passing through the eye, and another for rays passing through the object. Their intersections give all the possible vertices. (This construction of course gives the same result as the former, when object and eye are at the same level.) But the images are now by no means necessarily alternately erect and inverted, even though the curve of vertices be continuous. However, I merely note this extension of the rule, as we shall not require it in what follows.

I then investigated the form of the curve of vertices in a medium in which the square of the refractive index increases by a quantity proportional to the square of the distance from a plane in which it is a minimum, and found that (under special circumstances, not however possible in air) three images could be produced in such a medium. But the study of this case (which I could not easily explain here without the aid of mathematics) led me on as follows.

As the curvature of a ray is given by the ratio of the

rate of change of index per unit of length perpendicular to the ray, to the index itself (a result which I find was at least virtually enunciated by Wollaston); and as all the rays producing the phenomena in question are very nearly horizontal:—*i.e.* perpendicular to the direction in which the refractive index changes most rapidly:—their curvatures are all practically the same at the same level. Hence if the rate of diminution of the refractive index, per foot of ascent, were nearly constant, through the part of the atmosphere in which the rays travel, the rays we need consider would all be approximately arcs of equal circles; and the curve of vertices would (so far as these rays are concerned) lean wholly from the eye; being, in fact, the inferior part of another equal circle which has its lowest point at the eye. Hence but one image, an erect one, would be formed; but it would be seen elevated above the true direction of the object. This is practically the ordinary horizontal refraction, so far as *terrestrial* objects on the horizon are concerned. The paths of the various rays would be of the form in Fig. 3 (the drawing

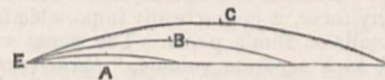


FIG. 3.

is, of course, immensely exaggerated) and the locus of vertices, ABC, obviously leans from the eye. But now suppose that, below a stratum of this kind, there were one of constant density, in which of course the rays would be straight lines. Then our sketch takes the form Fig. 4 (again exaggerated); each of the portions of the ray in

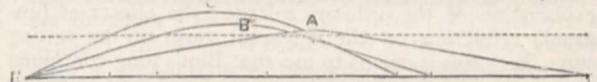


FIG. 4.

the upper medium being congruent to the corresponding one in the former figure (when the two figures are drawn to the same scale), but pushed farther to the right as its extremities are less inclined to the horizon. In its new form the curve of vertices ABC leans back towards the eye, and we have an inverted image. The lower medium need not be uniform as, for simplicity, we assumed above. All that is required is that the rate of diminution of density upwards shall be less in it than in the upper medium.

Those who have followed me so far will at once see that, as a more rapid decrease of density, commencing at a certain elevation, makes the curve of vertices lean back, so a less rapid decrease (tending to a "stationary state") at a still higher elevation will make the curve of vertices again lean forward from the eye. I need not enlarge upon this.

Thus to repeat:—the conditions requisite for the production of Vince's phenomenon, at least in the way conjectured by him, are, a stratum in which the refractive index diminishes upwards to a nearly stationary state, and below it a stratum in which the upward diminution is either less or vanishes altogether. The former condition secures the upper erect image, the latter the inverted image and the lower direct image.

In my paper read to the *Royal Society of Edinburgh* I have given the mathematical details following from the above statement; and have made full calculations for the effect of a transition stratum, such as must occur between two uniform strata of air of which the upper has the higher temperature. From Scoresby's remarks it appears almost certain that something like this was the state of affairs when the majority (at least) of his observations were made. When two masses of the same fluid, at different temperatures, rest in contact; or when two fluids of different refractive index, as brine and pure



water, diffuse into one another; the intervening layer must have a practically "stationary" refractive index at each of its bounding surfaces, and a stratum of greatest rate of change of index about midway between them. The exact law of change in the stratum is a matter of comparatively little consequence. I have assumed (after several trials) a simple harmonic law for the change of the square of the refractive index within the stratum. This satisfies all the above conditions, and thus cannot in any case be very far from the truth. But its special merit, and for my purpose this was invaluable, is that it leads to results which involve expressions easily calculated numerically by means of Legendre's Tables of Elliptic Integrals. This numerical work can be done once for all, and then we can introduce at leisure the most probable hypotheses as to the thickness of the transition stratum, the height of its lower surface above the ground, and the whole change of temperature in passing through it. I need not now give the details for more than one case, and I shall therefore select that of a transition stratum 50 feet thick, and commencing 50 feet above the ground. From the physical properties of air, and the observed fact that the utmost angular elevation of the observed images is not much more than a quarter of a degree, we find that the upper uniform layer of air must under the conditions assigned be about  $7^{\circ}$  C. warmer than the lower. Hence by the assumed law in the stratum, the maximum rise of temperature per foot of ascent (about the middle of the transition stratum) must be about  $0.2$  C. per foot. Such changes have actually been observed by Glaisher in his balloon ascents, so that thus far the hypothesis is justified. But we have an independent means of testing it. The form of the curve of vertices is now somewhat like the full lines in the following cut, Fig. 5:—

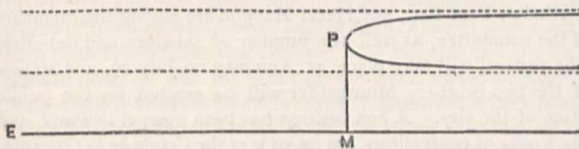


FIG. 5.

where E is the eye, and the dotted lines represent the boundaries of the transition stratum. It is clear that, if PM be the vertical tangent, there can be but one image of an object unless its distance from E is at least twice EM. This will therefore be called the "Critical distance." If the distance be greater than this there are three images:—one erect, seen directly through the lower uniform stratum—then an inverted one, due to the diminution of refractive index above the lower boundary of the transition stratum—and finally an erect image, due to the approximation to a stationary state towards the upper boundary of that stratum. Now calculation from our assumed data gives EM about six miles, so that the nearest objects affected should be about twelve miles off. Scoresby says that the usual distance was from ten to fifteen miles. Thus the hypothesis passes, with credit, this independent and severe test. A slight reduction of the assumed thickness of the transition-stratum, or of its height above the ground, would make the agreement exact.

All the phenomena described in Vince's paper of 1799, as well as a great many of those figured in Scoresby's works, can easily be explained by the above assumptions. Scoresby's remarkable observation of a single inverted image of his father's ship (when thirty miles off, and of course far below the horizon) requires merely a more rapid diminution of density at a definite height above the sea. His figure is the second in Fig. 1 above. But Scoresby figures, as above shown, several cases in which two or more inverted images, without corresponding erect ones, were seen above the ordinary direct image. The natural ex-

planation is, of course, a series of horizontal layers of upward diminishing density and without a "stationary state" towards their upper bounding planes. I find that, by roughly stirring (for a very short time) a trough in which weak brine below is diffusing into pure water above, we can reproduce this phenomenon with great ease. In fact, when temporary equilibrium sets in, the fluids are arranged in a number of successive parallel strata with somewhat abrupt changes of density.

But the mathematical investigation, already spoken of, shows that it is quite possible that there may be layers tending to a stationary state without any corresponding visible images.

This depends on the fact that, while the inverted image (due to the lower part of a stratum) is *always* taller than the object seen directly (though not much taller unless the object is about the critical distance); the numerical calculation shows that the erect image is in general extremely small, and can come into notice only when the object is not far beyond the critical distance. Thus there may have been, in *all* of Scoresby's observations (though he has only occasionally noticed and depicted them) an erect image above each inverted one, but so much reduced in vertical height as to have been invisible in his telescope, or at least to have formed a mere horizontal line so narrow that it did not attract his attention. The greatly superior number of inverted images, compared with that of the direct ones, figured by Scoresby may thus be looked upon as another independent confirmation of the approximate correctness of the hypothetical arrangement we have been considering.

To obtain an experimental repetition of the phenomena in the manner indicated by the above hypothesis, a tank, with parallel glass ends, and about 4 feet long, was half-filled with weak brine (carefully filtered). Pure water was then cautiously introduced above it, till the tank was nearly filled. After a few hours the whole had settled down into a state of slow and steady diffusion, and Vince's phenomenon was beautifully shown. The object was a metal plate with a small hole in it, and a lamp with a porcelain globe was placed behind it. The hole was triangular, with one side horizontal (to allow of distinction between direct and inverted images), and was placed near one end of the tank, a little below the surface-level of the unaltered brine, the eye being in a corresponding position at the other end. A little vertical adjustment of object and eye was required from time to time as the diffusion progressed. The theoretical results that the upper erect image is usually much less than the object, and that it is seen by slowly convergent rays, while the inverted image is larger than the object and is seen by diverging rays, were easily verified.

To contrast Wollaston's best-known experiment with this, a narrow tank with parallel sides was half-filled with very strong brine, and then cautiously filled up with pure water. (The strong brine was employed to make up, as far as possible, for the shortened path of the rays in the transition stratum.) Phenomena somewhat resembling the former were now seen, when object and eye were nearly at the same distance apart as before, and the tank about half-way between them. But in this case the disparity of size between the images was not so marked—the upper erect image was always seen by diverging rays, the inverted image by rays diverging or converging according as the eye was withdrawn from, or made to approach, the tank. In this case, the curvature of each of the rays in the vessel is practically constant, but is greatest for the rays which pass most nearly through the stratum of most rapid change of refractive index. Hence, when a parallel beam of light fell horizontally on the tank and was received on a sufficiently distant screen, the lower boundary of the illuminated space was blue—and the progress of the diffusion could be watched with great precision by the gradual displacement of this blue band.



I propose to employ this arrangement for the measurement of the rate of diffusion, but for particulars I must refer to my forthcoming paper.

Wollaston's experiment with the red-hot poker was probably, his experiment with the long red-hot bar of iron almost certainly, similar to that above described with the long tank and the weak brine; and *not* to that with the short tank, though the latter is usually cited as Wollaston's contribution to the explanation of the Vince phenomenon. We have seen how essentially different they are, and that the latter does not correspond to the conditions presented in nature.

P. G. TAIT

### NOTES

THE Council of the Scottish Meteorological Society are soliciting subscriptions, however small, for the proposed Ben Nevis Observatory. It is essential to the success of this important national undertaking that the buildings should be erected during the present summer, and several thousand pounds are required before operations can be commenced. A considerable sum has already been received in liberal subscriptions from a few individuals, but not nearly enough for the purpose. We trust that many of our readers will send what they can to the Scottish Meteorological Society, Edinburgh.

DR. WILD, president of the International Circumpolar observation parties, announces that in conformity with the request of several Governments, the observations now going on round the Pole will not be prolonged beyond the time originally fixed, viz. September, and that all the parties, if not prevented by ice, will be back within that month.

A LETTER read at the Paris Geographical Society states that P. Vidal, French missionary to Samoa, has discovered the remains of La Perouse and his unfortunate companions.

THE Rev. S. J. Perry, S. J., has lately been elected a Corresponding Member of the Accademia dei Lincei.

DR. HENRY SCHLIEMANN has been elected an Honorary Fellow of Queen's College, Oxford.

LAST week we announced that a baronetcy had deservedly been conferred on Dr. William Chambers, and this week we regret to announce the death of the veteran publisher in his eighty-fourth year. As the head of the firm of Messrs. W. and R. Chambers, he has through a long life done splendid service in the spread of education, and of a knowledge of science. In his "Information for the People," his "Tracts," his text-books of science, among the first of their kind, and by various other means, he did good pioneer work in scientific literature and education.

In reference to our note last week (p. 63), a correspondent writes that the American table at Naples is being used by its first occupant, Dr. E. B. Wilson, of the Johns Hopkins University, Baltimore. Dr. Wilson has been working during a part of the year at Cambridge on early mammalian embryology, and at Naples his work will probably be either on certain points in the development of some of the Coelenterata or upon the embryology of the Dicegemidæ as available material permits. Williams College, Mass., which holds the American table, receives a brief course of lectures from each worker whom it appoints to the privileges of the Naples Station.

ON the evening of Friday last week several tornadoes swept over the states of Minnesota, Wisconsin, Illinois, and Missouri, which were exceptionally destructive to life and property even for that tornado-troubled region. It is reported that 83 persons have been killed and 340 injured, many of them fatally, and a very large number of houses reduced to ruins. Of these torna-

does the most terrible in its destructiveness would appear to have been the one which passed over Racine in the south-east of Wisconsin, killing 25 and injuring 100 persons, and wrecking 150 buildings. The path of the tornado was about 400 yards wide and half a mile long, and all buildings, particularly those in the central line of its path, collapsed into mere masses of ruins. Waggons and other movable articles were blown into Lake Michigan, over which the tornado passed on leaving the town, the whirling columns of clouds and the violent commotions of the lake presenting a grand and impressive spectacle. The recently published "Professional Papers of the Signal Service, No. VII." show that the region over which these tornadoes passed is comprehended within that portion of the United States where tornadoes are of most frequent occurrence.

MR. BRUNLEES, the President of the Institution of Civil Engineers, has sent out invitations for a *conversazione* at the South Kensington Museum on Wednesday, the 30th inst.

ON Saturday last, May 19, the Essex Field Club held its first meeting of the session. The party, nearly ninety in number, alighted at Theydon Bois Station on the Ongar branch of the Great Eastern Railway, and proceeded through Epping Forest to Ambresbury Banks, where they were met by Sir T. Fowell Buxton. The party was then conducted through the splendid park belonging to the Copt Hall Estate, and finally assembled at Warlies, Waltham Abbey, the seat of Sir Fowell Buxton, who had kindly invited the Club for the occasion. In the course of the evening a paper on "English Plant Names" was read by Mr. J. Britten, F.L.S.

THE Paris Aeronautical Exhibition will be opened at Trocadéro on June 5 and close on the 18th. MM. Janssen, Berthelot, Paul Bert, and Hervi Mangon are among the members of the committee, as well as a number of senators and deputies. The festival will take place at Annonay on July 29, and statues of the two brothers Montgolfier will be erected on the public place of the city. A competition has been opened in Paris, and the works of competitors are on view at the Cercle de la Librairie. The jurymen, mostly members of the Academy of Beaux Arts, will give their award on Saturday next. The height of the monument and pedestal will be 7 metres. The prize is 3000 francs for the plaster model to be exhibited at Annonay on July 29, and 40,000 francs for the bronze. The marble for the pedestal will be given by Government. A public banquet will be given in Paris, M. Gaston Tissandier being in the chair.

ON April 29, at 10.30 p.m., a brilliant meteor was observed in Jondalen in Norway. It appeared in the east, and went in a southerly direction, where it passed out of sight. Its size to the eye was about the same as the moon's, while its shape appeared to be conic. The colour of its track was deep red, and it shone so brilliantly that the smallest objects could be seen on the ground. It lasted several seconds, and disappeared behind some mountains.

ON the 13th, at 8 o'clock in the evening, a large meteor was observed at Epinal, travelling from south-east to north-west; it had a disk which has been estimated at a decimeter. The tail was of a pinky colour; a noise from explosion was heard. It was also observed at Mulhausen.

THE Reports on the Public Gardens and Plantations in Jamaica are becoming yearly of more importance. That for the year ending September 30 last is now before us. Mr. Morris opens his report by bearing testimony to the liberality of the Steamship and Railway Companies in conveying plants free of charge to the different ports and railway stations. "By these means," it is stated, "districts, formerly beyond the reach of the Public Gardens, have been able to obtain plants as conveniently and as



cheaply as if they were in the neighbourhood of Kingston." It is stated that a drought, caused chiefly by the failure of the May rains coming after a succession of dry months with parching winds, had a prejudicial effect on all agricultural operations. Mr. Morris says: "It is a subject of common remark amongst old planters that the 'seasons,' or the periodical rains which have hitherto fallen with great regularity and copiousness during the months of May and October of each year, are becoming more and more uncertain and irregular, and the effects of these conditions are clearly shown in the precariousness of the agricultural products affected by them. These remarks apply chiefly to the southern slopes of the Blue Mountains, and to such other districts stretching south and west where coffee and provisions are being chiefly raised." Under the head of "Cinchona Plantation," the cultivation of which plants has become an object of special attention in Jamaica, Mr. Morris reports very fully. He says: "In order to test the commercial value of Jamaica grown bark, no better plan could be followed than to send it in lots to the open market and place it in competition with barks from other countries. That it has so satisfactorily stood this test and brought in a large return on the outlay, and, moreover, that the results of the sales have induced cinchona planting to be undertaken in the island by private enterprise with energy and success, are matters for which the Government no less than the general public are to be congratulated." "During the past year chief attention has been given to the successful introduction of *Cinchona Ledgeriana* and its establishment as a cultivated plant in Jamaica. In addition to the plants established on the Government plantations, several thousands have been distributed amongst private planters, and each lot of these will doubtless form a nucleus from whence seeds and cuttings may hereafter be obtained, and thus prove most valuable acquisitions to private plantations." A few plants of the now well known cuprea bark, *Remijia pedunculata*, have been raised from seed received from Bogota, and are being tried in order to test the value of the bark under cultivation. An attempt is about to be made to manufacture cinchona febrifuge in the island in a similar way to what is being now done so successfully in the East Indies. By this means a valuable and cheap preparation will be available for use among the poorer classes. Besides the cinchonas the cultivation of jalap and various other economic plants has received attention during the year, so that we have evidence that a good deal of really useful work is being carried on by Mr. Morris in Jamaica.

A WRITER in the *North China Herald* on the history of gunpowder in China asserts that this explosive was known in the seventh century of our era. The alchemists of the Han dynasty, and subsequently in the fourth and following centuries, worked with saltpetre and sulphur, as well as cinnabar, red oxide of lead, and other common compounds. But in the seventh century we find gunpowder used to make a crackling sound and to afford an agreeable sight to the court of Sui Yang-ti, the emperor of that time. The earliest exhibitions of fireworks mentioned in Chinese history belong to that date. The substances used in the composition of gunpowder are all native to China, and the writer appears to prove conclusively that the Arabs derived the art of firework making, as well as gunpowder, from the Chinese. The discovery once made, the Chinese alchemists, owing to the badness of their hypotheses and the futility of their aims, were slow at improvement. But the doctors of the Arab colonies in China carried to Bagdad the germs of the Chinese discoveries, and there they were elaborated into new forms. In short, in many arts and sciences the Arabs learnt from China, and, assisted by Nestorians, Jews, and Greeks, improved on what they learned. In course of years, cannon, matchlocks, and shells for use in sieges were brought to China from Mohammedan countries. There are faint traces in the eleventh century of rude

firearms; in the twelfth and thirteenth centuries the records of their use in the Chinese wars become frequent and distinct. The Golden Tartars, in their wars with South China in the twelfth century, used cannon which they called "heaven-shaking thunder." In an iron tube was placed powder which was "set fire to, and would burn down half a square *li* of houses and pierce a coat of mail made of iron rings." It is expressly stated that Genghis Khan, the Mongol conqueror, used cannon in his wars. Kublai Khan also used these weapons at a siege celebrated in Chinese history—that of Siang-yang. Hearing, it is said, the sound of the explosion, which shook the sky, and seeing that the balls entered seven feet into the earth, the Chinese defenders of the city capitulated. It is clear that China owed its knowledge of artillery to the Mohammedans. In the fourteenth century commenced the European intercourse with China, which then abandoned the Arabs, and took the Portuguese as teachers in the construction of weapons of warfare.

NEWS from Iceland states that from the 12th to the 21st of March there were violent volcanic water eruptions.

A REUTER'S telegram from Hong Kong *via* San Francisco announces the completion of the telegraph line between Canton and that colony. This is the second great line in China, and appears to have been constructed wholly by native merchants in Canton, who found the want of early communication with western markets in their commercial transactions. Vigorous preparations are also being made for the most formidable undertaking of this nature that has yet been attempted in China, viz. a line connecting Peking with Canton. According to the latest information an expeditionary party has arrived at Shanghai to conduct the necessary surveys. It will proceed first to Soochow, and there, under the escort of 200 troops, will commence its work, proceeding southward.

A NUMBER of students at the Ecole des Mines of France will during the summer make an excursion to the Arctic regions. A steamer, in charge of a Norwegian Arctic hunter, will bring the party to Thronhjelm and Hammerfest, and thence to Spitzbergen, which will be examined during a fortnight's stay. The Naturalistic Museum of Paris sends two *savants* with the party.

THE additions to the Zoological Society's Gardens during the past week include two Green Monkeys (*Cercopithecus callitrichus* ♂ ♀) from West Africa, presented by Mr. Thos. H. Dixon; a Long-eared Owl (*Asio otus*), British, presented by the Rev. H. D. Grantham; a Smooth Snake (*Coronella levis*), European, presented by Mr. W. H. B. Pain; seven Black and Yellow Cyclopus (*Cyclodus nigro-luteus*) from Tasmania, presented by Baron Ferdinand von Mueller, C.M.Z.S.; a Proteus (*Proteus anguinus*), European, presented by Miss Maud Howard; a Sea Crayfish (*Palinurus vulgaris*), British Seas, presented by Messrs. Milestone and Staniforth; three Green-winged Doves (*Chalcophaps indica*) from India, a Herring Gull (*Larus argentatus*), British, deposited; a King Vulture (*Gypagus papa*) from Tropical America, purchased; a Cabot's Tragopan (*Cerionis caboti* ♀) from North-West China, received on approval.

#### OUR ASTRONOMICAL COLUMN

THE COMET OF 1707.—The elements of this comet's orbit, as calculated by Lacaille and Struyck, bear a certain degree of resemblance to those of the comet discovered by De Vico at Rome on February 20, 1846 (1846 IV. of our catalogues), to which Van Deinsse's definitive calculation assigns a period of revolution of 73 years. The interval between the perihelion passages in 1707 and 1846 would give two periods of 69.1 years; there is consequently a sufficient reason for examining how far the elements of the comet of 1707 represent the observations. It appears to have been discovered by Manfredi at Bologna on November 25, and the place given in the *Mémoires* of the Paris



Academy for that date was in R.A.  $308^{\circ} 25'$ , Decl.  $-24^{\circ} 17'$ . Pingré in his *Cométographie* mentions that according to Struyck this position is erroneous, and that ten minutes should be added to the declination and five to the right ascension as printed in the *Mémoires*, adding that if Lacaille has used the Bologna observation his orbit would be less accurate than that of Struyck. The Bologna observers Manfredi and Stancari found the comet on November 25, in the same field of view of an 8-foot telescope, with two stars, the distance between which they estimated at 9'. At 7h. 14m. 47s. apparent time the centre of the comet was in the right line joining these stars, and its distance from the northernmost star was one-third of the distance between them. It is easy to see from the rough position given, that the stars in question are Piazzis XX., 296 and 298, and carrying back his places, we have for the position of the comet referred to the mean equinox of 1708.0, R.A.  $307^{\circ} 49' 3$ , Decl.  $-23^{\circ} 44' 1$ . The equation of time was 12m. 37s. subtractive from apparent time, and hence the Greenwich mean time of observation was November 25.26163. The place calculated from Lacaille's orbit, first published in his "*Leçons d'Astronomie*," differs  $+7' 2$  in R.A. and  $+5' 6$  in Decl., so that it is evident he did not use the position as erroneously deduced in the *Mémoires*. The agreement of his elements with the Paris observation on December 17 is fairly good; there is a much larger deviation from the approximate places determined at Bologna, on January 13 and 17; but these observations of Manfredi and Stancari are probably affected with very material errors, as such is certainly the case with the deduced position for the night of discovery. So far as can be judged from this partial comparison of Lacaille's elements with observation, the hypothesis of identity of the comet of 1707 with that of 1846 is not supported, but the observations of the former may deserve further discussion.

THE TRANSIT OF VENUS.—Prof. C. A. Young has published his observations of all four contacts in the late transit of Venus, made at the Halsted Observatory, Princeton, N.J., with the 23-inch equatorial, and a power of 160. At the two internal contacts the aperture was diminished to  $5\frac{1}{2}$  inches, "in order to make the observations comparable as far as possible with those of the various government expeditions," but at the external contacts the full aperture was employed; a polarising helioscope was attached. We have compared the times given by Prof. Young with those calculated from the reduction-equations published in this column, in the formation of which it was the main object to get geometrical contacts. It has been previously mentioned that there was a close agreement between prediction and observation in the case of the results obtained at Harvard College, and the following are the small differences (calc.—obs.) for Prof. Young's:—

I. - 16s. ... II. + 3s. ... III. - 15s. ... IV. - 4s.

THE BRITISH ASSOCIATION CATALOGUE OF STARS.—We lately remarked, not without some surprise, that a copy of this work was priced in a continental list of second-hand books at the high figure of 12*l.* 10*s.*, or about three times the cost at its publication in 1845. Such a fact naturally induces the query, Is there occasion for a new general catalogue of the principal fixed stars, or, say, of stars within the limit of naked-eye vision? It is a question upon which there will probably be a wide difference of opinion, and it is one that it would be of interest to discuss.

### GEOGRAPHICAL NOTES

THE last number of the *Verhandlungen der Gesellschaft für Erdkunde* of Berlin contains a paper by Prof. Brauns, late of Japan, on the Island of Yezo. The writer agrees with Mr. Keane and other ethnologists that the Ainos are a totally different race from the Japanese. The number of these people in Yezo and the Kuriles is given by the Japanese Government as 18,000, but many authors place the number as high as 50,000. In Saghalin there are 10,000 to 12,000, and if those in the southern part of Kamschatka who are living under Russian rule are included, the total number of the race would probably be from 60,000 to 70,000. In the same issue the indefatigable explorer of the Philippines, Herr Jäger, describes briefly a recent journey through Luzon. An interesting communication also is a list of the papers published by the Geographical Society of Tokio in its volume for 1880. This Society is composed, we believe, almost wholly of natives, and its papers are printed in

Japanese. There appear to have been in all thirty-eight communications of one kind or another; the writers or translators (for some of the papers are apparently translations from others in European languages) are in all cases Japanese. Among the papers are several on the history and geography of Okinawa, as the Japanese call the Loochoo group; the climate of Peking; Japanese intercourse with foreign countries in the middle ages; a journey to Vladivostock; the history of geography in Japan; history and geography of Persia by a Japanese who had travelled through the country; description of Australia; description of a voyage in the Persian Gulf; of a journey on the Khirgiz steppes; ancient Japanese geographical names; description of Saghalin; on the absence of precious stones in Japan, &c., &c. Some of these papers would hardly meet with a favourable reception from the Council of the Royal Geographical Society; but in Japan they are listened to and read afterwards in their printed form by hundreds of people who have never left their own country, and who possess but a very small geographical literature. When this is remembered, the list will appear not only a creditable one to the travellers, but also a most useful one for the spread of geographical knowledge in Japan, which after all is the purpose of the Society.

THE annual report of Mr. Tremlett, the British Consul at Saigon, contains some interesting geographical information about the northern and less known districts of the Indo-Chinese peninsula. The governor of Cochin China sent out an expedition to explore the country between the Meikong and Annam at about  $14^{\circ}$  latitude. The party left Peamchileng, on the Meikong, proceeding eastward. After passing the river valley the country became hilly and wooded, intersected with numerous watercourses. No difficulty was experienced until the arrival of the travellers near the Cambodian frontiers. As they proceeded the hostility of the people became yet more pronounced, and finally their passage towards Annam was closed altogether. They were finally compelled to retreat, losing all their baggage on the way, and after three months' absence they reached a friendly post. The Moïs inhabit the wilds between Cambodia, Siam, Burmah, and China. Commerce, properly speaking, does not exist among them, and traffic is carried on by exchanges. The various roads and river are stopped up by the people themselves to prevent the passage of pillagers and enemies; as a result the people are very backward. Money is almost unknown or unappreciated among them. A native who will not work for a dollar a day will do so for a bell costing a few cents. The articles most valued by the Moïs are buffaloes, red and white cottonades, glass ware, brass wire, utensils, salt, and salt fish.

FROM the same report we learn that an exploration of the upper waters of the Saigon river by Lieut. Gauthier shows that previous charts are incorrect; the names given in them being imaginary. There appears to have existed in this region in former times one or more states in an advanced stage of civilisation, as may be seen by ruins still remaining, probably offshoots of the famous Angkor Wat. The present race of Moïs claim no descent from their predecessors on the soil, and indeed it would be difficult to find a lower state than theirs. It is difficult to communicate with them, as the language is not easily picked up by the Annamites. They appear to be in a state of independence, paying no tribute to any of their neighbours, although the King of Cambodia is their nominal suzerain. The report concludes by saying that the French Government will have to send a much more serious expedition if anything is to be learned about these regions; two or three men can learn nothing.

THE great attention which France has given for many years past to the Indo-Chinese peninsula is shown by a return printed in the *Proceedings of the Société Académique Indo-Chinoise* of all the scientific expeditions despatched by the French Government to this region. These embrace archaeological, ethnological, geographical, and other scientific objects, and up to 1881 they were seventy-seven in number. They commence as far back as 1680, when the Jesuit Pallu visited the courts of Tonkin and Annam. Seventeen of these took place before the military occupation of any part of Cochin China by the French; thirty-three were sent by the Ministry of Public Instruction, chiefly for archaeological purposes, while the remaining twenty-seven were sent by the Ministry of Marine and the Colonies, and were devoted principally to exploration. To understand the mass of scientific work done by the French in Indo-China, it must be remembered that these seventy-seven expeditions do not include the innumerable journeys and researches of the missionaries,



which commence as far back as the end of the fifteenth century, the various expeditions sent out by private enterprise, those despatched for military, naval, or diplomatic purposes, or, finally, the various hydrographic or geodetic surveys undertaken by the French authorities in Cochin China.

THE teachers at the school for the sons of Japanese nobles in Tokio appear to have hit upon a notable method of teaching physical geography. In the court behind the school building is a physical map of the country, between three and four hundred feet long. It is made of turf and rock, and is bordered with pebbles, which look at a little distance much like water. Every inlet, river, and mountain is reproduced in this model with a fidelity to detail which is wonderful. Latitude and longitude are indicated by telegraph wires, and tablets show the position of the cities. Ingenious devices are employed in illustrating botanical studies also. For example, the pine is illustrated by a picture showing the cone, leaf, and dissected flower, set in a frame which shows the bark and longitudinal and transverse sections of the wood.

IN No. 103 of the *Zeitschrift* of the Berlin Geographical Society will be found a fine series of new large scale maps by H. Kiepert on the region containing the ruins of Babylon, embodying the results of new surveys and explorations. In the same number Herr Karl Schneider has a long paper on the valley formations of the Eifel.

PROF. FRIES has written an interesting paper proposing that part of Greenland should be colonised by Lapps. He maintains that the country would be a paradise to the mountain Lapps, that it is no more inhospitable than their own country, that there would be no restrictions to their wanderings, and that in the interior in summer and on the coast in winter they would find abundant forage for their herds. Prof. Fries is of Nordenskjöld's opinion, that in the interior abundant reindeer pasture will be found. Moreover, as a Lapp can always follow where a reindeer leads, this would be an excellent plan of discovering the true nature of the interior; it seems certainly worth trying.

Two gentlemen from Münster (Westphalia)—Dr. Bachmann and Dr. Friedrich Wilms—are about to start on a scientific tour to Southern Africa, the Transvaal to begin with, in order to make zoological and botanical researches. Their journey will extend over several years, and the travellers will endeavour to establish direct commercial relations between the South African colonies and Germany.

#### ELECTRICAL UNITS OF MEASUREMENT<sup>1</sup>

THE lecturer began by observing that no real advance could be made in any branch of physical science until practical methods for a numerical reckoning of phenomena were established. The "scale of hardness" for stones and metals used by mineralogists and engineers was alluded to as a mere test in order of merit in respect to a little understood quality, regarding which no scientific principle constituting a foundation for definite measurement had been discovered. Indeed it must be confessed that the science of strength of materials, so all important in engineering, is but little advanced, and the part of it relating to the quality known as hardness least of all.

In the last century Cavendish and Coulomb made the first advances towards a system of measurement in electrical science, and rapid progress towards a complete foundation of the system was effected by Ampère, Poisson, Green, Gauss, and others. As late as ten years ago, however, regular and systematic measurement in electrical science was almost unknown in the chief physical laboratories of the world; although as early as 1858 a practical beginning of systematic electric measurement had been introduced in the testing of submarine telegraph cables.

A few years have sufficed to change all this, and at this time electric measurements are of daily occurrence, not in our scientific laboratories only, but also in our workshops and factories where is carried on the manufacture of electric and telegraphic apparatus. Thus ohms, volts, amperes, coulombs, and microfarads are now common terms, and measurements in these units are commonly practised to within one per cent. of accuracy. It seems, indeed, as if the commercial requirements of the application of electricity to lighting and other uses of everyday life

were destined to influence the higher region of scientific investigation with a second impulse, not less important than that given thirty years ago by the requirements of submarine telegraphy.

A first step toward the numerical reckoning of properties of matter is the discovery of a continuously varying action of some kind, and the means of observing and measuring it in terms of some arbitrary unit or scale division; while the second step is necessarily that of fixing on something absolutely definite as the unit of reckoning.

A short historical sketch was given of the development of scientific measurement, as applied to electricity and magnetism, from its beginning with Cavendish about 100 years ago, to the adoption of the absolute system of measurement by this country in 1869, at the instance of the British Association Committee on Electric Standards. The importance in this development of the originating works of Gauss and Weber was pointed out, as also of the eight years' labours of the British Association Committee. This Committee not only fairly launched the absolute system for general use, but also effected arrangements for the supply of standards for resistance coils, in terms of a unit, to be as nearly as possible  $10^9$  centimetres per second. This unit afterwards received the name of the ohm, which was adopted from a highly suggestive paper which had been communicated to the British Association in 1861 by Mr. Latimer Clark and Sir Charles Bright, in which some very valuable scientific methods and principles of electric measurement were given, and a system of nomenclature—ohms, kilohms, farads, kilofarads, volts, and kilovolts—now universally adopted with only unessential modification, was proposed for a complete system of interdependent electric units of measurement. At the International Conference for the Determination of Electrical Units held at Paris in 1882, the absolute system was accepted by France, Germany, and the other European countries; and Clark and Bright's nomenclature was adopted in principle and extended.

Gauss's principle of absolute measurement for magnetism and electricity is merely an extension of the astronomer's method of reckoning mass in terms of what may be called the universal gravitation unit of matter, and the reckoning of force, according to which the unit of force is that force which, acting on unit of mass for unit of time, generates a velocity equal to the unit of velocity. The universal-gravitation unit of mass is such a quantity of matter, that if two quantities, each equal to it, be placed at unit distance apart, the force between them is unity.

Here mass is defined in terms of force and space, and in the preceding definition force was defined in terms of mass, space, and time. Eliminating mass between the two, it will be found that any given force is numerically equal to the fourth power of the velocity with which any mass whatever must revolve round an equal mass, fixed at such a distance from it as to attract it with a force equal to the given force. And, eliminating force between the two primitive definitions of the universal-gravitation system, it will be found that any given mass is numerically equal to the square of the velocity with which a free particle must move to revolve round it in a circle of any radius, multiplied by this radius. Thus, take a centimetre as the unit of length, and a mean solar second as the unit of time, and adopt 5.67 grammes per cubic centimetre as the mean density of the earth from Baily's repetition of Cavendish's experiment, and suppose the length of the seconds' pendulum to be 100 centimetres, and neglect the oblateness of the earth and the centrifugal force of its rotation (being at the equator only  $1/289$  of gravity), the result for the universal gravitation units of mass and force is respectively  $15.36$  French tons, and  $15.36$  megadynes, or  $15.07$  times the terrestrial surface-weight of a kilogram.

The ultimate principles of scientific measurement were illustrated by the ideal case of a traveller through the universe who has brought with him on his tour no weights, no measures, no watch or chronometer, nor any standard vibrator or spring balance, but merely Everett's units and constants, and a complete memory and understanding of its contents, and who desires to make for himself a metrical system agreeing with that which he left behind him on the earth. To recover his centimetre the readiest and most accurate way is to find how many wavelengths of sodium light there are in the distance from bar to bar of a grating which he can engrave for himself on a piece of glass. How easily this is done, supposing the grating once made, was illustrated by a rapid experiment performed in the course of the lecture, without other apparatus than a little piece of glass with 250 fine parallel lines engraved on it by a diamond, and two candles and a measuring tape of unknown divisions of

<sup>1</sup> Abstract of lecture on "Electrical Units of Measurement," by Sir William Thomson, F.R.S.S.L. and E., M.Inst.C.E., delivered on Thursday evening, May 3, 1883, at the Institution of Civil Engineers.



length (only used to measure the *ratio* of the distance between the candles to the distance of the grating from either). The experiment showed the distance from centre to centre of consecutive bars of the grating to be 32 times the wave-length of yellow light. This being remembered to be  $5.89 \times 10^{-5}$  of a centimetre, it was concluded that the breadth of the space on which the 250 lines are engraved is 250. 32.  $5.892. 10^{-5}$ , or  $.4714$  of a centimetre! According to the instrument-maker it is really  $.5$  of a centimetre! Five minutes spent on the experiment instead of one, and sodium flames behind fine slits, instead of open candles blowing about in the air might easily have given the result within one-half per cent. instead of  $4\frac{1}{2}$  per cent. Thus the cosmic traveller can easily recover his centimetre and metre measure. To recover his unit of time is less easy. One way is to go through Foucault's experimental determination of the velocity of light.

But he must be imagined as electrically-minded; and he will certainly, therefore, think of "*v*," the number of electrostatic units in the electro-magnetic unit of electricity; but he will, probably, see his way better to doing what he wants by making for himself a Siemens' mercury unit (which he can do easily, now that he has his centimetre), and finding (by the British Association method, or Lorenz's with Lord Rayleigh's modification, or both), the velocity which measures its re-istance in absolute measure. This velocity, as is known from Lord Rayleigh and Mrs. Sidgwick, is 9413 kilometres per mean solar second, and thus he finds, in mean solar seconds, the period of the vibrator, or arbitrary-unit chronometer, which he used in his experiments.

Still, even though this method might be chosen as the readiest and most accurate, according to present knowledge, of the fundamental data for recovering the mean solar second, the method by "*v*" is too interesting and too instructive in respect to elimination of properties of matter from our ultimate metrical foundations to be unconsidered. One very simple way of experimentally determining "*v*" is derivable from an important suggestion of Clark and Bright's paper, referred to above. Take a Leyden jar, or other condenser of moderate capacity (for example, in electrostatic measure, about 1000 centimetres), which must be accurately measured. Arrange a mechanism to charge it to an accurately measured potential of moderate amount (for example, in electrostatic measure, about 10 c.g.s., which is about 3000 volts), and discharge it through a galvanometer coil at frequent regular intervals (for example, ten times per second). This will give an intermittent current of known average strength (in the example,  $10^5$  electrostatic c.g.s., or about  $1/300,000$  c.g.s. electromagnetic, or  $1/30,000$  of an ampere), which is to be measured in electromagnetic measure by an ordinary galvanometer. The number found by dividing the electrostatic reckoning of the current, by the experimentally found electromagnetic reckoning of the same, is "*v*," in centimetres per the arbitrary unit of time, which the experimenter in search of the mean solar second has used in his electrostatic and electromagnetic details. The unit of mass which he has chosen, also arbitrarily, disappears from the resulting ratio. It is to be hoped that before long "*v*" will be known within  $1/10$  per cent. At present it is only known that it does not *probably* differ 3 per cent. from  $2.9 \times 10^{10}$  centimetres per mean solar second. When it is known with satisfactory accuracy, an experimenter, provided with a centimetre measure, may, anywhere in the universe, rate his experimental chronometer to mean solar seconds by the mere electrostatic and electromagnetic operations described above, without any reference to the sun or other natural chronometer.

The remainder of the lecture was occupied with an explanation of the application of the absolute system in all branches of electric measurement, and the definition of the now well known practical units founded on it, called ohms, volts, farads, microfarads, amperes, coulombs, watts. The name mho, found by saying ohm to a phonograph and then turning the drum backwards, was suggested for a unit of conductivity, the reciprocal of re-istance. The subdivision, millimho, will be exceedingly convenient for the designation of incandescent lamps.

The British Association unit has been found by Lord Rayleigh and Mrs. Sidgwick to be  $.9868$  of the true ohm ( $10^9$  centimetres per second), which differs by only  $1/50$  per cent. from  $.9870$ , the number derived from Joule's electrothermal measurements described in the British Association Committee's Report of 1867, with 772 Manchester foot-pounds taken as the dynamical equivalent of the thermal unit from the measurement

described in his Royal Society paper of 1849, and confirmed by his fresh measurement of 20 years later, published in his last Royal Society paper on the subject.

It is satisfactory that, whether for interpreting old results, or for making resistance-coils anew, electricians may now safely use the British Association unit as  $.9868$ , or the Siemens unit as  $.9413$ , of the ohm defined as  $10^9$  centimetres per second.

#### U.S. NATIONAL ACADEMY OF SCIENCES<sup>1</sup>

THE annual meeting of this body was held in Washington during the last week, with an attendance of forty members. Scientific sessions were held on Tuesday, Wednesday, and Friday, in the large lecture-room of the National Museum, and business sessions on every day of the meeting.

Twenty-four foreign associates were elected as follows:—Astronomers: Prof. Otto von Struve, of the Imperial Observatory at Pulkowa, Russia; Prof. J. C. Adams, of Cambridge, Eng.; Prof. A. Auwers, Director of the Observatory at Berlin; and Prof. Theo. von Oppolzer, Director of the Observatory at Vienna. Mathematicians: Prof. Arthur Cayley, of the University of Cambridge, Eng.; Prof. J. J. Sylvester, of the Johns Hopkins University, Baltimore; and Prof. E. Bertrand, of Paris. Physicists: Prof. R. Clausius, of the University of Bonn; Baron H. von Helmholtz, Professor in the University of Berlin; Prof. Robert Kirchhoff, of the University of Berlin; Prof. G. G. Stokes, of the University of Cambridge, Eng.; and Sir William Thomson, Professor in the University of Glasgow. Chemists: Prof. J. B. Dumas, Secretary of the Academy of Sciences, Paris; and Professors M. Berthelot, Boussingault, Chevreul, and Würtz, all of Paris. Geologist: Freiherr von Richtofen, Professor in the University of Bonn, and President of the German Geographical Society. Botanists: Sir J. D. Hooker, Director of the Botanical Gardens at Kew, Eng.; Prof. A. de Candolle, of Geneva. Biologists: L. Pasteur, of Paris; Prof. T. H. Huxley, of London; Prof. R. von Virchow, of the University of Berlin; A. von Kölliker, Professor of Anatomy in the University of Würzburg. Prof. Struve, one of the newly elected foreign associates, who is on a visit to this country, was a regular attendant at the scientific sessions of the Academy, and read a paper.

In consequence of the death of Prof. W. B. Rogers, the President, it became necessary to elect his successor. On the first ballot, Prof. Wolcott Gibbs, of Cambridge, one of the founders of the Academy, was elected. He, however, firmly declined the honour, from a feeling, as he said, that he could not give the time necessary to the work. The Academy reluctantly acquiesced in the decision of Prof. Gibbs, and proceeded to a second ballot, when Prof. O. C. Marsh, of New Haven, the acting President, was elected by a handsome majority. The newly-elected President will hold office for six years.

The first act of the new President was to announce that he had received from Mrs. Mary A. Draper, widow of Prof. Henry Draper, the sum of six thousand dollars, accompanied by a deed of trust which fully specified the objects she had in view. He called upon Prof. Barker to explain the nature of the trust to the Academy. Prof. Barker first made some appropriate remarks, recalling Prof. Draper's interest in the Academy, and then read the deed, the substance of which is as follows:—The income of the trust is to be used "for the purpose of striking a gold medal which shall be called the 'Henry Draper Medal,' shall be of the value of two hundred dollars," and shall be awarded from time to time, but not oftener than once in two years, as a premium to any person in the United States or elsewhere who shall make an original investigation in astronomical physics, the results of which shall be deemed by the Academy of sufficient importance and benefit to science to merit such recognition. If at any time the income of the fund shall exceed the amount necessary for the striking of the medal, the surplus may be used in aid of investigations and work in astronomical physics to be made and carried on by a citizen of the United States.

The President appointed Messrs. G. F. Barker, W. Gibbs, S. Newcomb, A. W. Wright, and C. A. Young as a committee to have charge of the fund, to make rules to govern the award of the medal, and to suggest to the Academy for approval the names of those who may be considered worthy of the award.

The Treasurer announced that in accordance with the will of

<sup>1</sup> From *Science*, April 27.



the late Prof. James C. Watson the sum of about fourteen thousand dollars had been placed in his hands. When the estate is finally closed a further sum will be paid over to the Academy. The income of the Watson fund is to be used under the direction of three trustees—Messrs. J. E. Hilgard, S. Newcomb, and J. H. C. Coffin—for the purpose of aiding astronomical researches. In accordance with the recommendation of the trustees the Academy granted five hundred dollars from this fund towards defraying the expenses involved in observations of the total solar eclipse of May 6, 1883.

Later in the meeting Prof. Simon Newcomb of Washington was elected Vice-President, and Prof. Asaph Hall of Washington Home Secretary. Five new members were elected; Prof. A. Graham Bell of Washington, Dr. J. S. Billings, U.S.A., of the U.S. Army Medical Museum, Washington; G. K. Gilbert of the U.S. Geological Survey; H. B. Hill and C. L. Jackson, Professors of Chemistry in Harvard College. The whole number of members is now ninety-five.

On the afternoon of Thursday the Academy adjourned to take part by invitation in the ceremonies attending the unveiling of the statue of Prof. Henry in the grounds of the Smithsonian Institution. The time for these ceremonies was purposely fixed to coincide with that of the spring meeting of the Academy. Henry was preeminently a scientific man, and at the time of his death President of the Academy; and yet the members of the Academy were placed far down the line in the procession—after the Commissioners of the District of Columbia, and after officers of the army and navy. This fact must be regarded as evidence of a lack of appreciation of the relations existing between Henry and the Academy and of the true worth and dignity of science.

The exercises, which were in good taste, began with a short address by Chief Justice Waite. After this, at a signal, the covering was quickly drawn aside, instantly revealing the entire statue. Loud applause followed, those who were seated rose to their feet, and all hats were removed. The scene was highly impressive; and when the Philharmonic Society, accompanied by the full marine band, burst forth with Haydn's grand chorus, "The heavens are telling," the heart must have been a hardened one which did not experience a feeling of exaltation.

In the opinion of all, the statue is dignified and pleasing, and vividly calls to mind the honoured original. President Porter's oration, which was the principal event of the afternoon, was listened to with much interest. It dealt with the plain facts of the life of Henry, and was all that his best friends could have desired.

Among the pleasantest social features of the meeting was a reception given to the members of the Academy on Thursday evening by Prof. A. Graham Bell. There were present many well-known gentlemen, among them General Sherman, Chief Justice Waite, Senator Sherman, ex-Secretary Blaine, and the Japanese, Swedish, and Belgian ambassadors.

### SCIENTIFIC SERIALS

*Zeitschrift für wissenschaftliche Zoologie*, Bd. xxxviii, Heft 1, February 20, 1883, contains:—On the vascular system and the imbibition of water in the Najadæ and Mytilidæ, by Dr. Hermann Griesbach (Pl. 1).—Researches among the Protozoa, by Dr. A. Gruber (Plates 2 to 4); describes and figures several new genera and species.—On the origin of the saliva (*Putter saft*) and the salivary glands in the bee, together with an appendix on their olfactory organ, by Dr. P. Schiemenz (Plates 5 to 7).—On the development of the red blood corpuscles, by Dr. W. Feuerstack (woodcuts).—Candid reply to my critics in the matter of the "Brain of Fishes," by G. Futsch.

*Proceedings of the St. Petersburg Society of Natural History*, Vol. xiii, Part I, for 1882, contains:—On the archæology of Russia, by Count Tivatkov (the Stone Period).—Notes of a journey on the Dnieper in 1844, by Dr. Kessler.—On *Capra caucasica*, Güld., by H. Dinnik.—Darwinism from the point of view of universal physical science, by A. Beketov.—A monograph of the Mysidæ to be found in Russia (Marine, Lacustrine, and Fluvial), by Voldemar Czernavsky, fasc. 2. All the above articles are in Russian except the last, which is in Latin, and it is illustrated by four lithographic plates.

*Journal of the Russian Chemical and Physical Society*, vol. xv, fascicule 3.—On the hydrocarbon  $C_{12}H_{20}$  obtained from the allyl dimethyl carbinol, by Prof. A. Zaytseff and W. Nicolsky.—On the hydrocarbon  $C_{10}H_{18}$  obtained from the allyl dipropyl carbinol,

by S. Reformatsky. It is a colourless liquid boiling at about  $158^{\circ}$  Celsius, insoluble in water, and easily soluble in alcohol and ether. It rapidly absorbs the oxygen of the air; density  $0.787$  at  $0^{\circ}$ ,  $0.774$  at  $16^{\circ}$ , and  $0.770$  at  $21^{\circ}$ .—Chemical analysis of Kieff clays, by S. Bogdanoff. The white clay contains 96 per cent. of kaolins; the loess contains 83.5 per cent. of quartz, felspar, mica, and other silicates, 5.38 of kaolin, and 6.73 of carbonate of lime.—On the diisooctyl, by A. Alechin.—On the composition of the water which accompanies the naphtha and is discharged by mud-volcanoes of the Government of Tiflis, by A. Potylitzin (second paper).—An elementary demonstration of the pendulum-formula, and on a differential aerial calorimeter, by W. Preobrajensky.

*THE Archives des Sciences Physiques et Naturelles* for February, 1883, contains papers by C. E. Guillaume on electrolytic condensers; by Emile Yung, on the errors of the senses, a contribution to the study of illusions and hallucinations; by Ernest Favre, on the Geological Survey of Switzerland for 1882, concluded in the March number. To the latter C. de Candolle sends an interesting essay on the ripple marks formed on the surface of sands under water, and on other analogous phenomena.

*THE Journal de Physique théorique et appliquée* for March contains papers by Ph. Gilbert, on the experiments best suited for demonstrating the rotation of the earth; by G. Lippmann, on Helmholtz's theory of double electric layers as applied to electro capillary phenomena; by H. Pellat, on the same subject; by A. Rosenstiehl, on the definition of complementary colours; by Ch. Cros and Aug. Vergeraud, on a direct positive photographic paper.

### SOCIETIES AND ACADEMIES

#### LONDON

**Royal Society, March 15.**—"On the Changes which take place in the Deviations of the Standard Compass in the Iron Armour-plated, Iron, and Composite-built Ships of the Royal Navy on a considerable change of Magnetic Latitude." By Staff-Commander E. W. Creak, R.N., of the Admiralty Compass Department. Communicated by Capt. Sir F. J. Evans, R.N., K.C.B., F.R.S., Hydrographer of the Admiralty.

The period comprised between the years 1855-68 was one of active research into the magnetic character of the armour-plated and other ships of the Royal Navy and iron ships of the Mercantile Navy.

Among other contributions to this subject a paper by F. J. Evans, Staff-Commander R.N., F.R.S., and Archibald Smith, F.R.S., was read before the Royal Society in March 1865, relating to the armour-plated ships of the Royal Navy, and containing the first published results of the system of observation and analysis of the deviation of the compass established four years previously.

From lack of observations in widely different magnetic latitudes the authors of that paper were unable to define the proportions of the semicircular deviations arising from vertical induction in soft iron and that arising from permanent or sub-permanent magnetism in hard iron.

During the last fifteen years vessels of all classes—except turret ships—have visited places of high southern magnetic inclination or dip, and the analysis of the deviations of their standard compasses has been made, showing the constants of hard and soft iron producing semicircular deviation.

The constants for soft iron provide a means of predicting probable changes of deviation on change of magnetic latitude for certain vessels of the following classes, and others of similar construction.

1. Iron armour-plated ships.
2. Iron cased with wood.
3. Iron troopships.
4. Iron and steel cased with wood.
5. Composite-built vessels.
6. Wooden ships with iron beams and vertical bulkheads.

These vessels were all in a state of magnetic stability previous to the observations which have been discussed, and their compasses have had the semicircular deviation reduced to small values, or corrected, in England by permanent bar magnets.

This correction may be considered as the introduction of a permanent magnetic force acting independently, and in opposition to the magnetic forces of the ship proceeding from hard iron.



It is now proposed to consider the effects of a change of magnetic latitude on the component parts of the deviation.

#### *Semicircular Deviation*

On semicircular deviation from fore and aft forces, time has but little effect, and the greater part of it is due to permanent magnetism in hard iron which may be reduced to zero for all latitudes, by a permanent magnet.

A second but small part of this semicircular deviation proceeds from sub-permanent magnetism in hard iron. It is subject to alterations slowly by time, from concussion, and from the ship remaining in a constant position with respect to the magnetic meridian for several days, and is more intensely affected by a combination of the two latter causes.

Deviations from sub-permanent magnetism which have temporarily altered in value as described, return slowly to their original value on removal of the inducing cause.

The principal cause of change in the semicircular deviation on change of magnetic latitude, in corrected compasses, arises from vertical induction in soft iron, which changes directly as the tangent of the dip.

In standard compasses judiciously placed with regard to surrounding iron this element of change is small and similar in value for similar classes of ships.

With very few exceptions, nearly the whole of the semicircular deviation from transverse forces is due to permanent magnetism in hard iron subject to the same laws as that proceeding from fore and aft forces.

In the exceptional cases alluded to there is a small part due to vertical induction in soft iron, changing directly as the tangent of the dip.

#### *Quadrantal Deviation*

This deviation is caused by induction in horizontal soft iron symmetrically placed, and it does not change with a change of magnetic latitude. Time alone appears to cause a gradual change in its value during the first two or three years after the ship is launched, when it becomes nearly permanent.

The diminution of the mean directive force of the needle which is common to all modern vessels of war, improves slowly at first by lapse of time, and finally assumes a permanent value.

#### *Relative Proportions of Hard and Soft Iron*

It has been found that the relative proportions of the hard and soft iron affecting the standard compasses of twenty-five vessels examined differ considerably, even in ships of similar construction.

This difference may be accounted for by the compasses not being placed in the same relative position in the ships, considered as magnets of various forms and containing numerous iron bodies introduced during equipment.

#### *General Conclusions*

The following general conclusions have special reference to the standard compass positions in the six classes of vessels previously mentioned.

1. A large proportion of the semicircular deviation is due to permanent magnetism in hard iron.
2. A large proportion of the semicircular deviation may be reduced to zero, or corrected, for all magnetic latitudes, by fixing a hard steel bar magnet or magnets in the compass pillar, in opposition to, and of equal force to, the forces producing that deviation.
3. A very small proportion of the semicircular deviation is due to sub-permanent magnetism, which diminishes slowly by lapse of time.
4. The sub-permanent magnetism produces deviation in the same direction as the permanent magnetism in hard iron, except when temporarily disturbed (1) by the ship's remaining in a constant position with respect to the magnetic meridian for several days, (2) by concussion, or (3) by both combined, when the disturbance is intensified.
5. To ascertain the full value of changes in the sub-permanent magnetism, observations should be taken immediately on the removal of the inducing cause.
6. In the usual place of the standard compass the deviation caused by transient vertical induction in soft iron is small, and of the same value (nearly) for ships of similar construction.
7. The preceding conclusions point to the conditions which should govern the selection of a suitable position for the standard compass with regard to surrounding iron in the ship.

**Anthropological Institute, April 24.**—Prof. W. H. Flower, F.R.S., president, in the chair.—The election of Mr. C. Roberts, F.R.C.S., was announced.—Mr. W. M. Flinders Petrie read a paper on the mechanical methods of the Egyptians. The author exhibited several specimens of ancient Egyptian work, and described the methods by which he believed them to have been produced.—Mr. F. C. J. Spurrell read a paper on some palæolithic knapping tools and modes of using them.

May 8.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. Frederick Bonney read a paper on some customs of the aborigines of the River Darling, New South Wales. The tribes with which the author was most familiar are called Bungy-arlee and Parkungi. They inhabit a district within lat. 29°–34° S., long. 141°–146° E. The country in its natural state was incapable of supporting a large population, being subject to protracted droughts, during which both food and water were scarce. There is a similarity in the typical features of all the Australian aborigines, but to a close observer each tribe has its own peculiarities. Though ugly and unprepossessing in appearance, they are most kind, gentle, and of quite average intelligence and morality. The aborigines of Australia are often spoken of as the lowest type of humanity, but the author considered this to be a libel on the whole of them, and was positive it is so as regards the tribes he knows best. Mr. Bonney then proceeded to give a description of the life-history of the above-mentioned tribes.—Lieut.-Col. H. H. Godwin-Austen, F.R.S., read a paper on the discovery of some worked flints, cores, and flakes from Blackheath, near Chilworth and Bramley, Surrey.—A paper by Admiral F. S. Tremlett, F.G.S., was read, on stone circles in Brittany, in which the author described three circles discovered by the late Mr. James Milne, in the commune of Carnac; they had presumably been places for cremating the dead, and also for depositing the urns; the greater part of the latter were found inclosed in cists of quartz covered over by a slab of schist, neither of which are to be found in the district.—Mr. W. Galloway exhibited a skull and a number of rubbed bones and other implements from the islands of Oronsay and Colonsay, forming part of a large collection exhibited by him in the Great International Fisheries Exhibition.

**Physical Society, May 12.**—Prof. Clifton in the chair.—New Member, Mr. A. W. Soward.—Mr. Woodward described an experiment illustrating motion produced by diffusion. A porous reservoir of clay containing air was suspended from one end of a weighted balance beam. A glass tube projected from it below and dipped into a vessel of water. A jet of hydrogen gas was allowed to play on the outside of the reservoir and the balance beam began to oscillate. This is an adaptation of Graham's well-known experiment, and is in fact a diffusion engine. Prof. Adams explained the action by the variation of pressure in the reservoir set up by diffusion.—Mr. W. Lant Carpenter read a paper on some uses of a new projection lantern. This lantern, of German make, is applied by Mr. Pater-son, and is simple in construction, cheap, and gives a good image visible to a large audience. It can be used with a three-wick oil lamp or the limelight. Mr. Carpenter showed a number of objects on the screen. Mr. Lecky and Mr. Woodward offered some remarks, the latter deprecating a too frequent use of projection with students.—Dr. C. R. Alder Wright read a paper on the electromotive force of Clark's mercurous sulphate cell and the work done during electrolysis. He described the best mode of constructing Clark's standard cell. According to numerous tests, these cells vary in E.M.F. about 0.2 per cent. + or – among themselves. A cell properly made will keep its value for about two years. It is of great importance that the cell should not be worked or the current reversed through it, otherwise it may permanently deteriorate. The extraction of air from the paste is not very essential, and boiling it is unnecessary. It is more important that the solutions of zinc sulphate should be saturated. Dr. Wright described a cell in vacuum which is a good standard. He found the E.M.F. to vary 0.4 per cent. between 0° and 100° C. With regard to the work done in a cell, among other interesting deductions, he found that in a secondary battery the larger the plates the greater the economy. In the electrolysis of water the greater the surface condensing power of the electrodes for gas the less difference of potential is required to decompose the water. Thus with platinum electrodes a lower E.M.F. serves for the electrolysis than with gold electrodes.—Prof. Foster then took the chair, and Prof. Clifton read a paper on a complete determination of a double convex lens by lineal



measurements on the optical bench. This was a method (some what similar to that of Mr. Boys, previously described to the Society) for determining the four quantities of a lens on the bench by lineal measures, and without the use of the spherometer and prism. Experiments showed that it was about as accurate as the spherometer method.

## EDINBURGH

Royal Society, May 7.—Prof. Maclagan, vice-president, in the chair.—By request of the Council Prof. James Geikie gave an address on recent advances in the Pleistocene geology of Europe. The characteristic deposits of this period, which embraces the Palæolithic age of the antiquarians, were described in considerable detail—the terminal and ground moraines and other glacial remains, the fluviatile and lacustrine formations, and the cave deposits. The limits were indicated of the great Scandinavian ice-sheet, which pushed itself southward over North Germany and over the watershed of Central Russia, and westward across the German Ocean towards our islands, thereby modifying the trend of the native ice-streams that have left their traces all over our hills and round our coasts. As an indication of the great power of this agent it was mentioned that some portions of the brown-coal beds of Saxony which have been long worked are really not *in situ*, but have been pushed out of place by the ice-sheet. In describing the fluviatile deposits Prof. Geikie drew attention to a suggestion made by Darwin, that frozen snow accumulating in the valleys below the glacier limits might easily act as barriers and give rise to extensive flooding. The fauna and flora and the evidence of the interglacial beds were then touched upon, and the address ended with a general summary of results with special reference to the climatic peculiarities of the Pleistocene period. It thus appeared that Europe was subjected to great climatic changes, severe glacial periods alternating with times of peculiar equable climate in which temperate flora and fauna flourished side by side with forms which are now met with only in southern regions.

## SYDNEY

Linnean Society of New South Wales, March 28.—Rev. J. E. Tenison-Woods, F.L.S., vice-president, in the chair.—The following papers were read:—Occasional notes on plants indigenous in the immediate neighbourhood of Sydney (No. 3), by Edwin Haviland. This paper refers chiefly to the genus *Lobelia*, its mode of fertilisation, and its domestication.—On tooth-marked bones of extinct marsupials, by Chas. W. de Vis, B.A., A large proportion of fossil marsupial bones from the Darling Downs, recently examined by Mr. de Vis, are considered by him to show more or less decided traces of the action of the teeth of carnivorous animals. The tooth-marks are ascribed to the agency partly of the native dog, partly of the *Thylacoleo*, and partly of an extinct species of *Sarcophilus* which was identified by a portion of a tibia.—On *Brachalletes palmeri*, an extinct marsupial, by Chas. W. de Vis, B.A. A femur from the Darling Downs differs so markedly from that of *Macropus* and *Halmaturus* in the less prominent character of the great trochanter that it is considered to belong to a new generic type, proposed to be named *Brachalletes*.—On the habits of the "Mallee hen" (*Leipoa ocellata*), by K. H. Bennett. This gives an interesting and detailed account from the author's own observation of the nidification and general habits of this very curious bird.—Mr. Macleay exhibited a specimen of *Dendrolagus dorianus*, a new species of Tree Kangaroo from Mount Owen Stanley, New Guinea, described by Mr. E. P. Ramsay at the January meeting of the Society. He pointed out that the hair on the body all turned the wrong way.

## BERLIN

Physiological Society, April 13.—Prof. du Bois Reymond spoke about a series of electrophysiological investigations which he began at the same time as his "Investigations in Animal Electricity," which have long since been incorporated in science, now forty years ago, and about which he has as yet not published anything, viz., about the secondary electromotor phenomena of muscles, nerves, and electric organs. These latter are distinguished from primary electromotor phenomena of nerves and muscles by the fact that the latter appear in quiescent organs and take place without being directly influenced by an external electric current, whereas the former appear only as a consequence of an extrinsic electrical current, and consequently are connected with the polarisation appearances in electrolytic conductors. When a current is led through a fluid electrolyte

by means of metallic electrodes, a reverse (negative) polarisation current is, as is well known, produced between the electrodes by the accumulation of ions on the anode and cathode. In the year 1836 Peltier described a similar negative (in direction opposed to principal current) polarisation in masses of frogs' limbs through which an electrical current was being passed, and explained it in the same way by the development of ions on the electrodes. When Prof. du Bois Reymond repeated this experiment in the beginning of the forties, he found that an electromotive force was active not only at the electrodes, but that each piece of the preparation through which the current was passing had a negative electromotive reaction, and showed an opposite current to the polarising one in a galvanometer that was applied. On further study of this phenomenon, he found this "inner" polarisation in every porous conductor, which is soaked with a readily conducting electrolyte, and it was in all cases negative; on the other hand an outer positive polarisation exhibited itself on the line of contact of dissimilar electrolytes, e.g. when the current was led through a pad soaked with water into a salt solution. Fresh animal tissues of the most different kinds, when a current was led through them between pads soaked in common salt, accordingly showed an outer positive and an inner negative polarisation. Further, the lecturer studied an outer and an inner secondary (called forth by the current) resistance, of which the former was at least partially accounted for by the cataphorical action of the current. When afterwards (*i.e.* after the determination of the above-mentioned physical phenomena) the inner polarisation was studied on living muscles, secondary electromotor appearances of such irregularity and complexity manifested themselves that it was only after laborious investigations that were extended over many years that the simple law that the phenomena obey was discovered. It was discovered that when a current was passed through a muscle the inner polarisations might be positive as well as negative, that they depend on the density and length of duration of the polarising current, and that each of these polarisations can be altered in a different manner by these two factors. If the densities and duration of action of the primary current are properly graduated, the phenomena follow the following law:—With very weak polarising currents the inner polarisation is negative, and increases up to a certain limit with the duration of the current; with somewhat stronger currents, the inner polarisation is at first positive, but soon passes over into the negative, which goes on increasing with the duration of the current; with still stronger currents, the initial positive inner polarisation becomes stronger and longer lasting, and then again becomes negative with the longer duration of the primary current. If the density of the polarising current increases still more, the initial positive current becomes weaker and weaker, and finally disappears altogether, and gives way to a polarisation that is negative from the beginning. Accordingly there exists in the inter-polar portion of a muscle that is traversed by a current, after a certain limit has been exceeded, a positive inner polarisation, which in a short time is replaced by a negative polarisation, and the deduction from these phenomena is that both secondary electromotive forces—those with the same and with opposite directions—are present in the portion of muscle traversed by the electrical current. These electromotive forces manifest themselves alternately, the predominance of the one and the other being conditioned by the several dependence of each upon the density and duration of the primary current. This indication of a positive inner polarisation, *i.e.* of secondary electromotor forces, which occasion a current in the same direction as the primary current, is a fact of fundamental import in the theory of animal electricity. The positive polarisation proved itself to be dependent upon the direction of the primary current, since it was stronger in the upper half of the muscle when the direction of the current was from below upwards, whereas it was stronger in the lower half with a descending current; furthermore it manifested itself in living muscles only, whereas the negative polarisation occurred also in muscles that had been boiled or otherwise killed; finally, the positive polarisation was less strong in active than in quiescent muscles. At the end of the fifties the lecturer had also succeeded in demonstrating a positive inner polarisation in nerves; it showed the same regularity as was afterwards, with finer appliances, quantitatively estimated in muscles; that is to say, with small current-densities a negative polarisation only was manifested; with greater current-densities and very short duration of closing a purely positive polarisation was manifested,



which passed over into a negative polarisation with the longer duration of the primary current. Here also the different manifestations of the nerve polarisation led, as in muscles, to the recognition of two simultaneous electromotive forces, which behave differently to the intensity and duration of the primary current. And as in muscle the direction of the primary current influenced the strength of the positive polarisation, similarly in nerves the direction had an influence upon the positive polarisation predominating in the motor nerve-roots when the current was a descending one, and conversely in the sensory nerve-roots when the current was an ascending one; consequently both times the direction of the physiological nerve-wave predominated. Finally, Prof. du Bois Reymond gave an account of his experiments by which he has demonstrated quite analogous secondary electromotor phenomena in the electric organs of the electric fish (*Malapterurus*). In the theoretical discussion of the results of these experiments that were carried on for so many years the lecturer pointed out in conclusion that the inner polarisation, the positive polarisation in particular, could scarcely be otherwise explained except by the hypothesis that in the above-mentioned organs (the muscles, nerves, and electric organs) electromotor molecules preexisted during life, which, being turned by the polarising current, became the occasioned causes of the electromotor phenomena.—Prof. Rosenthal of Erlangen spoke about the experiments he had made to ascertain the electric conductivity of living tissues. He dwelt on the difficulty of exactly measuring its amount, which he could only overcome by using alternating currents, of which, by the help of a particular apparatus, currents of one direction only acted upon the galvanometer of the Wheatstone's bridge. On the living man he found the resistance of the epidermis so great that he regards it as an excellent insulator which permits the electrical current to pass through to the deeper organs only through the medium of the canals (the pores) that ramify through it and that are filled with fluid. The measurements of the conductivity of living animal tissues are not yet quite completed.

PARIS

Academy of Sciences, May 7.—M. Blanchard in the chair.—M. Loewy explained his new method for determining at any moment the relative position of the instrumental equator in relation to the real equator. This method is analogous to that already given for right ascensions, being founded on the observation of the stars near the pole, and on the variations in the relations of the coordinates due to the deflection of the instrument. M. Loewy demonstrates mathematically that his plan combines all the theoretical and practical conditions required for the complete solution of the problem. It is based on the theorem here demonstrated that when the track described by a star in apparent distance from the pole coincides with its distance in relation to the instrumental plane, the angle may be exactly determined which is formed by the terrestrial axis with the line of the instrumental poles, by means of the variation observed between the apparent polar distance and the distance in relation to the instrumental plane. The method is independent of any possible variations in the state of the instrument during a period of twelve hours, and it excludes the cause of systematic error due to refraction. It is moreover capable of extreme accuracy, which, by multiplying the points, may be carried as far as is desirable.—M. Tresca submitted some remarks on the observations made last year by Prof. Lemström in Lapland on various circumstances connected with the phenomenon of the aurora borealis, which have been reported in NATURE.—M. Th. du Moncel presented a paper by M. E. Semmola on the annual variation of temperature in the waters of the Bay of Naples, showing the results of observations made during the summer of 1879 and January, 1880, with a Negretti and Zambra thermometer. The observations were generally taken during calm weather between the hours of 11 a.m. and 3 p.m., in depths of 30 or 40 feet, and at some distance from the coast. They showed that on the whole the Bay of Naples is only a few degrees warmer than the Mediterranean, which, from the observations made in the August of 1870 by the English expedition under Prof. Carpenter, was found to be 25° C. at the surface, 15°·5 at a depth of 180 feet, 14° at 230, 13° at 620, and nearly the same down to 10,000 feet. In the bay the temperature varied from 13° on the surface in winter to 27° in summer, showing a mean of about 20°, or 3° higher than the city of Naples. This result also agrees with the mean annual temperature of the Mediterranean, which, according to Mohn,

lies between 16° and 19° in the west, and 21°–23° in the east.—Other papers were contributed by M. Lecoq de Boisbaudran on the extremely sensitive character of salts of iridium, rendering them most useful in detecting the presence of the smallest particles of iridium in compound substances; by G. A. Hirn, continuing the *résumé* of the meteorological observations made during 1882 at four points of the Upper Rhine and Vosges highlands; by Th. Schwedoff, on the form of the great comet of September, 1882, with two cuts showing its appearance on October 12 at Lyons, and on October 17 and November 7 at Odessa; by E. de Jonquières, on the identities presented by the reductions belonging respectively to the two "modes" of continuous periodical fractions. By "the two modes" of continuous fractions the author understands, on the one hand the ordinary continuous fractions ("first mode"), on the other those in which the numerators differ from unity ("second mode").—Papers were also submitted by M. Vieille, on the specific heats of some gases at high temperatures; by C. Resio, on the electro-dynamograph, an instrument constructed for recording the work executed by machinery; by J. A. Le Bel, on the amylic alcohol developed in alcoholic fermentation; by M. Gonnard, on the staurolites and regular groupings of the felspar crystals in the siliceous porphyry of Four-la-Brouque, near Issoire (Puy-de-Dôme); by J. Thoulet, on the elasticity of rocks and minerals; by P. Mégnin, on the direct reproduction of tœnia in the intestines of the dog and man; by B. de Chancourtois, on a common meridian and measurement of time in view of the universal adoption of a complete decimal system, with a planisphere showing two proposed initial meridians passing through Behring Strait and the Azores; by Ch. Contejean, on some special cases of distribution in the Italian flora.

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