THURSDAY, NOVEMBER 17, 1881

SYSTEMATIC MINERALOGY

Text-Book of Systematic Mineralogy. By Hilary Bauerman, F.G.S., Associate of the Royal School of Mines. (London: Longmans, Green, and Co., 1881.)

THE text-books of Mineralogy written for English readers are so extremely small in number that the publication of a new one may be almost looked upon as an epoch in the history of the science. The text-books already in existence are in many respects very unsatisfactory, and in general character fall far below the standard of those of France and Germany. The symbols adopted in them for the faces of crystals are generally those of Naumann, whilst the simple and elegant symbols of Miller, if mentioned at all, seem to be regarded as intended for ornament rather than for use. As for the advances made by theoretic crystallography during the last quarter of a century, no account, save that to be found in the excellent little work of Mr. Gurney, has yet been offered to the English student.

Mr. Bauerman had thus a clear field and a splendid opportunity. It will be interesting to consider how far the result of his labour is worthy of the occasion.

We find, on opening the book, that the Descriptive Mineralogy, which the author's wide experience and extensive travel should render highly instructive, has been assigned through force of circumstances to a supplementary volume: of the 367 pages of the present one, 200 are given up to the geometrical, 100 to the physical, chiefly optical, and the remainder to the chemical and other properties of minerals. One immediately remarks with pleasure the numerous figures of crystals distributed throughout the work, all well drawn and clearly printed, and what is almost as important to the student, having wherever practicable the Millerian indices affixed to the faces.

On coming to the letterpress, however, one soon finds that there is something wrong; in fact, from the first page almost to the last there seems an evident disposition to perplex the reader. The style is very confusing throughout. On p. 5, for instance, we are informed that "quartzite and statuary marbles are aggregates of particles of quartz and calcite into masses of a slaty or granular texture." On p. 7 we find the following:—

"The leading property of crystals, as distinguished from mere geometrical solids, is the invariability of the angles between corresponding faces in different individuals of the same substance. There is usually a very marked *symmetry* to be noticed in the arrangement of their plane faces and edges, and occasionally of their points also, although the latter symmetry is not essential, crystallographic symmetry being one of direction and not of position, so that two parallel planes or two parallel lines are not distinguished from one another, and on that account the invariability of the angles is a paramount consideration."

Leaving out of sight the fact that the accuracy of the above distinction may be very reasonably contested, we much doubt the ability of the ordinary mineralogical student to master the compound nature of the latter sentence. On p. 10 we are puzzled on being told that "a polyhedron may be turned through an aliquot part of a whole revolution without its position in space as a whole being changed"; on p. 12 we find that the tetragonal system is characterised by the existence of *two* axes of binary symmetry, while on p. 112 *four* are mentioned; on p. 15 we are informed that if α, β, γ are all different, the molecular net-work has no symmetry, while on the following page it is said that, granted certain relations between these different quantities, there will be symmetry; on p. 19 we are told that the values of h, k, l are in no wise altered by multiplying them by any numerical co-efficient; and from p. 24 it will be concluded that a crystal with a "concave" angle is necessarily a twin.

On p. 36 it is stated that "the test of whether we have really four faces of *one* crystal is the rationality of this anharmonic sine-ratio when reduced to numbers." It is a well-known fact that in every zone of the cubic system, and in particular zones of the tetragonal and rhombohedral systems, the anharmonic ratio of any four planes belonging to *two* twinned crystals will be rational.

The account of the inter-relations of the various holohedral forms of each crystallographic system is not such as will relieve this branch of the subject from being still looked upon by the English student as somewhat dry and wearisome, while the attempt to evolve the hemihedral forms must cause the learner to despair. Thus the chapter on the Tetragonal System begins as follows:—

"The complete symmetry of this system is contained in an upright prism upon a square base, which has quaternary symmetry about a principal axis parallel to the vertical edges, and binary about four lateral axes."

Instead of an explanation there is then a statement of the fact that only certain permutations of the indices are possible: next follows a calculation, one of the principal features of which is the use of some spherical triangle characterised only by the fact that it is described *about* the pole of the principal axis and has a side represented by π, which symbol has been unfortunately selected to represent an *arbitrary* arc. The hemihedral forms are then arrived at by arranging the symbols of the faces of a complete form in a particular order, and then halving the faces, or the symbols, or the table, it is not clear which, in some symmetrical way not easy to discover (see pp. 89 and 120).

On p. 152 we find that, in the case of oblique crystals the axis of symmetry is a direction of "physical equivalence," while from p. 156 we should conclude that a face is "crystallographically possible" when it has a similar face parallel to itself.

In the discussion of twin crystals the relation in which the twin axis and the twin plane stand to the lines and planes of the crystalloid system is not clearly expressed. It may be worth while to point out that on p. 170 the somewhat common error is made of considering a face of the cube as the twin plane of the two interpenetrant tetrahedra, whereas a little reflection will make it clear that the twin plane is really a dodecahedral face. On p. 352 the striations on the cube faces of iron pyrites are, strange to say, referred to as *twin striations*. In describing a goniometer, of which the picture on p. 192 is no doubt very ideal, it is remarked that "the angle through which the circle is rotated will be the supplement of the dihedral angle required, *if it was originally set to zero*."

On the same page a probable injustice is done to the instrument, for we find that "in all cases the observation must be often repeated to obtain results of any value." What can be meant by the statement on p. 198, that the octahedron, or unit pyramid, is always the largest, and the cube rectangular prism, or pinakoid the smallest, of the constituent forms of any combination? On p. 205 we learn that density and hardness are common to all solids alike, and are therefore *independent of structure*. A definition (p. 212) informs us that a substance is *ductile* when it can be cut with a knife, but crushes to powder under a hammer, copper pyrites being cited as an example; and on the same page we find as an illustration of toughness of a mineral that "Malleable native Copper, especially when intimately mixed with siliceous vein-stuff and some varieties of Hematite and Iron Pyrites, has the property of toughness in a high degree."

The discussion of the optical properties is more unsatisfactory still. On p. 225 is to be found the following remarkable statement:—

"The movements in such a medium can only be reduced to order by supposing them to be made up of portions of homogeneous substances of different properties, and treating each one separately."

The proof of the existence of a minimum angle of deviation as submitted on p. 233 can scarcely be meant as serious; but to this curious proof should not have been appended an erroneous translation of the formula into words. On p. 235 the azimuths of vibration of an ordinary ray of light are said to change *continuously*, but so very rapidly that the changes are not perceptible to the eye; on the following page we read of "impulses having different velocities." Perhaps matters reach a climax about p. 245, where the intelligibility of the book for a brief period suffers total eclipse, as perhaps may be imagined when we read that "these orthogonal *forces* are called the *axes* of optical elasticity." On p. 263 one of the rays produced by double refraction is said to be "an extraordinary ray in all directions but those of the optic axes." Phosphorescence is, according to p. 290, "the power of emitting light in a dark place." On p. 293 we find a serious misapprehension as to the precise nature of the method employed by Fizeau for the determination of the expansion of crystals. One infers from the explanation here given that the interference rings, produced by help of a plane crystal surface and a lens, are *distorted* on change of temperature, and that the measurement of this distortion serves for the determination of the coefficient of dilatation for different directions in the plane surface; as a matter of fact Fizeau by his method determined with each crystal section the dilatation in only *one* direction, namely, that normal to the plane surface.

It is difficult to understand how Mr. Bauerman has contrived to allow so many loose statements to creep into his book; and we can only surmise that he has himself not had the time or the opportunity for a careful revision of the proofs. At any rate it will be evident from the above that a careful revision is absolutely required before the book can take its proper place in mineralogical literature. We trust that the present edition may be speedily disposed of, and that Mr. Bauerman will thus be enabled to offer to the English student a second and revised edition of a work, the want of which is urgently felt by every teacher of the subject. L. FLETCHER

A TREATISE ON CHEMISTRY

A Treatise on Chemistry. By H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors of Chemistry in the Victoria University, Owens College, Manchester. Vol. III. The Chemistry of the Hydrocarbons and their Derivatives, or Organic Chemistry. Part I. 8vo. (London: Macmillan and Co., 1881.)

THE term "Organic Chemistry" was originally used to denote the chemistry of compounds produced in the living vegetable or animal organism, all of which contain the element CARBON. For a long time indeed it was supposed that these compounds were peculiarly the products of living organisms, being formed under the influence of a so-called *vital force*, and that they could not be produced, like those of the mineral world, by artificial means. But the course of research has shown that this idea was erroneous, and that a large number of the more important organic bodies—hydrocarbons, alcohols, acids, &c., originally known only as products of the living organism—can be formed synthetically from their ultimate elements. The name "Organic Chemistry" has thus acquired a new signification, and in its widest sense is now used to denote the chemistry of carbon-compounds. As however some of these compounds, viz. the oxides and sulphide of carbon, have long been known to be producible by direct combination of their elements, and have accordingly been described in Manuals of Chemistry amongst inorganic compounds, the authors of the present treatise have thought it desirable to use the term "Organic Chemistry" in a somewhat narrower sense, viz. as signifying the CHEMISTRY OF THE HYDROCARBONS AND THEIR DERIVATIVES.

The volume commences with a historical sketch of the development of this department of the science, tracing it from the few facts respecting organic bodies known to the ancients, to the discoveries of Scheele, Lavoisier, Berzelius, Liebig, and numerous other workers, down to the present time—dwelling especially on the idea of Compound Radicals introduced by Lavoisier, and further developed by Berzelius, and by Liebig and Wöhler in their classical research on Bitter Almond Oil and its Derivatives, published in 1837—thence to the theories of Substitution and Types, founded chiefly on the researches of Dumas, and of Laurent and Gerhardt, and to the subsequent fusion of the Radical and Type theories brought about by the experiments of Williamson on Etherification, and those of Wurtz and Hofmann on the Compound Ammonias.

The next division of the work treats of the methods of ORGANIC ANALYSIS, which are explained in considerable detail, and illustrated by admirable diagrams; also of the determination of Vapour-density, in which the latest methods introduced by Victor Meyer are fully described, after which we come to the Determination of Molecular Formulæ, the explanations of which are characterised by singular clearness.

Next follows the CLASSIFICATION OF CARBON-COMPOUNDS, which are divided into groups according to the mode of linking of the carbon-atoms, the principal divisions being the Fatty or Paraffin group, in which the carbon-atoms are joined together in a single open chain, and of the Aromatic or Benzene group, in which the

atoms are supposed to form closed chains. Objections have sometimes been raised against this division as somewhat arbitrary; but we cannot help thinking that it is justified by the peculiar kind of isomerism, depending on the relative position or "orientation" of the substituted groups or radicals which exists amongst benzene derivatives.

The formation, constitution, and general properties of the Paraffins are next explained, together with those of the several groups of bodies, Alcohols, Acids, Amines, &c., derived from them by substitution, and the remainder of the volume is devoted to the special description of these several compounds, which are arranged according to the number of carbon-atoms contained in them, beginning with the lowest or Methyl-group, the fundamental hydrocarbon or paraffin of each group being first described, and then in succession the Alcohols, Ethers, Nitrogen-bases, Phosphorus-bases, Organo-metallic compounds, Aldehydes, Acids, Ketones, Sulphur-compounds, &c., derived from it.

All these compounds are clearly and ably discussed, especial attention being given to those which are of industrial importance, *e.g.* common Alcohol, Acetic Acid, and the Higher Acids of the Fatty series, which enter into the composition of soap. Several industrial processes are described in considerable detail, and amply illustrated by figures, *e.g.* the separation of the Paraffin-oils by fractional distillation, the manufacture and rectification of Alcohol, the testing of the strength of Spirits and of Wine and Beer, the preparation of Vinegar, and the manufacture of Soap. And here perhaps it may not be out of place to point out the great practical importance of Organic Chemistry, which, strange to say, has been called in question by some writers in the periodical press, who have spoken of it as consisting, in great part, of elaborate trifling about compounds of little practical importance, but rejoicing in names of fearful length, and formulæ of excruciating complexity--and in fact treating this branch of chemical science as altogether of second-rate importance in comparison with Mineral Chemistry. Now the importance of this last-mentioned branch of chemistry, which includes the description of the Metals and their Compounds, is of course beyond all question; but it is perhaps not too much to say that at least an equal value in a practical point of view may be ascribed to that department of the science which is concerned with the materials of our food and clothing, and with the constitution of the compounds which make up the bodies of plants and animals. To remove any doubt that may yet exist as to the practical importance of Organic Chemistry, we can imagine nothing more effectual than a perusal of the volume under consideration, the appearance of which will doubtless be hailed with pleasure by all who are interested in the subjects of which it treats. H. WATTS

OUR BOOK SHELF

Acoustics, Light, and Heat. By Thomas W. Piper. (London: George Philip and Son, 1881.)

THIS little work is not without its merits, the descriptions of the simpler phenomena and laws of these branches of physics being for the most part clear, accurate, and couched in easy language. The arrangement adopted in the chapters of the book is a departure, and we think not a very wise one, from the usual order of subjects in elementary text-books of physics. After a preliminary chapter on the atmosphere, its elasticity and its weight, the author plunges into vibratory motion, and under this head treats of acoustics. Chapter III. is on rectilinear motion, under which heading we have the following subjects:—The reflection of sound, the linear propagation of light, reflection of light and its applications, convection, radiation, and conduction of heat, laws of curved mirrors, laws of refraction, lenses, magic lantern, refraction of sound, spherical aberration of lenses, and, lastly, properties of matter. We have quoted these in the order in which they occur, and cannot help thinking that, however clearly the individual subjects are treated of, this heterogeneous lumping together of them must hamper the comprehension of beginners. Chapter IV. deals with the conservation of matter, including expansion by heat; Chapter V. is on thermometers. Chapter VI., on the conservation of energy, is another example of the author's peculiar method. It begins with the correlation of forces, deals with the prismatic spectrum, diathermancy, acoustic resonance, the laws of vibrations of strings, and specific heat. The book concludes with a chapter on sensation, optical and acoustical. Except for these aberrations of arrangement, and for one or two slips, the book would be a satisfactory one for beginners in natural philosophy.

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 ensure the appearance even of communications containing interesting and novel facts.*]

The Parasitic Habits of Molothrus

IN the "Origin of Species" I adopted the view maintained by some writers, that the cuckoo lays her eggs in other birds' nests, owing to her habit of laying them at intervals of two or three days; for it could hardly fail to be disadvantageous to her, more especially as she has to migrate at a very early period, to have young birds of different ages and eggs all together in the same nest. Nevertheless this occurs with the non-parasitic North American cuckoo. If it had not been for this latter case, it might have been argued that the habit of the common cuckoo to lay her eggs at much longer intervals of time than do most other birds, was an adaptation to give her time to search for foster-parents. The Rhea or South American ostrich is believed likewise to lay her eggs at intervals of two or three days, and several hens deposit their eggs in the same nest on which the male sits; so that one hen may almost be said to be parasitic on another hen. These facts formerly made me very curious to learn how the several species of Molothrus, which are parasitic on other birds in very varying degrees, laid their eggs; and I have just received a letter from Mr. W. Nation, dated Lima, September 22, 1881, giving me information on this head. He says that he has there kept in confinement for a long time *Molothrus perpurascens*, and has likewise observed its habits in a state of nature. It is a resident species of Western Peru, and lays its eggs exclusively in the nests of a sparrow (*Zonotrichia*), starling (*Sturnella bellicosa*), and a pipit (*Ænthus chii*). He then proceeds: "The eggs of the sparrow are very much like those of the Molothrus in size and colour. The eggs of the starling are larger and somewhat different in colour; while the eggs of the pipit are very different both in size and colour. Generally one egg of the Molothrus is found in a nest, but I have found as many as six. The young Molothrus does not always eject its foster-brothers; for I have seen a young one nearly fully feathered in a nest with two young

starlings. I have also found two young birds of the Molothrus nearly fully feathered in the nest of a starling; but in this instance the young starlings had been ejected from the nest." He then states that he had long kept in confinement a male and female of this species of Molothrus, which are now six years old. The hen began to lay at the age of two years, and has laid each time six eggs, which is the number laid by Icterus, a near ally of Molothrus. The dates on which the eggs were laid this year are as follows:—February 1, 6, 11, 16, 21, and 26; so that there was an interval of exactly four clear days between the laying of each egg. Later in the season she laid six additional eggs, but at much longer intervals and irregularly, viz. on March 8, April 6 and 13, May 1, 16, and 21. These interesting facts, observed by Mr. Nation in relation to a bird so widely distinct from the cuckoo as is the Molothrus, strongly support the conclusion that there is some close connection between parasitism and the laying of eggs at considerable intervals of time. Mr. Nation adds that in the genus Molothrus, out of every three young birds he has invariably found two to be males; whereas with Sturnella, which lays only three eggs, two of the young birds are, without any exception, females.

CHARLES DARWIN

Down, Beckenham, Kent, November 7

The Velocity of Light

IN reply to Mr. Macaulay (NATURE, vol. xxiv. p. 556) I will endeavour to explain more clearly the statements, made in my former communication on this subject (NATURE, vol. xxiv. p. 382). On one important point the explanation will include a correction.

With reference to the group-velocity U, we know from Fourier's theorem that any disturbance travelling in one dimension, can be regarded as resulting from the superposition of infinite trains of waves of the harmonic type, and of various amplitudes and wave-lengths. And we know that any one of these trains, of wave-length λ, is propagated unchanged with a velocity V, which we regard as a known function of λ, dependent upon the nature of the medium.

Unless we can deal with phases, a simple train of waves presents no mark by which its parts can be identified. The introduction of such a mark necessarily involves a departure from the original simplicity of a single train, and we have to consider how in accordance with Fourier's theorem the new state of things is to be represented. The only case in which we can expect a simple result is when the mark is of such a character that it leaves a considerable number of consecutive waves still sensibly of the given harmonic type, though the wave-length and amplitude may vary within moderate limits at points whose distance amounts to a very large multiple of λ. We will therefore suppose that the complete expression by Fourier's series involves only wave-lengths which differ but little from one another, and accordingly write it—

$$a_1 \cos\{(n+\delta n_1)t-(\kappa+\delta\kappa_1)x+\epsilon_1\} + a_2\cos\{(n+\delta n_2)t-(\kappa+\delta\kappa_2)x+\epsilon_2\}+\ \dots\ ,$$

or in the equivalent form—

$$\cos(nt-\kappa x)\,\Sigma a_1\cos(\delta n_1 t-\delta\kappa_1 x+\epsilon_1) - \sin(nt-\kappa n)\,\Sigma a_1 \sin(\delta n_1 t-\delta\kappa_1 x+\epsilon_1),$$

where $\kappa=2\pi/\lambda$, and $n=\kappa V$. From this we see that, as in accordance with the suppositions already made,

$$\frac{\delta n_1}{\delta\kappa_1}=\frac{\delta n_2}{\delta\kappa_2}=\ \dots\ =\frac{dn}{d\kappa},$$

the deviations from the simple harmonic type travel with velocity $dn/d\kappa$, and not with velocity n/κ, that is with velocity $d(\kappa V)/d\kappa$, and not with velocity V.

I now pass on to the theory of Foucault's experiment. If D be the distance between the fixed and moving mirrors, ω the angular velocity of the latter, then the angle through which the mirror turns in the time occupied by the wave in making the double journey is $2D\omega/V$, and the angular deflection θ, which is the immediate subject of observation, is according to the usual view—

$$\theta=\frac{4D\omega}{V}.$$

Now it is here assumed that the deflection is due merely to the change of position of the mirror between the two reflections, and that the wave returns to the mirror with its front parallel to the position occupied immediately after the first reflection, as would be the case if the mirror were at rest. But if V be a function of λ, this assumption is not true. Besides the deflection above considered, there is another depending upon the fact that the wave front rotates in the air between the two reflections. The rotation is a consequence of the inclination to one another of successive wave fronts, which involves a variation of wave-length and therefore of velocity at points situated on the same wave-front in a line perpendicular to the axis of rotation. Denoting distances measured along this line by x, we have for the angular velocity of the wave's rotation—

$$\omega'=\frac{dV}{dx}=\frac{dV}{d\lambda}\frac{d\lambda}{dx},$$

in which $d\lambda/dx$, representing the angle between successive wave-fronts of similar phase, is equal to $2\omega\lambda/V$. Accordingly—

$$\omega'=2\omega\frac{d\log V}{d\log\lambda},$$

and the actually observed rotation is—

$$\theta=\frac{4D\omega}{V}\left(1-\frac{d\log V}{d\log\lambda}\right).$$

The result of a calculation which leaves the aërial rotation out of account is therefore not V, but—

$$\frac{V}{1-\dfrac{d\log V}{d\log\lambda}}$$

Now

$$U=\frac{d(\kappa V)}{d\kappa}=V\left(1+\frac{d\log V}{d\log\kappa}\right)=V\left(1-\frac{d\log V}{d\log\lambda}\right);$$

so that the result of the experiment is V^2/U, and not as previously stated the group velocity U itself. The error arose from a mistake as to the direction of the effect of ω'.

The force of the arguments which I founded upon these considerations is increased rather than diminished by the correction, and with Mr. Michelson's evidence on the same side of the question almost excludes any appreciable variation of V. It should be noticed that by the combination of the two methods of the toothed wheel and of the revolving mirror we have the means of determining both V and U, and the results of Cornu and Michelson appear to prove, independently of astronomical observation, that there is no sensible difference between them.

Indeed by a slightly varied arrangement it would seem possible to determine V directly from Foucault's experiment. If a convex lens were so interposed at the distant station that the fixed mirror occupied its focus, the sides of short and long wave-length would be in erchanged, and thus the rotation acquired during the outward journey would be neutralised during the return.

RAYLEIGH

The Struggle of Parts in the Organism

I AM very glad to learn that Mr. Romanes fully accepts as "well-known and unquestionable" the definition of the term *law of nature* which I propounded as expressing its true scientific sense; but I would suggest to him, as to other writers who are accustomed to speak of such laws as "governing" phenomena,[1] whether the use of such "metaphorical" language is not objectionable, as tending to keep up in the *un*scientific mind the notion of the "coercive" and "self-sufficient" agency of natural laws. I am glad also to be able to express my entire accordance with Mr. Romanes in regard to the inferiority of the teleological argument based on *special instances* of adaptation of means to ends, to that which is based on the *general order* which we designate by the term law. For I maintained this view even in that remote pre-Darwinian age in which my scientific life commenced, urging to the best of my young ability, forty-three years ago,[2] that the principles admirably laid down by Whewell in regard to physical inquiry, viz. that final causes should be excluded, because "we are not to assume that we know the objects of the Creator's design, and put the assumed purpose in the place of a physical cause," and that "the notion of design and end is transferred by the researches of science from the

[1] I continually meet with this phrase in the pages of NATURE.

[2] See *British and Foreign Medical Review* for April, 1838: "Physiology an Inductive Science."

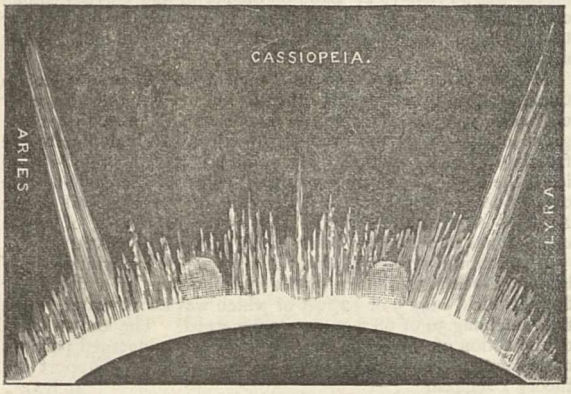
region of facts to that of laws," are no less applicable to physiology than to physics; although Whewell himself (in his "History of the Inductive Sciences") had maintained the contrary. The full acceptance of the doctrine of evolution as our highest expression of the *order* of creation seems to me to lead to a much nobler conception of the Intelligent Cause of that order than any accumulation of such individual adaptations as might be made by the "mechanic-god" of Paley.

WILLIAM B. CARPENTER

56, Regent's Park Road, N.W., November 14

The Aurora and its Spectrum

YOU have already illustrated in NATURE the aurora and electric storm of January 31. Though somewhat tardily, another drawing with a description of this fine display has found its way into my hands, of sufficient interest, I think, to be added to those already published. It comes from Mr. C. L. Prince, the well-known meteorologist of Crowborough Beacon, Tunbridge Wells, who says, "I inclose a photo of my sketch on a slate.[1] I had a splendid sight of the aurora at about 9h. 15m. It soon became very hazy, but I saw it again at 11h. 15m., when I made the sketch (see drawing). The arch was exceedingly well defined, and extended about 80° along the horizon. At 11h. 20m. some more brilliant streamers shot up along the whole convexity of the arch, and the two *patches* of *light* became very tremulous, almost shifting a little from right to left; but I particularly noticed that they did not vibrate *simultaneously*, *i.e.* if one indicated motion the other was quiescent *until* the first had ceased to show excitement, and this action was alternating for nearly an hour. At 11h. 40m. the arch had much contracted, and by midnight had nearly faded away. The whole phenomenon was



free from colour. I noticed a few small meteors. The night was quite calm; wind south-west."

This account seems to me interesting in connection with an observation made by my friend Dr. Vogel of Potsdam, that during the aurora of April 9, 1871, certain lines in the spectrum alternated in intensity with the character of the discharge, some brightening as others faded.

Mr. Prince does not mention any spectroscopic observations, and while noting this will you permit me to make a few remarks on the "spectrum of the aurora," an old hobby of mine. I notice that Dr. Spottiswoode, P.R.S., in his lecture delivered before the British Association at York on September 5 last (NATURE, vol. xxiv. pp. 572, 3, "On the Electric Discharge, its Forms and Functions,") has referred to the aurora in connection with experiments showing that the discharge in rarefied gases differs from that at higher pressures, and that the difference corresponds to that observed between the flickering play of the aurora and the crashing spark of the lightning-flash. After then referring to the questions of height and colour of the auroral discharge, Prof. Stokes' theoretic view of the connection of earth-currents, disturbances of the magnetic needle, and solar radiation is dwelt upon. In NATURE, vol. xxiv. pp. 613-18 (lecture by Prof. Stokes, Sec.R.S., in the South Kensington Theatre, on "Solar Physics"), this theoretic view is set forth, and the aurora is described as a flash of lightning passing through the higher regions of the atmosphere where the air is rarefied. There are, I think, some objections to this theory founded upon certain circumstances of the aurora itself, such, for instance, as the well-authenticated cases of auroræ seen close upon the surface of the earth.

The passage, however, in Prof. Stokes' lecture which particularly struck me is this: "But what of the auroræ? It has long been recognised that the aurora is an electrical phenomenon. It has been supposed to be imitated, *and there can be no doubt that the supposition is a correct one* [the italics are mine] by sending an ordinary electric discharge through a highly exhausted tube."

Now it may be true that the aurora is thus imitated so far as external appearance is concerned, and it has long been a favourite idea that this imitation in some way extended to the aurora's actual composition; but what does that Ithuriel spear, the spectroscope, say upon that point—a point which gains the more importance from the fact that such an instrument is mentioned by Prof. Stokes (p. 614) as the true touchstone for the aurora? It says positively that Prof. Piazzi Smyth's citron line, the one true test of the aurora, has never yet been seen in any electric discharge whatever which we have yet produced, whether in air at ordinary pressure, or rarefied; that the red line (its companion in some auroræ) is equally noticeable for its absence therefrom; and that of the remaining faint and less marked lines one or two only have with doubt and uncertainty been by some referred to the air spectrum as excited by the electric spark or glow. The late Prof. Ångström endeavoured to place some of these fainter lines in accord with the spectrum bands of the violet pole in Geissler air-tubes, but the comparison failed on critical examination. Prof. Vogel has also considered the aurora might probably be an air-spectrum modified by conditions of temperature and pressure. The Professor's actual line comparisons, however, quite failed as to the citron and red lines, and could hardly be called a success as to the fainter ones. In fact any analogy between the aurora spectrum and the spectrum of the electric discharge in air is all but hypothetical, and the aurora still maintains that mysterious quality which distinguishes it from electric discharges of all sorts, and indeed everything else, viz. its peculiar spectrum.

I therefore again plead the necessity for spectrum observations in connection with the aurora, a point from which Prof. Stokes' lecture, I am afraid, owing to its absence of any remarks on the subject (save that before referred to) is somewhat calculated to draw them away. It is certainly *possible* that some special gas *may* exist in the upper regions of the atmosphere giving rise to the citron, perhaps the red, lines; but then (as Prof. Smyth remarks) if so, why, being an emission spectrum in the aurora, does it not, according to the theory of exchanges, appear as an absorption spectrum or dark lines in the solar spectrum? Even, too, could this be shown, it would still remain an unexplained fact that such a gas has hitherto failed to be recognised in any other body, celestial or terrestial.

To sum the matter up, the electric discharges in vacuum-tubes, as tested by Prof. Stokes' prism and slit, no more represent the aurora than did the cirrus cloud illuminated by the light of the moon, mentioned by him, which also simulated it.

Upon reliable authority the spectrum of lightning may be considered that ordinarily given by a spark in air; but when we come to rarefied discharges and the aurora, the same comparison does not hold good; and where the electrician has to leave the matter, the spectroscopist has yet to take it up.

To aid in solving the aurora's mystery I would invite all spectroscopists, armed with suitable instruments, persistently to aim at accurate micrometer readings of the aurora spectrum. The approximate places of the lines are pretty well established, but their actual wave-length positions are much wanted, for so only may we hope to master one of the remaining riddles of science.

J. RAND CAPRON

Guildown, November 1

[1] I commend this method of sketching to other observers, the slate pencil showing white on the dark slate being readily photographed with good effect, displaying the aurora light on a dark ground.—J. R. C.

Arctic Research

"PROGRESS of Arctic Research since the Foundation of the British Association," by C. R. Markham, C.B., F.R.S. Such is the title of a very able and instructive paper read before the Geographical Section of the British Association at York on September 6 last, and published *in extenso* in the November number of the *Proceedings* of the Royal Geographical Society. The casual reader of this history may suppose it to be a fair and correct record of half a century of Arctic exploration, and that

the names of distinguished men commanding naval expeditions, who themselves, or the officers under them, did a large amount of discovery and good scientific work, are mentioned, however briefly. Yet this is far from being the case. True, some of those expeditions which have been considered unworthy of notice were sent to the far north to gain tidings of the lost expedition under the good and noble but unfortunate Franklin; yet, in addition to doing an immense extent of sledging, by which many hundred miles of *new* coast were traced, they collected much scientific information, little, if any, less valuable than that brought home by the Nares Expedition, whose object was purely scientific.

The names of Kellett, Belcher, and Austin are conspicuous by their absence, except a notice of the officer who acted as second to the latter, and who made a comparatively short sledge journey. We can scarcely suppose that the author was in any way influenced by the fact that he himself sailed in the same vessel under the immediate command of the officer whom he selects for notice, whilst wholly ignoring Admiral Austin, the chief in command.

The names of the Americans, Kane and Hayes, who with very inferior means traced *more* than 300 *miles*[1] of the unknown shores of Smith's Sound, are left out, although each was awarded the gold medal of the Royal Geographical Society—the highest honour that can be bestowed on an explorer—for their Arctic discoveries, as being the most important of the year in which they were made. These are a few examples to prove what has been said.

The author attempts to show that the system of sledging adopted by the Government Naval Expeditions is all but perfect, and that the first long sledge journeys were performed by them. Both these statements admit of question. The sledging arrangements are more or less defective in almost every particular. The tents are bad shelters, far inferior to snow-huts; the sleeping-bags are objectionable, as they prevent transmission of caloric from one individual to the other. The bag itself, being exposed all day to a temperature of perhaps 20° or 30° below zero, is so cold when the occupant gets inside, that all the moisture from the breath, &c., gets condensed upon it, making this bed after a few days' use like "sheet iron" (as remarked by a naval officer who had some unpleasant experience of the fact) and afterwards, when thawed by the extraction of most of the animal heat from the shivering tenant, becomes a veritable "wet blanket." Besides this blanket bag each man before going to rest (?) struggled into what is called a "jumper" made of thick duffle, cold as the other night-gear, thrusting the arms into the sleeves, thus keeping these unfortunate members separated by two folds of a thick non-conductor from the body that owned them, a procedure wholly opposed to both experience and science. To conserve the animal heat the Indian and white *voyageur* either take off their coats or take their arms out of the sleeves when camping out in winter. The Eskimo strips to the waist in his snow hut. It is only the naval Arctic sledge parties that act differently, with very uncomfortable results. The construction of the sledges is very defective for certain conditions of snow when in a semi-packed state—the sledge-runners then sink down six or seven inches, and have to be dragged with great difficulty *through* the snow, not *over* it. The dead weight—exclusive of food and fuel—to be hauled on this "admirable system" usually amounted to 85 or 90 lbs. per man, which weight has been reduced under another system to 35 or 40 lbs. per man without loss of efficiency.

"Long-sledge journeys," which Mr. Markham claims to have been initiated by the Government Naval Expeditions, were made at the rate of twenty miles or more a day by civilians, before the "naval system" was put in use, the first occasion of which was by the most experienced of Arctic explorers, Sir James C. Ross, in 1849, when the results were by no means satisfactory, considering the large number of men employed; a small party of five Hudson's Bay men having accomplished more than double the distance at nearly double the daily rate two years before over very difficult ice.

Mr. Markham "formulates three canons," which, he says, "are equally important, as the true methods for future Polar research," but the latter of these, namely, "to reach an advanced position within the unknown area, it is necessary to follow a coast-line trending northwards, *with a western aspect*, is of most interest to the geographical inquirer."

[1] This distance (300 miles) of new discoveries is that credited to the Nares Expedition by Mr. Markham.

The four words I wish to comment upon are in italics. Had not Mr. Markham given examples of his meaning by mentioning Foxe's Channel and Regent's Inlet, I should have supposed "*western aspect*" a misprint.

It is a curious but certain fact, established by men who have been there, that the shores of all bays and inlets on the northern coast of America, the shores of all inlets and of the great islands lying north of America, having a *western aspect*, are usually ice-blocked, whilst the shores having an *eastern aspect* are comparatively free from ice and navigable. As cases in point Hudson's Bay, Regent's Inlet, Victoria Strait, the shores of Banks Land and Melvile Island, also Smith Sound,[1] are all far more ice-obstructed on the shores having a western than on those having an eastern aspect.[2]

The greater frequency of westerly and north-westerly winds and gales in the western hemisphere in high latitudes are of themselves sufficient to produce the effect I have mentioned, and to form the so-called "ancient ice" in a month or less.

The route by the west shore of Franz-Josef Land is named as favourable for getting far to the north. This may be true, but from the formation of the land it may be considered as by no means safe. The most experienced navigators of these seas state that Franz-Josef Land may perhaps be reached once in every four or five years, as the condition of the ice in Barentz Sea is very uncertain. Supposing this to be true, might not the vessel that was fortunate enough to reach these interesting islands be detained there rather longer than was either safe or agreeable? Already some not unreasonable anxiety is felt for Mr. Leigh Smith in his fine vessel manned by able icemen, who, it is believed, sailed in the direction indicated. Fortunately he is in every respect well provided to face *one* Arctic winter.

That a large quantity of heavy ice is to be generally met with in the Sea of Barentz is not difficult to account for—Spitzbergen on the west, Franz-Josef Land on the north, and Novia Zemblia on the east, form a sort of *cul-de-sac*, into which the floes appear to be drifted about by winds and currents in a most erratic manner.

Mr. Markham states that the "ancient ice" 80 or 100 feet in thickness seen by Nares and other Arctic navigators is the result of "*slow accretions*," meaning, I presume thereby, that these great ice-masses are the gradual growth of a single floe by the freezings of many winters. My opinion is that this thick-ribbed ice is the result of extreme pressure, which has forced one floe over or under the other, as in the case of the upheaval of the Austrian ship with Weyprecht and Payer in 1873-4, of which mention has already been made in the columns of NATURE.

It is doubtful if the extremest cold of an Arctic winter would have any freezing influence on the lower surface of a floe 60 or 80 feet thick, only 9 or 12 feet of which was above water, whilst the remaining 50 or 68 feet were submerged. The known effect of the action of the sea in wasting away the submerged portions of icebergs, even when these are not large, tends to support the theory I suggest.

J. RAE

4, Addison Gardens, W., November 5

[1] The author says that Sir G. Nares ascended Smith Sound having a northern trending, but he does not tell us that both on the outward and homeward voyage he kept to the shore having an *eastern aspect*—a fact.

[2] The east coast of Greenland is an exception to this rule, being ice-encumbered by the force of a well known strong current pressing the floes against it.

A Photographic Experiment with Swan's Incandescent Light

SOME trials I have recently made with Swan's incandescent electric lamps give results that may possibly interest your readers. I employed throughout an electric stream of the same energy—that generated by thirty Grove cells, and as the whole experiment lasted but an hour, it may be assumed, for all practical purposes, that the strength of the current was uniform throughout.

I made use of four lamps in all, supplied promiscuously from Newcastle, their resistances being respectively: No. 1, 67 ohms; No. 2, 59 ohms; No. 3, 58 ohms; No. 4, 52 ohms. My object was to ascertain their actinic power upon a gelatino-bromide film, individually, collectively, and in groups. To do this I exposed a gelatino-bromide film to the action of one light at a distance of 14 feet, the sensitive film being placed behind a screen more or less transparent. The screen was divided into small squares, each representing different thicknesses of fine tissue paper. Thus: square marked No. 1 represented one thickness; No. 2, two thicknesses; No. 3, three thicknesses, and so on;

the last square being almost opaque, and representing twenty-five thicknesses of tissue paper. The exposure in every case was ten seconds; the distance of the film from one light or lights always 14 feet, and the number of cells 30. The development was confined to two minutes exactly, an oxalate developer of normal strength being employed [1]:—

Result of First Experiment

	No. of square through which the light penetrated.
Group of four lamps (Nos. 1, 2, 3, 4)	13
,, three ,, (Nos. 2, 3, 4)	15
,, two ,, (Nos. 3, 4)	19
,, one lamp (No. 4, low resistance)	23
,, ,, (No. 3, high resistance)	20

As it was a matter of difficulty to judge the exact square or number printed through, the mean results of three observers or readers was taken. So that the development should be the same throughout, all plates were developed simultaneously in the same dish.

The result may not permit us to estimate with scientific accuracy the value of the lights under *the above conditions*, but it proves practically (1) that the amount of light given off by four lamps is less than that given by three, and that the electricity is employed most economically for lighting when only one lamp is used; and (2) that, at any rate in the conditions described, a comparatively low-resistance lamp gives more light than a comparatively high one.

In my second experiment I estimated the actinic power of single lamps, when one or more were in circuit, the photographic arrangements being the same.

Result of Second Experiment

	No. of square through which the light penetrated.
One lamp, No. 4 (only one in circuit)	23
One lamp, No. 4 (two in circuit)	18
One lamp, No. 4 (three in circuit)	14
One lamp, No. 4 (four in circuit)	9
Arc light (with 30 cells) ... ... upwards of	25
Ordinary fish-tail burner (burning 5 cubic feet per hour)	7

The result here is interesting in showing the comparative strength of the lamps by themselves, and to what extent the light, so to speak, is "turned down" by bringing another lamp into circuit. As my screen was only graduated as far as 25, it was impossible to estimate the comparative value of the arc light, for it went above this degree, while a low-resistance Swan lamp only goes as far as 23. This last lamp, No. 4 in the series, was the one always tested, and therefore the results shown may be considered the most favourable. The result given by a good fish-tail burner permits us to make some comparison between gas and the Swan light at a minimum.

It says something for the skill with which the sensitive gelatino-bromide is prepared commercially nowadays, when we find it is so uniformly sensitive that in the two trials (where square 23 is recorded) there should be so unanimous a result.

November 12 H. BADEN PRITCHARD

[1] Developer:—
Saturated solution of oxalate of potash ... ... 3 parts.
Saturated solution of proto-sulphate of iron ... ... 1 part.

Sound-producing Ants

REFERRING to Mr. Blanford's letter in NATURE, vol. xxv. p. 32; whilst lying awake early one morning before the servants were stirring, when camped in the Deccan at the present small station of Chota or Chick-Soogoor, on the G.I.P. Railway, during the winter of 1868-69, I heard a sound, as Mr. Blanford suggests, repeated at regular intervals of about a second. It sounded as though the wall of the tent was being struck by a light fringe along one side; but noticing that the air was perfectly still, I listened for some minutes, wondering what it was and trying to fix the locality. I got out of bed cautiously and looked out; the whole of one side of the tent for a height of two feet was covered with white ants so thickly that at the first glance I thought the wall was covered with a grey-reddish mud to this height. I was trying to make out how the sound could be produced, when it stopped suddenly, the ants evidently having become aware of my presence; they then began to clear off the wall rapidly, and in a few moments it had become white again. They had not attacked the cotton wall. On looking at the ground round the tent I found their runs extending from a tamarind tope, the nearest trees of which were about 50 feet away. The runs were built in the usual way of red Deccan soil, there were great numbers of them, varying in width from the thickness of twine to 2½ inches, often crossing each other diagonally. No doubt the ants had found the tent in the evening, and were prepared to make a morning meal of a portion of it, when I disturbed them on looking out. The impression on my mind at the time was that the whole body of ants struck the tent wall at the same time with their heads, and that it was very extraordinary that they all stopped at once; there were no dropping shots, it was an instantaneous cessation along the whole line. The tent, which was a double one, was at least 30 feet long, and the ants possibly extended round the ends. JOHN FOTHERINGHAM

13, Springfield Road, N.W., November 12

UNIVERSITY COLLEGE, LONDON.—The Calendar will give you all the information you want.

"FIFTY YEARS' WORK OF THE GEOGRAPHICAL SOCIETY."—We believe Mr. Markham's narrative may be ordered through any bookseller. The Polar Observing Station at Lady Franklin Bay was really occupied by a United States party in the past summer, with the intention of carrying on observations for at least a year.

SEALS IN LAKE BAIKAL.—Mr. Edward Fry refers Mr. Keane to Bell's "British Quadrupeds," 2nd edit. p. 248, where he will find that Herr Radde figures and describes the seal of Lake Baikal in his "Reise im Süden von Ost-Siberien"; and to Murray's "Geographical Distribution of Mammals," p. 126. Mr. Thomas Ward sends the following references:—Myer's "Geography, vol. ii. p. 9, edition 1829; Erman's "Travels in Siberia," vol. ii. p. 200 (Cooley's translation); "English Cyclopædia" (Article Baikal). From this last Mr. Ward quotes as follows:—"The existence of the salmon, of the seal, and of a kind of sponge in the fresh water of the Baikal has given rise to many speculations among naturalists. Pallas and Georgi are unable to explain this phenomenon otherwise than on the supposition that the Lake of Baikal at some remote period formed a part of the Northern Ocean, . . . or on another supposition, that these animals were transported into the lake by some excessive inundation of the Lena River, whose sources are not far from its western borders."

HEADS AND HATS

WE have received the following further communications on this subject:—

HAVING last March laid the subject referred to by your correspondents, Messrs. Kesteven and Hyde Clarke, before the Bristol Naturalists' Society in a short paper, I venture to offer a *résumé* of the facts collected by my friend Dr. Beddoe and myself, which seem to justify the conclusion that a diminished size of hat is now required by young men as compared with those used by the same classes twenty to twenty-five years ago. (1) I have from time to time during the last three or four years had my attention called to this alleged change by Mr. R. Castle, hatter and hosier of 1, St. Augustine's Parade, Bristol, who, in proof of it, has frequently shown me hats of small size, such as are now generally required by both gentlemen and servants between the ages of twenty and thirty. He states that these smaller hats, which used to constitute only a small percentage of his stock, now form the bulk of it, whilst those which formerly suited the larger proportion of his customers are now usually required only by the older ones amongst them. Mr. Castle estimates the difference as amounting to at least one whole size, which is equivalent to three-eighths of an inch in circumference, and he has furnished me with typical extracts from his order book to Messrs. Lincoln and Bennett in fuller proof of his assertion. I have arranged and reduced his figures in the accompanying table, and the result does undoubtedly seem to be that the buyers of 1875-80 are taking a hat at least one size smaller than the same *class* (not necessarily the same individuals) used to do twenty to twenty-five years previously. (2) My friend Dr. Beddoe, whose attention I called to the subject last year, informed me that Mr. C. Garlick, hat manufacturer of 87, Castle Street, Bristol, furnished him with the sizes of 200 hats sold by him in 1862, and the average is precisely 7, one of the lots yielding 7·01 and the other 6·99. Two

Orders for Hats from Messrs. Lincoln and Bennett in 1855, 1875, 1878, *and* 1881

Sizes.	6½	6⅝	6¾	6⅞	7	7⅛	7¼	7⅜	7½	Total	Avge.	Remarks.
Circumference.	20½ in.	20⅞	21¼	21⅝	22	22⅜	22¾	23⅛	23½	No.	Size.	
1855		1	2	4	4	4	4	2	1	22	7 3/32	7 1/16
,,				4	3	3	2			12	7 1/32	
1875		2	4	6	6	4	2			24	6 29/32	Average shrinkage about ¼(·014), or rather more than one size, which amounts to ⅛. Circumference of head diminished by nearly ⅛ an inch.
,,		1	3	2	3	2	1			12	6 31/32	
1878		1	3	8	4	6	2			24	6 15/16	6 1/16
,,	1	3	3	3	1	1				12	6 27/32	
1880		1	4	4	2	1				12	6 29/32	
,,	1	6	3	6	5	2	1			24	6 31/32	These are all livery hats and are excluded from the averages.

lots of 100 each sold by him in 1880 averaged respectively 6·89 and 6·92, or a mean of 6·905. Thus the shrinkage since 1862 appears to be about 0·1 of the technical scale usually employed by the trade, of which 0·125 (⅛) represent a difference of *one size*, but a difference in the *circumference of the head* of ⅜ths (=0·375) of an inch. Therefore the above 0·1 deduced by Dr. Beddoe from Mr. Garlick's figures represents a shrinkage in circumference of over ⅛ of an inch, which agrees pretty closely with my previous result of "nearly ½ an inch" from Mr. Castle's data. (3) While in Scotland during the summer of 1880, Dr. Beddoe learned from the principal hatter in Glasgow that his experience fully corroborated what has been stated, so that the diminution appears not to be confined to the southern portion of the kingdom. (4) Mr. Mordey, hat manufacturer, of 159, Blackfriars Road, London, wrote me on February 22 as follows:—"In answer to your inquiry I beg to say that my experience tells me that men's heads have decreased in size during the last twenty years. Twenty years ago the circumference of men's heads ran from 21¼ to 23⅜ inches. At the present time the size is from 21 to 22¾—mostly 21 to 22⅜. This decrease is so general that we do not make big sized hats for stock, but only as ordered, and very few then." (5) Another hat manufacturer writes:—"Fifteen years ago the usual sizes of hats in England were from 6¾ to 7⅜, and even 7½ was not uncommon. But now if a 7⅜ hat were wanted we should have to make a block purposely." This may be sufficient evidence to show the probable accuracy of those who assert the fact of shrinkage in the size of hats, and it only remains to add a few words as to the possible cause. To the somewhat obvious suggestion that the practice of wearing the hair more closely cropped might account for the difference, Mr. Castle, as a practical hatter, replies that the effect of this would be scarcely perceptible, and further urges that the less the head is protected by a cushion of hair, the easier must be the fit of the hat, to prevent friction and ensure comfort. The same view is taken by the manufacturer quoted in paragraph (5), who writes, "this solution of the matter is inadmissible." Another suggestion is that the mode of wearing hats has changed, and the present style admits of a smaller size. On this point Mr. J. C. Withers, hat manufacturer, of 80 and 81, Castle Street, Bristol, who has been in the trade upwards of thirty years, writes as follows:—"I am well aware that the size has considerably decreased within the last twenty to twenty-five years, but I attribute this entirely to the manner in which they are now worn, which is far more forward on the head than formerly. If I were to wear my hat as my grandfather did I should take one quite a size larger. When I was first at the trade I well remember that all hats had a cloth patch sewn on the under side of the brim at the back for the purpose of taking the friction off the coat collar, and thirty-five years ago we never made a hat without one." This explanation, I confess, sounds plausible; but though I well remember the cloth patch, so far as my memory serves it scarcely seems to me that the mode of wearing the hat has sufficiently changed within the interval (fifteen to twenty-five years) stated by the various authorities quoted to be adopted as offering a solution of the problem. In *Public Opinion* for May 28, 1881, is a letter on the subject signed "F. J.," which concludes thus: "This really does not account for the change, as hatters can testify. Twenty-five years have made little difference in the way of wearing hats, and it is during the last twenty-five years that the change has taken place." By Dr. Beddoe's kind permission I am enabled to add a curious list of the sizes of hats worn by several eminent men, which was sent to him by Mr. Garlick, who obtained it from a friend in London:—

Lord Chelmsford ..	6½ full.	Earl Russell	7¼
Dean Stanley	6¾	Lord Macaulay ...	7⅜
Lord Beaconsfield ...	7	Mr. Gladstone ...	7⅜
H.R.H. the Prince of Wales	7 full.	Mr. Thackeray ...	7⅜
Charles Dickens ...	7⅛	Louis Philippe ...	7¾
Lord Selborne ...	7⅛	M. Julien[1]	7¾
John Bright	7⅛	Archbishop of York	8 full.

In conclusion, to quote the remarks on my paper of a writer in the *Bristol Daily Press*, "In future the familiar expression, borrowed from Milton, of an opponent 'hiding his diminished head,' will possess a special significance. Fuller alludes, in his dissertation on 'Natural Fools,' to persons whose heads are 'sometimes so little that there is no room for wit, and sometimes so long that there is no wit for so much room,' so that, possibly, a slight diminution in the cranium is not an unmixed evil. There is, at any rate, no marked deterioration in the mental faculties, so critics may still find themselves in the position of the rustics who gazed in wonder at Goldsmith's village parson—

"'And still they gazed, and still the wonder grew
How one small head could carry all he knew.'"

If the diminution of heads, as well as of hats, be established, does it imply a diminution of the amount of brain, or only of the size of the cranium? F. F. TUCKETT

Frenchay, near Bristol, November 12

I BELIEVE that hatters' measurements of the head can only be accepted as mere records of the change of fashion, and that they are of little anthropological value. Thirty years ago close cropping of the hair was confined almost entirely to soldiers, grooms, and prisoners, and it was popularly considered a badge of servitude, or worse; but now, thanks perhaps to the Volunteer movement, and to the discontinuance of hair-cutting as a punishment in prisons, the military style of wearing the hair is almost universal among young men; hence smaller hats are required now than formerly. I find that long and short hair make a difference in the circumference of some heads of nearly half an inch. Again, our nightcap-wearing fathers and grandfathers were very much concerned about the temperature of their heads and ears, and they were accustomed to press their hats well down to keep them warm. Now they are worn much higher on the head, as a glance into any old print-shop window will show. Travelling-caps, and caps worn by boys, were formerly provided with lappets to cover the ears, but these peculiarities have long since disappeared, and caps of an undress military character, or felt hats, stuck on the *top* of the head, have taken their place. Mr. Hyde Clarke, in his letter in your last week's issue (p. 32), says that he has observed that the ears are lower down now than formerly, and he thinks this a proof of degeneracy of race; but the ears only appear lower because the hats are higher on the heads, and in any case it could be no proof of degeneracy, because the lower the ear the bigger the brain. But the chief reason for the falling off in the dimensions of hats in the present day is the accession to the hat-wearing community of a very large number of small-headed persons, such as clerks and shopmen, who formerly did not wear hats at all; and, on the other hand, the defection of a large-headed class, the clergy, who have given up tall hats and taken to the use of soft felt ones. The only way hatters' measurements could be made available for anthropological purposes would be to examine the statistics of one class, say the professional, who have always worn hats, and then allow for the change of fashion in the hair and the position of the hat at the present day. If it is really the case that the heads of the present generation are smaller than those of the last, we must look for the cause, not in tight-lacing, but in the diminished size or the deformity of the female pelvis, for it is this which is the gauge of the heads of the people. Male infants are longer, heavier, and have longer heads than females, and at the time of birth a greater destruction of males takes place in consequence. In Europe the proportion of infants born alive is 105 males to 100 females; but if we include the stillbirths, the proportion of the sexes is 150 males to 100 females, showing that there is a sad loss of some of the finest physical and probably mental products of our race by the mere mechanical

[1] A most remarkable head, 7¾ × 7¾.

difficulties at the time of birth. There can be no doubt that rickety conditions of town children, and the sedentary or persistent standing occupations of young girls in shops, &c., will tend to distort the pelvis, and thus act injuriously on the race by reducing both the physical and mental standard of their children. With regard to the progressive degeneracy of our population referred to by Mr. Hyde Clarke, I think something more definite than personal recollections is required to prove it. We all know how we are daily compelled to recognise the fallacy of our earlier recollections. If we go down to a country town or village, which we knew well a few years ago, we find the houses smaller, the streets narrower, and the whole place shrunken in its proportions, and it would be the same with the inhabitants also if they had stood still as the inanimate objects have done around them. There can be no doubt that our large towns are, as it were, the graves of the physique of our race, but it is not because town life is so very injurious, but because the feeble, the halt, and the blind gravitate towards them in search of work suitable to their capacities. So far from admitting the degeneracy of our population as a whole, I am satisfied that it is improving in physique, and is better now than at any former period of our history. The skill and care which saves the weak child to the community, gives health and strength to the strong, and the physique of the whole is raised to a higher level. It is difficult to find direct evidence of this improvement, but some statistics of the stature and weight of factory children (where we might expect degeneracy if anywhere), recorded in 1833 and in 1873, show that the children of the latter period were a whole year in advance of the former—children of ten or eleven years of age in 1873 being as tall and heavy as those of eleven and twelve fifty years previously. CHARLES ROBERTS

Bolton Row, Mayfair, November 11

IN a letter on the above subject in NATURE, vol. xxv. p. 8, Dr. W. B. Kesteven asks for information or opinions on the statement that English heads have diminished in size during the last twenty-five or thirty years. My own opinion is that this is really the case. On the cause of this diminution I am not at present prepared to give a decided opinion. In the course of some investigations on heredity, commenced many years ago, I discovered that in some instances the average size of the heads of the sons and daughters was less than the average of those of the two parents. In each case the former had arrived at maturity before the comparison was made, and in every instance the children had had the advantage of a much larger amount of intellectual training than the parents had enjoyed. This discovery, so contrary to all the generally accepted opinions, surprised me considerably, and caused me to make inquiries from one of the leading hat-makers in this city as to what had been his experience in the increase or diminution in the sizes of hats sold. The facts furnished to me fully confirm the statements made in Dr. Kesteven's letter, and as the hat manufacturer to whom I refer has been more than forty-five years in the business he has had ample opportunity for collecting trustworthy information. From an article subsequently published by him in a trade journal (*Umbrella and Portmanteau Trades Review*, July, 1880) I take the following: "There is another feature in connection with heads which is singular in this district, and that is the decrease in sizes. It used to be considered sufficient to make one to each dozen; we now make, on an average, three or four of these sizes, which we are now obliged to keep in stock to meet our requirements. I allude to such sizes as $6\frac{1}{2}$ and $6\frac{5}{8}$, which formerly were only necessary in boys' hats. This decrease in the size of heads has been going on for the last twenty-five years to my certain knowledge." In the letter appended to Dr. Kesteven's letter Prof. Flower asks the important question, "Does it [the statement] refer to any particular class of men, and does it refer to the same class of men?" In answer to this I have to say that the classes to which the figures in my quotation refer are, and have always been, much the same, namely, the upper and middle classes; and the individuals included in these two classes have had as much variety in their occupations as any large city, such as Manchester, can furnish. Another important question is also asked by Prof. Flower, namely, "May it (the decrease in the size of hat) not arise from some change of fashion, . . . such as hats being worn more on the top of the head than formerly." In some few instances this might possibly account for the difference, but in the majority of cases, and especially in those belonging to the dolichocephalic class of heads, it will be found that it requires quite as large a size of hat when worn more on the back part of the head as it does when worn on the top. The data already collected are probably not sufficient to base any settled opinion upon; but if more extended investigation should confirm the statements made above, it will then be a matter of some importance to us to endeavour to discover the cause of this diminution in the size of English heads. It will also be interesting to know if any such phenomenon has occurred in any other country.

Old Trafford, Manchester CHARLES H. BLACKLEY

SURELY Mr. Hyde Clarke's arguments in favour of the hatters, statements are somewhat defective. Even if the survival of human weaklings be granted, it by no means follows that a being with a weakly body must needs have a small head. Indeed the exact converse is usually accepted; for big-chested athletes are generally supposed to be the men in possession of the smaller heads, and persons of weakly constitution the possessors of the larger heads. A weakly condition of body and health is often associated with great mental activity. Besides, at birth, the conditions, if favourable for the survival of weaklings, are surely equally favourable for the strong and well-made; under ordinary circumstances then these latter individuals should show an increase in the size of the head. It cannot be imagined that the weaklings are surviving at the expense of the strong and hearty, such a case would be, as some one has said, a survival of the unfittest. It would be interesting if Mr. Hyde Clarke would tell us something more about the "old standard" in ears, when he observed ears begin to fall below this old standard, and how the old standard in ears is to be recognised. My business as an artist has caused me to particularly notice heads and faces for many years past, and from ten to thirty portraits (old and new) pass through my hands every week. My opinion, founded on this experience, entirely agrees with the statements made by some of the speakers at the meeting of the Anthropological Institute mentioned by Mr. Clarke. The alleged diminution in size of men's heads is I think due to a misinterpretation on the part of the hatters of the fact that the hair is worn much shorter now than formerly, and the hat is now worn more on the crown of the head than in the past generation. The brim of the hat brought close down over the brows and the long hair in men is a very marked feature in old portraits. W. G. SMITH

125, Grosvenor Road, Highbury, N.

I SHALL not enter into the question of the relative sizes of the heads of our generation and of that of our fathers or grandfathers, beyond stating my general agreement with the explanation suggested by Prof. Flower, viz. that we carry our hats perched on the top of our heads instead of bringing them down as they did over occiput and ears, and that many of us, myself included, wear what hair we have so short that brushes and combs become superfluities. But I must express my surprise at so eminent a reasoner and statistician as Dr. Hyde Clark giving his support to a notion that to every medical statist seems a transparent fallacy—that a reduced infant mortality implies a deterioration of the race. If the deaths of children were owing solely to exposure to the elements, there might be a survival of the fittest, and such was the case among the Highlanders in former days, as it is perhaps still among Red Indians and the like; but we know that disease does not strike or weed out the feeble ones, or the people of Liverpool and Manchester, among whom 60 to 70 per cent. die before attaining their fifth year, ought to be a more stalwart race than the Scandinavians, who lose only about 16. No! infant mortality in civilised (?) and urban populations is due to two great causes, zymotic diseases and parental neglect, including insanitary surroundings. Now scarlatina, diphtheria, &c., do not show any preference, but cut off healthy and weakly alike; and improper food, foul air, overcrowding, bad drainage, though they may kill the feebler outright, tend to deteriorate the survivors; the weak die, the strong are made weak; those who do not die of scrofula, or diarrhœa, or rickets in infancy grow up puny or consumptive—"Mox daturos progeniem vitiosorem." I maintain that just as each death registered represents two whole years of sickness, so each infant's life saved implies two who would have been feeble rendered healthy and valuable members of society. The opposite view would strike at the root of all sanitary reform.

76, Marquess Road, N. EDWARD F. WILLOUGHBY

MONOS ISLAND, TRINIDAD

THE following extract from the log of the R.Y.S. *Northumbria* has been sent us for publication by Dr. G. H. Kingsley; it is dated February 28, 1881:—

"An almost perfectly land-locked harbour is formed by Monos itself and the neighbouring islands; on the Monos side indented with little bays, each one with its pretty white cottage, sparkling in the shade of clumps of coco-palms, with a silk cotton tree here and there, the latter looking as if they were trying to grow themselves into boards to save the sawyer trouble. The general tone of the vegetation just now is rather dull and New Zealand-ish, but the rocks along shore are covered with an infinity of bright flowers and shrubs, slender-shaped aloes bearing golden blossoms on their candelabra-like branchlets; wild pines with pink bracts and bright yellow petals, with sweet-scented orchids dangling anywhere and everywhere.

"*February* 29.—From Morrison's Bay in the hot level morning sun (most punishing and dangerous of all are the point-blank darts of Apollo), fairly into the Bocca Mono, upon the mysterious 'Guacharo,' which is here called 'watchelo.' The only cave containing them accessible at present was a low-browed one at the base of the cliff, into which an occasional roller sweeps ever and again in a most unpleasant manner, lighting up the black interior with flashes of foam, which augurs badly for the safety of our delicate pine gig. On this it was thought better to fall back on native talent, fishing close by in an island boat formed as to its lower parts of a 'dug-out' from the solid tree, and as to its upper of two planks nailed on to heighten the free board. A tituppy, ticklish kind of a craft to the inexperienced, crank in the extreme, but with a huge reputation for seaworthiness when properly handled. The negro proprietor had his head tied up in a dirty clout, in consequence of a difference of opinion with another 'cullud gebblum,' who had revenged his broken nose by literally 'mashing him jaw with rock-stone.' Though mumbly in speech, he was civil and accommodating, and taking Morrison and L. on board his dancing walnut-shell, he backed into the cave on the back of an accommodating wave. The cave was not deep enough to prevent the proceedings of those within being seen and heard by those without, and soon dismal yells, followed by smoky and smothered explosions, showed that hints were being given to the 'watchelo' to show themselves to their visitors. Another shot, followed by a jubilant shout, told us that one at least had shown himself once too often, and the party emerged blinking into the sunlight with their prey. The second entry was like the first: the interior commonplace and cavey, the interesting thing, of course, the 'watchelos,' fluttering about and perching on the more prominent projections. It is a remarkably handsome, upstanding, and even graceful bird, long-tailed, brown-feathered, with white diamond markings, just the colour of the quartz crystals in the reddish-brown rock on which it stood—a capital instance of preservative colouring, or the effect of surrounding colour. Altogether the 'watchelo' looks very much like a cross between the long-tailed cuckoo and a fair sized hawk; though the thighs are quite bare of feathers. We have been told all that is known about these queer fruit-eating Fissirostres—still there is much that is not known; for example, where they spend the night in collecting the fruit which contains the hard bristly seeds found in the stomachs of the adults and the young, and which, developing their nestlings into mere masses of fat, render them, as charming Mrs. Morrison says, '*si bon à manger*.' Mr. Morrison says that they feed on the '*Tierra firma*,' or mainland, but even he knoweth not on what.

"Having finished thus successfully our chase of the frugivorous goatsucker, we turned our attention and boat's head to another cave on the other side of the Bocca, in which dwelt an equally eccentric and out-of-the-way animal, the 'piscivorous bat.' These queer creatures, possibly in imitation of their opposite neighbours, have relinquished their supposed natural food, and have betaken themselves to catching fish at night in a manner which is not very clearly made out. Either they scoop them off the surface of the water by means of the membrane extended between their hind legs, or they catch them with their exceedingly sharp and curiously arranged claws. They dwell in a cave much more lowly and commonplace than their neighbours the 'watchelos,' and as they declined to answer the invitation sent to them by a shot into its interior, some of the party jumped overboard, mid-leg into the water, and proceeded with shouts and yells to drive them out into the glaring sunlight. Out they came in scores, these odd members of the Fishmongers' Company, flickering and fluttering in the slanting morning rays that shone through their diaphanous wing membranes and almost translucent chestnut-coloured bodies. Gnomes, Fays, Fanfullas, Flibbertigibbets, any queer, fantastic thing you have ever fancied or dreamt about, were not half so fantastic as these! Strange, and not without weird beauty to the eye. But to the nose! Fairylike in form and fluttering as they might be, the simple truth is they stank like Fitchets! 'Ruddy Miss Prue with golden hair,' in her wildest rompings, was nothing to them, and the scent produced in the hardest and strongest 'illiad' mariner a fervent desire to heave up his immortal soul. Possibly in revenge for this, the hardy one went for them with a boat-stretcher with such enthusiasm that shortly a hollow sound was heard, and another mariner, no longer enthusiastic, was observed hanging his head over the gunwale of the boat, with the blood trickling down his innocent nose from as pretty a scalp-wound as ever delighted a savage. However, but little harm was done, and we collected our wounded and slain, many of which had meanwhile sunk to the bottom, and wended our way back to the *Northumbria*.

"We visited the Bocca again in a late twilight, if there be such between the tropics, to study the mode of fishing of these most mysterious bats; but it was too dark to make anything out with certainty, though the queer scooping 'swish' supposed to be produced by their skimming the surface of the water with their posterior membranes, was distinct enough. What was even more distinct was, not to put too fine a point upon it, the stink; even right out in the open Bocca and at some distance from the cave, we were aware of the neighbourhood of individuals by the heavy rank smell floated towards us in the hot evening breeze.

"It is not the slightest use the 'parlour naturalists,' who study birds in glass cases and fishes in bottles, saying that this bat, from its 'dentition,' 'tripetition,' or any other of its 'itions,' must be frugivorous or insectivorous. The simple fact is that it is neither. When you find an individual of showy exterior, but slightly imperfect manners, with his pockets full of watches with the swivels broken off, you are justified in classing him, without the slightest reference to his 'dentition,' as a specimen of the 'swell mob—Homo watch-priggious'; and I maintain that when you find the stomach of a bat—the only pocket he possesses, not being a marsupial—stuffed with the scales and bones of fishes, you are fairly entitled to put him down as 'ichthyophagous' by all the rules of common sense. Our queer friend the 'watchelo,' with his deeply-cleft bill and outstanding bristles, *ought* to be a moth-catching goatsucker; but unless he swallows seeds for ballast he certainly lives on the fruits which contained them. It is the old story: directly we find what we call 'Nature' doing a thing perfectly well in one way, we immediately find her doing it equally well in another and directly opposite one. If she finds a bird with a bill perfectly formed for the catching of moths, she at once shows that it will do equally well for picking fruits off the bushes on dark nights; and if a bat can take the smallest midge in the twilight with unerring accuracy, she turns him without alteration into as good a fisher as the very otter himself.

"I am sorry to say that the 'fish booming and drumming,' described by Charles Kingsley, was not to be heard. Either we were there at the wrong season, or the fish had been driven away by the use of dynamite. From all I heard, the sound was identical with that produced by the drum-fish so common in the Indian river of Hinda."

ROBERT MALLET, F.R.S.

THIS eminent engineer, whose researches on earthquakes are so well known to scientific men, died on the 5th inst. at the age of seventy-one. During his very active career he accomplished a vast amount of work, of which his "Earthquake Catalogue" and other published books and memoirs form the best monument.

Robert Mallet was born in Dublin on June 3, 1810. He was descended from the representative of a Devonshire family who had settled in Ireland, his father being the owner and manager of an engineer's factory. During childhood Robert Mallet appeared to be of weakly constitution, but he grew up to be a man with great powers of endurance. His taste for science was exhibited at a very early period, and before he had reached the age of twelve years he had established a laboratory in his father's house, where he delighted in performing chemical experiments. After being taught in a private school in Dublin, and making a tour on the Continent, he entered as a student at Trinity College, Dublin, and in 1830 completed his studies there by taking his M.A. degree. In 1831 he made an extended tour on the Continent, and, upon his return, married, and entered into partnership with his father. From this time forth he was busily engaged in various engineering projects both in the capital and in various parts of Ireland. Private study and research were, however, by no means neglected during these busy times, and in the very year of his commencing business we find him publishing his first paper on the motion of glaciers. He had before this time been elected a member of the Royal Irish Academy. In 1839 Mallet was elected a member of the Institute of Civil Engineers, and in the same year made his important invention of "buckled plates," an invention which was not patented till 1852, the patent being prolonged in 1866. It was in 1846 that Mallet published his first paper on Earthquake Phenomena; this memoir, which appeared in the *Philosophical Magazine*, gave a simple explanation of the supposed "vorticose movements" during earthquakes, and two years later a paper in the *Transactions* of the Royal Irish Academy contained a full exposition of his views on the wave-movement in earthquakes, with which every one is now familiar. During subsequent years Mallet published in the British Association Reports his papers, which aimed at drawing up a complete catalogue of earthquakes, with various contributions to seismology and seismometry. In 1857 occurred the great earthquake in the Neapolitan territory, and in the following year Mr. Mallet was commissioned by the Royal Society to proceed to the district and to study its effects.

The results of his observations were published in two volumes in 1862. In 1858 the Earthquake Catalogue was completed by Robert Mallet with the aid of his son, now Prof. J. W. Mallet of Virginia. About this time we find Mallet engaged in experiments upon artillery, and in calling attention to a new gun which he had invented, but which never seems to have been of much practical utility. In 1872 Mallet laid before the Royal Society a memoir, to which he had evidently, during many years, devoted much time and labour; it was entitled "On Volcanic Energy, an Attempt to Develop its True Nature and Cosmical Relations."

Whatever differences of opinion may be entertained as to the truth of the theory which is there sought to be established, there can be none whatever as to the value of the experiments which constitute its basis, or of the important influence which it has exercised upon geological thought and speculation. This important memoir, which was published in the *Philosophical Transactions*, has been translated into German by Prof. von Lasaulx, who has added a valuable commentary to it.

During the later years of his life, Mallet, who had removed from Dublin to London, was afflicted with almost total blindness, but he nevertheless continued to make occasional contributions to his favourite branches of science. Altogether he was the author of more than seventy memoirs, besides separately published works. Mr. Mallet was elected a Fellow of the Royal Society in 1854 and of the Geological Society in 1859; in 1877 he was awarded the Wollaston medal of the latter society.

THE LAND OF THE MIDNIGHT SUN[1]

UNDER the above striking title we have an account of the Peninsula of Scandinavia and of the life of its people, based on a series of journeys made at different times, from 1871 to 1878, by Mr. Paul Du Chaillu. It is pleasant to meet with an author, already so well known for his travels in Equatorial Africa on new ground, and to find that his journeyings on virgin soil and among wild and savage races have not unfitted him for the study of the physical characteristics of an old country, and of the manners and customs of its inhabitants. The reader of these two handsome and well-illustrated volumes may form some notion of the extent of ground traversed during a five-years' sojourn, from the tracings of the author's routes on the map appended to the first volume. Not only was the country travelled over from north to south and from east to west, but the coast-line from Haparanda to the extreme north-eastern point of Norway, a distance of 3200 miles, was observed, the greater part of it both in winter and in summer, and over 3000 miles of fjords were sailed along. The illustrations are most frequently from photographs, but those representing Lapland winter scenes are the work of a Swedish artist.

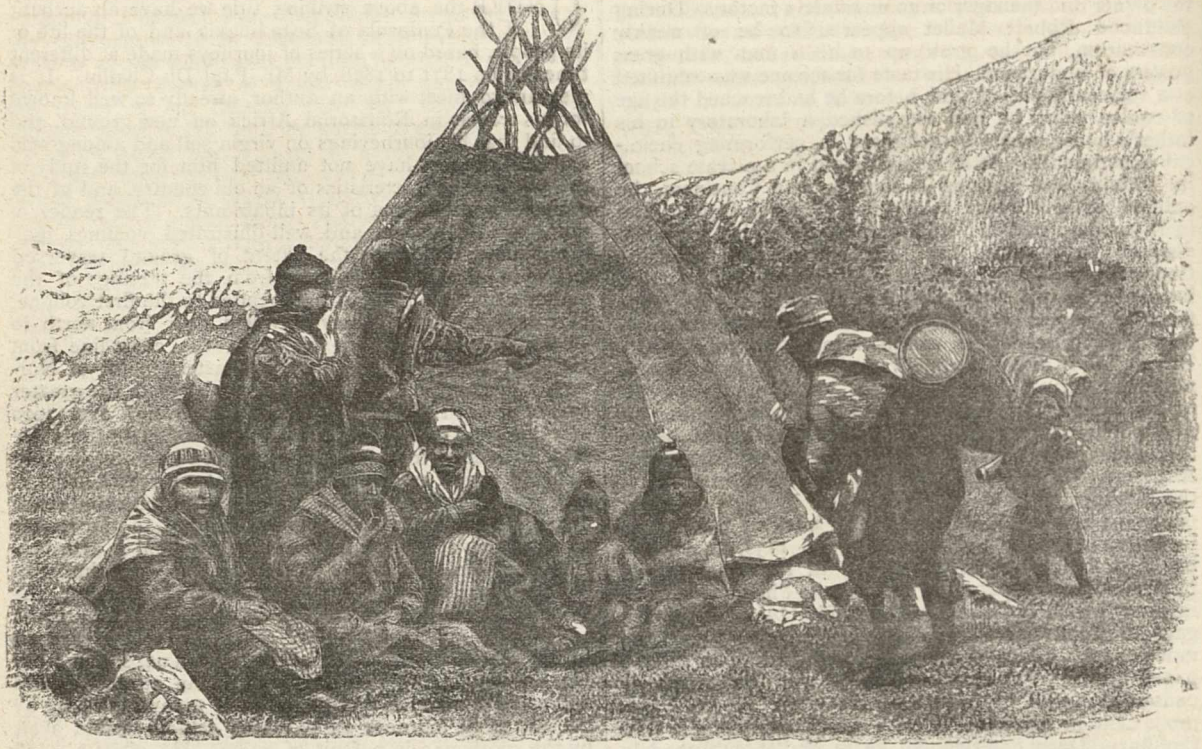
A great many pages of this work, while pleasant reading, will not afford much new information to the reader who may have already travelled in Sweden or Norway. The route from London to Göteborg, Stockholm with its beautiful suburbs, Upsala, Christiania, Bergen, the Dovrefeld, the splendid scenes of the Romsdal; these and a few more well-known routes and places are all within the compass of an ordinary summer's tour; but Du Chaillu has told of these all in an attractive and appreciative manner, and he treats of many such only by the way as he journeys on to places seldom visited even by the sportsman in pursuit of game. He gives a good deal of interesting information about the Laplanders. The Lapps are described as kind-hearted, dirty people. Their life during summer is a very hard one. They have to follow their reindeer day and night, lest the herds should wander. Coffee was their principal drink, mixed with the thick reindeer's milk. They were a fair-haired and fair-skinned people, with blue eyes, prominent cheek-bones, and the nose *retroussé*. The men were from four feet five to five feet and one-quarter inch in height, and three women measured four feet and one-quarter, four feet and three-quarters, and four feet six and three-quarters of an inch in height respectively. It was at the Lapp village of Jockmock that Prof. Baron von Düben, so well known and appreciated in this country for his writings, was met with. He was engaged in the study of the Lapps when Du Chaillu, fatigued and hungry, found himself entering the station. Longing to see a human

[1] "The Land of the Midnight Sun: Summer and Winter Journeys through Sweden, Norway, Lapland, and Northern Finland. With Descriptions of the Inner Life of the People, their Manners and Customs, their Primitive Antiquities, &c." By Paul B. Du Chaillu. In two volumes, with map and 235 illustrations. (London: John Murray, 1881.)

face, he saw the Professor approach ; and although he had never before met him, he addressed him correctly at once. He brought Du Chaillu to the neighbouring parsonage ; food was soon before him, and a hearty welcome. We incidentally learn that several of the illustrations of Lapland which accompany this narrative are from original photographs taken by the talented wife of the Baron.

A Laplander's encampment would not seem to be a savoury place. A Lapp tent at its base is scarcely more than eight feet in diameter; it is very portable. The frame is composed of poles fitting into each other, easily put together, and so strong and well knit that they can resist the pressure of the heaviest storm. A cross-pole high up sustains an iron chain, at the end of which is a hook to hold kettles. Over the frame is drawn a cloth of coarse wool called "vadmal," made by the Laplanders, who never use skins ; this is composed of two pieces, and is made fast by strings and pins, and well secured. A small door of canvas is suspended over the entrance. Such a tent will last more than twenty years. Some idea of its form can be obtained from the accompanying illustration. Such tents, when removed in winter, are drawn by reindeer in sledges, but in summer they have to be carried on the backs of the reindeer. Hence the moving of a Lapp encampment in summer is a very much more difficult undertaking than in winter.

The chapter on the vegetation within the Arctic circle is disappointingly short. There for some weeks there is little interruption to vegetation. What in these countries of midnight darkness can only be attempted by the aid of a brilliant electric light, is in those countries of midnight light brought about in a less artificial way. Rye planted at the middle of June was seven or eight feet high early in August, growing after first germination at the rate of three inches a day. The barley at Niavi was ready for the harvest six or seven weeks after being sown. Agricultural schools were met with all through Norway and



A Lapp Tent.

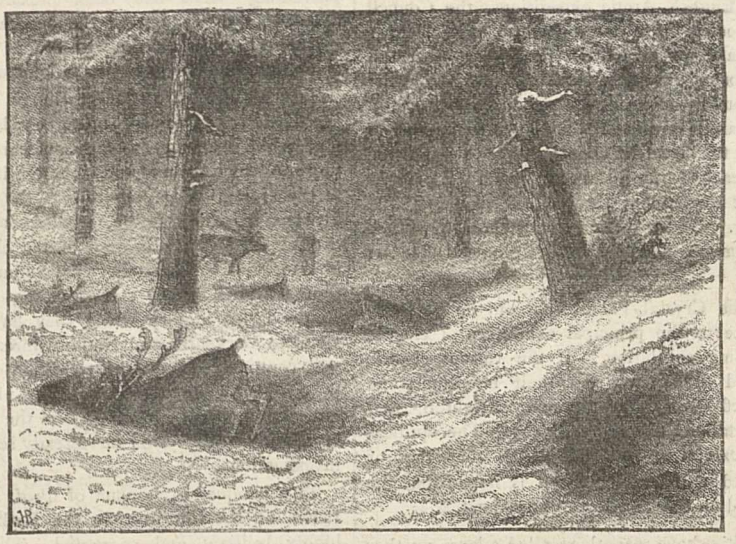
Sweden. These schools seem already to have been of enormous value to the country, raising the standard of agriculture and keeping the students up with the march of progress.

A second visit to Lapland—this time in midwinter—is described in Volume II. Entering a forest after a long drive, the author found himself suddenly in the midst of a number of holes several feet deep, dug by reindeer. Several thousand reindeer had evidently been at work. The snow was not very deep—not over four feet. Under that cover was buried the moss of which the reindeer is so fond. All except the younger ones were busy digging, first with one fore-foot, then with the other, the holes gradually becoming larger and larger, and the bodies of the animals more and more hidden. They would never stop excavating till they had reached the moss. Wherever one turned their eyes the reindeer were doing the same work, for they were evidently hungry. Their number seemed countless. Some had dug these holes so deep that nothing save the tails of the reindeer could be seen swaying to and fro outside of them.

The following extract, referring to the tame reindeer will be read with interest :—

"Late in April and early in May is the reindeers calving season ; the period of gestation is thirty-three weeks : the little ones are either carried or put into a sleigh. When the reindeer cows call for their young they utter a peculiar grunt, which is answered by the calves. Many of the animals had already dropped their horns. The large ones resembled those of stags, but they are smaller : the reindeer is clumsier than the deer, with stouter limbs, shorter head, and a larger and wider muzzle, more like that of a cow ; the hoofs are broader and much larger. The hair is grey, very coarse and thick, especially in winter, and sometimes two inches in length ; the colour is much darker on the back, and almost white under the belly ; the young are lighter-lined than the adults. The colour often varies considerably

among different herds, and frequently by this the ownership can be known. The reindeer are never housed, for they like cold weather and snow. Food is never given them, and they will not touch the moss that has been gathered, unless brought up to do so. They often will not even raise their heads as you approach them, and remain quiet when the Lapps pitch their tents, as we have seen. Some years prove unfavourable to their increase, on account of the amount of snow, which prevents them from digging for food; the herd then becomes weak and emaciated, and many die. The spring is also a bad time for them; the snow melts during the day, and a thick crust forms at night, so that their feet break through, causing lameness and disease. The horns of the males, which often weigh forty pounds, attain the full size at the age of five or six years, those of the cow at about four years. The time of dropping the horns in a herd varies from March to May; in the adult animal they attain their full size in September or at the beginning of October. After the age of eight years the branches gradually drop off. The shoulder-blades appear a little high, occasioning a slight hump or protuberance. Without the reindeer the Laplander could not exist in those northern regions: it is his horse, his beast of burden; it affords him food, clothing, shoes, and gloves. Domestic reindeer are a curious admixture of wildness and tameness. In some respects they are greatly superior to other cattle; in a herd they are very easily managed; they usually keep close together, and in the winter season remain where they have been left to feed. When on the march, with the help of dogs, they go in a solid mass, and a herd does not scatter unless wolves are after them; but in summer they often wander a long distance when left by themselves, as is often the case. When harnessed they become uneasy and distrustful, and great caution has to be taken not to startle them. Often trained reindeer, like horses, become refractory or vicious, and very difficult to manage, and then the Lapp shows his skill. The speed of the reindeer varies very much according to the time of the year, October, November, and December being the months in which they are fleetest, as then they are fresh from their summer pasture; the cold weather strengthens them, and they are not exhausted from digging the snow, not yet very deep, to procure their food. The rapidity of their gait depends much on the state of the surface. If this is well packed or crusted, and if



Reindeer digging in the snow.

previous furrows have been made, they go very fast. Much depends too upon the distance, and whether the country is hilly or not, with a long range of slopes. On the rivers, over well-packed snow and a good track, the animals can go twelve or fifteen miles the first hour, and down a long mountain-slope twenty miles, and even more. They can travel five or six hours without stopping, the first hour rapidly, the second more slowly, and towards the fifth or sixth quite slowly, for by that time they require rest and food. Early in the winter, when they are in good condition, one can travel with a swift reindeer one hundred and fifty miles in a day, where the country is not very hilly and the way good, and easily enough one hundred miles. The colder the weather the greater is the speed: seventy or eighty miles is a good average, but they were slow at the season of which I write."

The chapter on the Lofoden Islands gives a great deal of information about the extensive fisheries of cod, in which over 700 fishing-boats are employed.

We had marked several other extracts relating to the domestic life of the Norwegian peasants in the high north, and to the interesting people of Dalecarlia; but enough has been given to show our readers that, while the narrative of these travels is free from the exciting scenes witnessed in Gorilla Land, it is not wanting in much that will be read with pleasure and remembered with profit.

NOTES

We take the following from the *Times*:—The medals in the gift of the Royal Society for the present year have been awarded by the Council as follows:—The Copley medal to Prof. Karl Adolph Wurtz of Paris, For. Mem. R.S., for his discovery of the organic ammonias, the glycols, and numerous other investigations which have exercised considerable influence on the progress of chemistry; the Davey medal to Prof. Adolf Baeyer of Munich for his synthesis of indigo; a Royal medal to Mr. Francis Maitland Balfour, F.R.S., of Cambridge, for his numerous and important contributions to animal morphology, and more especially for his investigations respecting the origin of the uro-genital organs and the cerebro-spinal nerves of the vertebrata, and for his work on the development of the elasmobranch fishes; a Royal medal to the Rev. John Hewitt Jellett of

Dublin for his various mathematical and physical papers, more especially for his researches in chemical optics and his invention of the new and delicate analyser by which they were carried out.

THE Bakerian Lecture at the Royal Society will be given next Thursday by Prof. Tyndall, on the "Action of Free Molecules on Radiant Heat, and its Conversion thereby into Sound."

THE election to the Linacre Professorship of Physiology in Oxford University will take place on the 25th inst.

M. PAUL BERT, the eminent Professor of Physiology at the Sorbonne, has been appointed French Minister of Public Instruction by M. Gambetta. Two hours only after his nomination had been signed by the President of the Republic he read before the Academy of Sciences an elaborate paper on "Chloroform and other Anæsthetics." This may be considered as a token of the interest that the new French Government mean to take in scientific matters. M. Cochéry, of the Post and Telegraph Department, is the only member of the old Cabinet who has taken office in the new one.

IN view of the great and kindly interest the United States have always taken in English Arctic expeditions, the Council of the Geographical Society have determined to urge H.M.'s Government to undertake an expedition next year in search of the *Jeannette*. It being obviously more easy for the Americans than for this country to send an expedition by sea on the side of Behring Strait, it is thought that it would be best to despatch a party along the Great Mackenzie River to search the coast-line that can be reached from it, and that such work might most advantageously be carried out through the agency of the Hudson Bay Company. Under these circumstances it will be necessary to open communications with the Colonial Office on the subject.

THE authorities of the Crystal Palace sent out on Saturday the allotments of space to the principal exhibitors of electric light at the forthcoming exhibition. If the various electric lighting companies and firms carry out their intentions, the palace will be lighted from the London, Brighton, and South Coast Railway Stations to the north end. The applications from intending exhibitors in other classes than that of lighting will be attended to this week, and it is hoped that the allotments will be issued on Saturday or Monday next.

THE Chinese authorities have recently recalled a large number of students from Hartford College, U.S., under somewhat peculiar circumstances. The youths had been specially selected in China to undergo a thorough course of instruction in various branches of knowledge in the United States. They were placed under the charge of Mr. Yung Wing, who at one time held a high diplomatic post at Washington, and whose knowledge of the language and country led to his selection for this purpose. A large building for their accommodation was purchased by the Chinese Government near Hartford, and every thing seemed to be going on well. Much surprise was therefore expressed when, long before the termination of their course of study, the young men were suddenly recalled, and by this time they have all reached China. It appears that a high official of the Government, on his passage through America to Europe, examined the institution where his young countrymen were being trained. He was alarmed to find they were being rapidly Americanised, and that some had gone so far as to part with their pigtails, and dress in foreign garments. He thought also that he noticed a growth of ideas in their minds which, however congenial to a republic, were out of the question in China; that the youths' minds would gradually be alienated from their native country, and that the impressions which they were imbibing might lead to trouble and disaffection in China. He accordingly wrote to Peking, recommending the recall of the mission. His views were immediately adopted; but it is hoped that the students will not lose much, as they are about to be sent to European countries, to which the same objections do not exist as to the United States. The incident shows very clearly that, however anxious the Chinese may be for the science and knowledge of the west, they want none of its political doctrines.

ON Tuesday a deputation, consisting among others of Lord Harberton, Sir Antonio Brady, the Hon. Rollo Russell, Capt. Galton, C.B., Col. Festing, R E., Prof. Chandler Roberts, F.R.S., Mr. Ernest Hart, Mr. George Shaw, and Mr. W. R. E. Coles, waited upon the Lord Mayor at the Mansion House, and asked him to open the exhibition and trials of smoke-preventing appliances now about to be held at South Kensington. The earnest effort, they said, of the Committee had been to encourage general improvement in the methods and appliances by which heat was obtained, and thus to secure the advantages of greater economy of fuel and lessened smoke. That effort had been so far successful that a very considerable number of economical and effective grates, stoves, and furnaces had been brought forward, and many improved methods of firing and other smoke-preventing means had been introduced. The Lord Mayor acceded to the request of the deputation, and the Committee promised to communicate with him as to the day of opening the exhibition, which will probably be about the end of the month.

THE English Royal Commission on Technical Education have, the *Times* correspondent states, been busy in Paris in visiting the higher, the secondary, and the primary schools, and particularly those in which manual and technical instruction is provided. It is their intention to inquire into the state of instruction in the districts in which cotton, woollen, and silk manufactures are carried on, and also in some of the chief pottery manufactories abroad, and to ascertain what resources are available for the same purpose in the corresponding manufacturing districts in England—an inquiry which they have already commenced by visits to Yorkshire and Staffordshire. During the winter they will continue to study the condition of technical instruction in the English manufacturing districts, and in the spring it is their intention to visit Germany, where the chairman, Mr. Samuelson, on a late visit to Berlin, has already set on foot some preliminary investigations.

THE banquet in honour of Prof. Virchow is to be held at Berlin on the 19th inst., in the Rathhaus, and promises to be highly brilliant and successful. The sum of 30,000 marks, originally proposed to be raised for the memorial, appears likely to be greatly exceeded. This memorial will consist of a marble bust of the Professor, and will be placed in the hall of the Pathological Institute on the above-mentioned day, in commemoration of his twenty-five years' labours as a teacher of medical science.

THE *Morning Post* states that, among other experiments now being conducted by a chemist at the Royal Short-horn Dairy at Dytchleys, Essex, the powers of carferal, already well known as a filtering medium in the removal of ammonia from sewage, are being investigated. It has been found that all ammonia is taken up by the carferal, as is indicated by testing the fluid after it has passed through it, and the resultant is a material valuable in breaking up and manuring heavy land.

THE Nürnberg Natural History Society recently made an excursion to Berg near Neumarkt (Upper Palatinate), and upon this occasion a large number of Teleosaurus bones (vertebræ and others) were found at the boundary between brown Jurassic and Lias. Besides these fine specimens of Belemnites, Ammonites, and Terebratulæ were found. The bones in question are of course completely petrified; the impressions of the scaly hide on the back are well preserved. All the objects found are now deposited in the Society's collection.

THE Pesth correspondent of the *Daily News*, telegraphing on the night of the 9th inst., states that Agram has again experienced a violent shock of earthquake. Shocks of earthquake, sufficiently severe to occasion considerable alarm, were felt at Château d'Oex, Canton Vaud, on Wednesday night and Thursday morning last week. The second of the shocks, which appear to have been altogether local, was accompanied by loud rumblings. There were three slight earthquake shocks in various parts of Switzerland last month, all local, and affecting only a very limited area. The first occurred at Monthey, in the Valais, on October 14; the second at Berne, on October 17; and the third at Zürich, on October 27. A violent earthquake was felt on October 27 at 10.30 p.m. in the northern part of the canton of Zürich. On the same day at 4.30 a.m. a slight shock was felt at Cilli and several other places in Styria.

THE death is announced of Dr. Eduard Simon Heine, Professor at Halle University, an eminent mathematician. He died at Halle on October 24 last.

WE regret to announce the death of Dr. Carl Arendts of Munich, a well-known geographer, and founder of the Geographical Society of that city. Dr. Arendts died at Possenhofen on October 11, aged sixty-seven years. He was the author of numerous standard works, and editor of the excellent *Deutsche Rundschau für Geographie und Statistik*, which has just commenced its fourth volume.

OUR readers are aware that Dr. Kobelt visited North Africa and Spain last spring, by the assistance of the Rüppell Institution of Frankfort, in order to investigate the molluscous fauna of the two countries, as well as to gather evidence bearing upon the question how far the land-connection between the two countries extended in bygone ages. He now reports that it may safely be assumed that the connection was not confined to the Straits of Gibraltar, but extended at least as far as the meridian of Oran and Cartagena. Dr. Kobelt will publish the detailed results of his investigations in the malacozoological journal, of which he is the editor.

THE Gotha Natural History Society will arrange an exhibition of natural history and geographical objects from November 20 to December 20. The Duke of Saxe-Coburg Gotha has lent the Society a suite of rooms in the Castle for this purpose.

A TUNNEL through the Col de Somport, near the Pic du Midi, in the Pyrenees, is the next large engineering work which will be undertaken in France. It will shorten the railway distance between Paris and Madrid by 100 kilomètres, the Spanish line passing by way of Ayerbe, Caldearenas, Jaca, and Confranc, leading on the French side into the Gave de Aspe valley and Oléron.

WE have before us the prospectus of a new entomological monthly journal, to be styled the *Wiener entomologische Zeitung*, of which the first part is to appear at the beginning of 1882, each part to consist of a sheet and a half of text, large 8vo. The editors are Ludwig Ganglbauer, Dr. Franz Löw, Prof. Josef Mik, Edmund Reitter, and Fritz Wachtl, all of Vienna, and it will be published by Alfred Hölder of that city. With so strong a staff it should prove a success. In some respects it is intended as a successor to the for many years defunct *Wiener entomologisches Monatsschrift*.

MR. LEO LESQUEREUX, we learn from the Harvard Library *Bulletin*, No. 3, has made a preliminary Report on the Plants of the Dakota Group collected by Mr. Sternberg for the Museum of Comparative Zoology (to be published in the *Museum Bulletin*). He says the collection is valuable in regard to the data it furnishes in confirmation of, or contradiction to, some of the general conclusions derived from the examination of the materials formerly described from this peculiar Cretaceous flora. For example, the disconnection of the flora of the Dakota group from that of the older zones—those of the Jurassic times—does not appear now so positive as formerly, or as it was indicated in the Cretaceous Flora Reports of Hayden's Survey. On the other side, the disconnection of the Cretaceous flora from that of the Lower Tertiary appears now still more evident, as the new species do not indicate any affinity with the plants of the Laramie group, which is positively Eocene by its types. Mr. Lesquereux is inclined to explain the distribution of the fossil plants of this group over small areas, as showing that the trees apparently grew around small hillocks or dry surfaces of land disseminated in wide lagoons. If derived from distant shores the leaves should be more or less mixed, while a comparison of the different localities shows that the fossil leaves were derived from trees grown in the places where they are now found fossil.

THE complete annihilation of Elm, according to the Geneva correspondent of the *Daily News*, appears to be now only a question of time and a little bad weather. At the request of the Government, Prof. Heim has just made another inspection of the Tschingel. He finds the work of disintegration proceeding much more rapidly than he expected. The entire summit of the Risikopp, the peak nearest Elm, is in movement, and its fall is not likely to be long delayed. It may miss the village, but the chance is very remote. The inhabitants remain meanwhile in their houses, which they have not to quit until the sentinels who are watching on the mountain warn them that danger is imminent.

IN Japan, according to a recent return, there are in all 159 hospitals where the patients are treated on the principles of western medicine. Thirty-five of these are private institutions in the sense that they receive no government aid, while twelve are naval or military hospitals. The remainder are scattered throughout the country; but they are said to be, in the remote districts, in a very crude condition. Vaccination, which is compulsory, is performed *gratis* everywhere. A law prohibiting the practice of physic or surgery by any except persons holding certificates of permission has recently been passed. A large school of medicine, with German professors, has been established for the past eight or ten years in the capital.

AT Nagy-Look (Hungary) the remains of a two-wheeled Roman car were lately discovered, with the skeletons of two horses attached to it. The objects are believed to date from the third century, and the place where they were found to have been the tomb of the owner. All the parts of the carriage are of most exquisite workmanship.

THE Russian Government intend to construct a canal between the Dnjepr and Düna Rivers, the Orschitsa, a tributary of the Dnjepr, to form a part of the canal. The distance by water between the Euxine and the Baltic would thus be shortened by 415 versts.

A ROUMANIAN mechanic, Traiano Feodoresen, recently submitted to the Chamber at Bucharest a project of a submarine vessel, and after examination of this by a committee the Government was authorised to meet the expense of construction. The vessel is to be capable of moving under water, at a depth of 30 metres, for twelve hours, without requiring renewal of air. Steam is the motor, and the speed is quicker than that of sailing vessels. The vessel is simply sunk by opening certain valves, but return to the surface requires more complex operations. An electric light will render objects distinguishable at 30 or 40 metres. For renewal of air it is not necessary that the vessel rise to the surface; an apparatus can be sent up, which, by actuating a pump, forces air into suitable receivers.

IT is often supposed that the reality of alchemy, the transformation of the base into the noble metals, was generally accepted by orientals. But, according to Herr E. Wiedemann (*Ann. der Phys.* No. 10), some of the most noted *savants* rejected the idea. In his Prolegomenon, Ibn Khaldûn maintains that the transformation of metals is impossible, the philosopher's stone cannot exist, and the study of alchemy is ruinous. His own views, however, interest us less than his citation of Avicenna and his school as opponents of alchemy. While Abn Nasir al Farâbi, an older philosopher, held that all metals belong to the same species, and differed only in accidents, so that a transformation of these into each other was possible, Avicenna maintained that the metals differed in species, and that their specific differences, ordained by God, were therefore not alterable by chemical operations. A noted alchemist, Togair, contended, against this, that the task of alchemy was not to impart these differences to metals, but only to alter the latter so that they might be enabled to acquire them; the means to this being the elixir. A great predecessor of Avicenna, Al Kindi, also appears to have opposed alchemy.

THE additions to the Zoological Society's Gardens during the past week include two Vulpine Phalangers (*Phalangista vulpina*), a Rufous Rat Kangaroo (*Hypsiprymnus rufescens*) from Australia, presented by Mr. F. J. Horniman, F.Z.S.; a Ring-necked Parrakeet (*Palæornis torquata*) from India, presented by the Countess Dowager of Lonsdale; two Long-eared Owls (*Asio otus*), British, presented by the Rev. J. A. Wix; two Grey Wagtails (*Motacilla sulphurea*), British, presented by Mr. Swaysland; a Dufresne's Amazon (*Chrysotis dufresniana*) from South-East Brazil, a Yellow-cheeked Amazon (*Chrysotis autumnalis*) from Honduras, an Orange-winged Amazon (*Chrysotis amazonica*) from South America, deposited; a Pluto Monkey (*Cercopithecus pluto*), a Sykes's Monkey (*Cercopithecus albogularis*) from West Africa, a Darwin's Rhea (*Rhea darwini*) from Patagonia, a Picazuro Pigeon (*Columba picazuro*) from South America, two Spotted Zenaida Doves (*Zenaida maculata*) from La Plata, two Dominican Gulls (*Larus dominicanus*) from Antarctic America, purchased.

EXPERIMENTS ON COLOUR[1]

IN a former paper with the above title (NATURE, vol. iii. p. 234) I described some combinations of absorbing media capable of transmitting the red and green, while stopping the other rays of the spectrum. In this way I obtained a *purely compound* yellow, made up of red and green, and free from homogeneous yellow light. In devising such combinations we have in the first place to seek an absorbing agent capable of removing the yellow of the spectrum, while allowing the red and green to pass. For this purpose I used an alkaline infusion of litmus, or solution of chloride of chromium, placed in a trough with parallel glass sides. In order to stop the blue rays we may avail ourselves of chromate of potassium. If a second trough be not objected to, it is best to use the bichromate, as exercising the most powerful absorption upon the upper end of the spectrum; but the bichromate cannot be mixed with litmus without destroying the desired action of the latter upon yellow. In this case we must content ourselves with the neutral chromate.

During the last year and a half I have resumed these experiments with the view, if possible, of finding solid media capable of the same effects, and so of dispensing with the somewhat troublesome troughs necessary for fluids. With this object we may employ films of gelatine or of collodion, spread upon glass and impregnated with various dyes, gelatine being chosen when the dye is soluble in water, and collodion when the dye is soluble in alcohol. Thus in the case of litmus a slightly warmed plate is coated with a hot and carefully filtered solution of gelatine, allowed to remain in a perfectly horizontal position until the gelatine is set, and then put aside to dry, by preference in a current of warm air. The films thus obtained are usually somewhat rough upon the surface, so that I have preferred to use two pieces cemented together, coated sides inwards, with Canada balsam. In conjunction with the litmus we may employ a silver-stained orange glass, and so isolate the red and green rays. For the orange glass Mr. C. Horner has substituted a film of collodion stained with aurine. Samples possibly vary; but that which I have used, though extremely opaque to the blue-green rays, and therefore so far very suitable for the purpose, allows a considerable quantity of the higher blue to pass. By spreading aurine upon a pale yellow glass, I obtained a very perfect absorption of the blue-green and higher rays. Plates prepared as above described answer the purpose very well; but I have found that in some cases the litmus in contact with the balsam becomes slowly reddened, the action creeping inwards from the edge. A dye, capable of replacing litmus, and free from this defect, is "soluble aniline blue," whose absorption, as I found rather unexpectedly, begins in the yellow and orange. Bichromate of potash and aniline blue may be mixed in the same solution, and there is no difficulty in so adjusting the proportions as to secure a good compound yellow. To obtain solid films gelatine must be used, as in the case of litmus, for the dye is not soluble in collodion. With aniline there is no difficulty from the Canada balsam, and two plates cemented together answer perfectly.

For systematic observations on compound colours nothing probably can be better than Maxwell's colour box in its original form; but it seemed to me that for the examination of certain special questions a more portable arrangement would be convenient. In an instrument of this class a full degree of brightness requires that the width of the eye-slit, placed where the spectrum is formed, should not contract the aperture of the eye, *i.e.* should not be less than about one-fifth of an inch; and although the maximum of brightness is not necessary, considerations of this kind largely influence the design. If we regard the width of the eye-slit as given, a certain length of spectrum is necessary in order to attain the desired standard in respect of purity of colour; so that what we have to aim at is a sufficient linear extension of the spectrum. A suitable compromise can then be made between the claims of brightness and purity.

The necessary length of spectrum can be obtained by increasing either the angular dispersion of the prisms or the focal length of the lens by which the image is formed. If portability be no object, the latter is the preferable method, and the focal length may well be increased up to five or six feet; in this way we may obtain a field of view of given purity of colour and of maximum brightness, at the expense only of its angular extent. If, however, we desire an instrument which can be moved from one place to another without losing its adjustment, the focal length of the lenses must be kept down, and then a large prismatic dispersion is the only alternative.

Increased dispersion can of course be obtained by multiplication of prisms; but for the purpose in view, high resolving power is not wanted, and our object may be attained with a comparatively small total thickness of glass, either by the use of higher angles than usual, or by giving the light a more nearly grazing emergence. The latter was the course adopted in designing the first instrument of which I have to speak. A pair of prisms of 60°, cut from an ordinary single $1\frac{1}{4} \times 1\frac{1}{4}$-inch prism along a plane bisecting at right angles its refracting edge, were arranged in the corner of a shallow box, so as to form what Thollon calls a *couple*. Considered as a simple, rigidly connected refractor, the pair of prisms are placed so as to give minimum deviation, but the incident and emergent light makes smaller angles with the final surface, than if each prism were adjusted separately for minimum deviation. The collimating and focussing lenses are common spectacle glasses of about $8''$ focus. The box is $12'' \times 12'' \times 3''$. Light entering at a slit on one of the sides of the box would be turned by the prisms through an angle rather greater than a right angle, and throw a pure spectrum upon another side of the box. This side is cut away, and provided with movable screens of cardboard, so that any part may be open or closed as desired. When the eye is applied to the first slit, the prisms are seen uniformly illuminated with colours whose composition depends upon the situation and width of the slits between the cardboard screens through which light is allowed to enter. In this way we may obtain a uniform field of view lighted with any combination of spectral colours. My object, however, was to obtain an instrument for making comparisons between the simple and compound yellow, and for this purpose an addition was necessary. This consisted

[1] Read before Section A of the British Association, September 2, 1881, by Lord Rayleigh, F.R.S.

of a very acute-angled prism held close to the dispersing prisms in such a position that its refracting edge was horizontal, dividing the field of view into two equal parts. The action of this prism is most easily understood by again supposing the light to enter at the eye-slit. Half of the light proceeds as before, forming ultimately a pure spectrum upon the side of the box. The upper half of the beam, however, is deflected by the acute-angled prism, and the corresponding spectrum is thrown upwards, so as to lie somewhat higher upon the side of the box. This part is also cut away, and provided with movable screens. By the principle of reversibility the consequence is that an eye placed at the first slit sees *two* uniform patches of colour, the lower formed as before by light from the lower set of slits, the upper, covering the acute-angled prism, by light from the upper set of slits. These colours are in close juxtaposition, and may be compared with ease and accuracy.

The great difficulty in this class of instruments is to devise any efficient and reasonably simple method of controlling the position and widths of the slits. In the present case I contented myself with strips of blackened cardboard cemented to the side of the box with sealing-wax, or soft wax, according to the degree of permanence of adjustment aimed at. One part of the field was illuminated with homogeneous yellow (about the line D) from a single slit. The other half was lighted with a mixture of full red and full green, and the observation consisted in adjusting the widths of the slits through which the red and green were admitted, until the mixture was a match with the simple yellow.

The first trials of this instrument in the spring of last year revealed an interesting peculiarity of colour vision, quite distinct from colour blindness. The red and green mixture which to my eyes and to those of most people matches perfectly the homogeneous yellow of the line D, appeared to my three brothers-in-law hopelessly too red, "almost as red as red sealing-wax." In order to suit their eyes the proportion of red had to be greatly diminished, until to normal sight the colour was a fair green with scarcely any approach to yellow at all. So far as could be made out at the time, the three abnormal observers agreed well among themselves, a fact which subsequent measurements have confirmed. It appeared afterwards that a fourth brother was normal as well as the three sisters.

These peculiarities were quite unexpected. After the fact had been proved, I remembered a dispute some years before as to the colour of a dichromatic liquid, which appeared to me green, while one of my brothers-in-law maintained that it was red; but the observation was not followed up, as it ought to have been, each of us, I suppose, regarding the other as inaccurate. After the establishment of the difference I determined to carry out a plan, which I had tried with success some years before (October 1877), for a colour-mixing arrangement depending on double refraction, by which I hoped to obtain an easily adjustable instrument suitable for testing the vision of a number of persons.

In my original experiments I used a 60° doubly refracting prism of quartz, which threw two spectra of the linear source upon the screen containing the eye slit. These oppositely polarised spectra partially overlapped, and by suitable placing of the prism could be made to furnish red and green light to the eye. By the rotation of a small Nicol held immediately behind the eye slit, the red or green could be isolated or mixed in any desired proportion. One advantage of this arrangement is that the two component lights come from the same slit, so that we are less dependent upon the uniformity of the light behind; but it is perhaps a greater merit that the adjustment of proportions is effected by simple rotation at the eye slit, allowing the observer to try the effect of small changes with ease and rapidity.

In the new instrument, which was completed during the autumn of last year, separate prisms were used to effect the dispersion and double refraction. For the sake of compactness, a direct vision prism by Browning, containing two flints and three crowns, was chosen, in conjunction with a small achromatic double image prism. At one end of a long narrow box, 24″ × 2″ × 2″, the light is admitted through a slit whose position and width can be adjusted by sliding its jaws along a divided scale. After travelling about 9¼″ it falls upon the double image prism mounted upon a small table so as to allow of rotation, and then after two more inches upon a collimating lens, by which the two beams are rendered parallel. Next comes the dispersing prism, and then the focussing lens, throwing pure spectra upon the other end of the box, which carries the eye slit. The distance between the two lenses is 3½″, and the entire length of the box is about 24″.

The eye slit is a fixture, and immediately behind it is the rotating Nicol, whose position is read by a pointer on a divided circle.

The parts of the spectrum from which the component lights are taken can be chosen over a sufficient range by use of the two adjustments already mentioned. By rotation of the table on which the double image prism is mounted, the separating power is altered, and one spectrum made to slide over the other, while by moving the entrance slit the spectra are shifted together without relative displacement.

It yet remains to describe the parts by which the comparison colour is exhibited. Between the double image prism and the collimating lens a small vertical reflector is mounted on a turn-table at an angle of about 45°. Its dimensions are such that it covers the lower half of the field of view only, leaving the upper half undisturbed, and its function is to reflect light coming from a lateral slit through the dispersing prism so as to throw a third spectrum upon the eye. The lateral slit is carried in a small draw tube projecting about 2″ from the side of the box, and the light proceeding from it is rendered nearly parallel before reflection by a lens of short focus. No adjustment is provided for the position or width of the lateral slit; all that is necessary in this respect being attainable by rotating the mirror and by varying the brightness of the light behind. As sources of light I have found Argand gas flames, surrounded by opal globes, to be suitable. The gas tap supplying the lateral flame is within reach of the observer, who has thus the means of adjusting the match both with respect to colour and with respect to brightness, without losing sight of the subjects of comparison. The zero of the divided circle corresponds approximately to the complete exclusion of green, but readings were always taken on both sides of it so as to make the results independent of this adjustment. The circle is divided into 100 parts, green being excluded at 0 and 50, and red at 25 and 75. Tenths of a division could be estimated pretty correctly, an accuracy of reading fully sufficient for the purpose, as the observations of even practised observers would vary two or three-tenths.

It is evident that the numbers obtained are dependent upon the quality of the light by which the principal slit is illuminated. In order to avoid errors in the comparison of different persons' vision arising from this source, it is advisable always to take simultaneous observations from some practised individual whose vision may be treated as a standard; but no evidence appeared of any variation in the quality of the gaslight. The special application of such instruments to the comparison of the qualities of various kinds of mixed light was alluded to at the end of my paper "On the Light from the Sky," &c. (*Phil. Mag.*, April, 1871).

I have obtained matches between simple and compound yellow from twenty-three male observers, principally students in the laboratory. Of these sixteen agree with myself within the limits of the errors of observation. The remaining seven include my three brothers-in-law, and two others, Mr. J. J. Thomson and Mr. Threlfall, whose vision in this respect agrees very nearly with theirs. The vision of the other two observers differs from mine in the opposite direction. In one case the difference, though apparently real, is small, but in the other (Mr. Hart), though there was some difficulty in getting a good observation, the difference is most decided. Among seven female observers whom I have tried, there is not one whose vision differs sensibly from my own.

Although the number examined is insufficient for statistical purposes, it is evident that the peculiarity is by no means rare, at least among men. As far as my experience has gone, it would seem too as if normal vision were not of the nature of an average, from which small deviations are more probable than larger ones; but this requires confirmation. In order to give a more precise idea of the amount of the difference in question, I have calculated from the laws of double refraction the relative quantities of red and green light required by Mr. F. M. Balfour and myself to match the same yellow light. If we call R and G the maximum brightnesses of the red and green light (as they would reach the eye if the Nicol were removed), and r, g the actual brightnesses (as modified by the analyser) necessary for the match, then for Mr. Balfour—

$$r/g = 1{\cdot}50\ (R/G),$$

while for myself—

$$r/g = 3{\cdot}13\ (R/G).$$

In other words, Mr. Balfour requires only half as much red as myself, in order to turn a given amount of green into yellow.

The corresponding numbers for the other four observers of this class would be substantially the same. On the other hand, Mr. Hart requires much *more* red than I do in order to convert a given green into yellow—in the ratio of about 2·6 : 1.

Except in the case of Mr. Hart, the colour vision of these observers is defective only in the sense that it differs from that of the majority. Their appreciation of small colour differences is as distinct as usual. In order to test this Mr. G. W. Balfour made a complete series of colour matches with revolving disks in the manner described by Maxwell and in my former paper. Six matches, of which only two are really independent, were observed, the consistency of the set being a measure of the accuracy of observation. The average error proved to be only double of that which I have found in my own observations, and rather less than that usually met with in the case of observers whose vision is normal.

In connection with what has been described above with respect to trichromic vision, it is interesting to notice that corresponding and perhaps larger differences are to be found in the vision of the so-called colour-blind. The double-refraction apparatus may conveniently be used in this investigation. With the pointer adjusted to 0 or 25, we have in the upper half of the field pure red or pure green respectively, and in the lower half pure yellow as usual. By suitable adjustment of the gas taps two observers of this class, Mr. T—— and Mr. B——, are able to obtain perfect matches both between red and yellow, and between green and yellow, but the proportions necessary are very different for the two observers. In Mr. T——'s red and yellow match, the red is to normal vision dazzlingly bright, and the yellow almost too dark to be recognised; while the green and yellow match, however extravagant as to hue, appears reasonable in respect of brightness. On the other hand, to Mr. B——'s eyes, the red of the spectrum does not look nearly so dark, and the equivalent red and yellow appear to the normal eye to be much more nearly upon a level. Although these great differences exist, there is no doubt that the vision of both observers is strictly dichromic, and that, apart from brightness, all the rays of the spectrum, from red to green, have the same effect upon their eyes.

If we wish to go beyond the fact that this vision is dichromic, and inquire whether the case is one of red blindness or of green blindness, we must be careful to consider whether the question itself has a definite meaning. If trichromic vision were always the same, and if a particular case of colour-blind vision differed from it merely by the absence of the red sensation, that vision would intelligibly be characterised as red-blind. There is reason to believe that such cases exist. In all probability the suppression of my own red sensation would lead me to make matches very nearly the same as Mr. T——'s; and in this sense he may fairly be called red-blind. But under the same circumstances my matches would be altogether rejected by Mr. B——; and the question may be asked, whether his case, being certainly not one of simple red-blindness, can be brought under the head of green-blindness. To this the sufficient answer is that if I became green-blind my matches would differ from those of Mr. B—— far more than if I was red-blind. The test of green-blindness would be the possibility of matches between colours which to normal eyes appear green and purple, or green and grey. Although a good deal has been said lately on this subject, I am not aware of a case in which accurate matches of this kind have been obtained from observers whose colour-vision is in other respects acute. If such cases exist, inquiry should be instituted, in order to see how far the matches would correspond to green-blindness of an otherwise normal eye.

We see, then, that there is dichromic vision which cannot accurately be described as affected with red-blindness, and still less as affected with green-blindness. The difference from normal vision, being not simply one of defect, cannot be defined by any single phrase. To obtain a complete knowledge of it quantitative observations over the whole spectrum, such as those carried out by Maxwell, are necessary. It is fortunate that these observations are easier to arrange for dichromic than for trichromic vision.

That I might be able to form an opinion upon the general acuteness of his colour-vision, Mr. T—— was good enough to observe a series of five colour matches between red, white, blue, green, and yellow, one being left out each time. The results are given in the accompanying table; those marked "calculated" being a consistent set derived by elimination from the two marked A and B. The good general agreement of the two sets of numbers is a proof that within its restricted range Mr. T——'s sense of colour is acute. The first observation in which a mixture of red and white is matched by a mixture of green and blue is the most characteristic.

	Red.	White.	Blue.	Green.	Yellow.	Dec. 2, 1880.
(1)	76·2	23·8	−23·3	−76·7	0	Observed
	77·4	22·6	−21	−79	0	Calculated
(2)	56·6	43·4	−52·3	0	−47·7	Observed
	56·2	43·8	−52·5	0	−47·5	Calculated
(3)	68·2	5·5	0	−100	26·3	Observed
	69·7	6·5	0	−100	23·8	Calculated
(4)	60·3	0	8	−100	31·7	Observed
	61·2	0	7·8	−100	31	Calculated
(5)	0	32·5	−43·5	67·5	−56·5	Observed
	0	32·3	−44·1	67·7	−55·9	Calculated
A	522	424	−511	35	−470	—
B	641	405	−470	−199	−377	—

In conclusion I will describe an apparatus by which it is possible to observe these colour-matches without rotating the disks. At the time of my first experiments, about ten years since, I was struck with the advantage which might ensue if it were possible to have the mixed colours in view during the time of actual adjustment, and I thought of a plan by which this object might be attained. The idea, which I carried out soon afterwards, was to spin an *image* of the disks instead of the disks themselves. An inverting prism was mounted in a tube which could be made to rotate. The axis of rotation is adjusted so as to point accurately to the centres of the disks mounted as usual. An eye applied to the prism sees the disks undisplaced as a whole, but inverted by reflection. As the tube rotates, the image of the disks rotates also, and with double angular velocity. When the speed is sufficient, the colours lying on any circle concentric with the disks are blended exactly as if the disks themselves revolved.

This apparatus is quite successful; but its real advantages of working at a smaller velocity, and of allowing adjustment while the rotation continues, are counterbalanced in practice by the inconvenience of having to look through a tube, and the uncertainty introduced by the possible disturbance of the match due to unequal illumination of the area occupied by the disks.

MAGNETIC DISTURBANCES, AURORAS, AND EARTH CURRENTS [1]

THE object of establishing a magnetic observatory is to determine at any instant the direction and magnitude of the earth's magnetic force. The direction of the magnetic force of the earth is the direction in which a small magnetic needle would point when it is freely suspended, so as to turn about an axis passing through its centre of gravity. But it is not easy to suspend a magnetic needle so as to turn freely and yet to be sure that the axis about which it turns passes accurately through the centre of gravity of the needle, and if it does not so pass, then on suspending the needle we have not only the magnetic force but also the gravitating force of the earth acting upon it to turn it about its axis, and the position which it takes up shows us the direction of these combined forces upon the magnetic needle.

This direction depends upon the mass of the needle, for to that its weight is due; it depends upon the form of the needle and the position of its centre of gravity with regard to the axis on which it is hung; it depends also on the magnetic properties of the substance, so that it is not easy to determine even the direction of the magnetic force by a plan which theoretically is so very simple. Instead of attempting to make the required determinations by such a method it is necessary that a steadier mode of suspension should be adopted, and that may be done as soon as it is discovered in what vertical plane the force of gravity,

[1] Lecture delivered at the Royal Institution on Friday evening, June 3, 1881, by Prof. W. Grylls Adams, F.R.S.

combined with the earth's magnetic force, will cause such a needle to rest. This is usually done by loading a steel needle at one end and then magnetising it with its poles so arranged that the extra weight of the heavier end shall balance the downward pull of the magnetic force on the other end. In this case the needle when magnetised will remain at rest in a horizontal direction, when suspended on a point on which it can turn freely in a horizontal plane.

A magnetic needle suspended in this way has been called a declination needle. Such a needle is employed in the mariner's compass, in our galvanometers for measuring currents of electricity, and in magnetic observatories for determining the declination or what is sometimes called the variation of the magnetic needle. This needle determines the position of the vertical plane in which lies the direction of the earth's magnetic force; this is called the plane of the magnetic meridian. The plane of the *magnetic* meridian is usually different from the vertical plane through the north and south poles, which is called the geographical meridian, and the angle between these two planes is the declination or variation of the magnetic needle.

If such a magnetic needle as I have just described be supported on horizontal knife edges instead of being supported on a point, the needle when magnetised may remain at rest balanced in a horizontal direction, one end being pulled downwards by the earth's vertical magnetic force, and the other by the force of gravity. Any change in the intensity of the vertical magnetic force of the earth will be shown by an up or down motion of the marked end of the needle. Such an instrument, called a balance magnetometer, is specially adapted for showing any changes in the vertical magnetic force of the earth, and the changes or disturbances of the earth's *vertical* magnetic force are determined by means of such a balance magnetometer. We have then our declination or variation needle to determine the vertical plane, called the magnetic meridian, and we have our balance magnetometer to determine any changes which may take place in the vertical magnetic force of the earth.

By the declination needle we can not only determine the plane of the magnetic needle, but by making the needle oscillate to and fro horizontally and counting the number of oscillations in a given time we can determine the horizontal pull upon the poles of the needle, *i.e.* the intensity of the earth's horizontal magnetic force upon the needle, just as by the swing of a simple pendulum in a vertical plane under the action of the force of gravity we can determine the pull of the force of gravity upon the bob of the pendulum. By a similar method and by a properly suspended needle either the vertical force or the total magnetic force of the earth may be determined.

In order then to determine the direction of the earth's magnetic force we may make use of a declination needle to give us the vertical plane, and place the dipping needle in such a position that it will oscillate in that plane; when it comes to rest it will point in the direction of the total magnetic force, *i.e.* in the direction through the room of Faraday's lines of magnetic force.

In order to determine the magnitude of that force the horizontal force may be found by finding the number of oscillations of the declination needle in the way that I have already explained, and these three determinations will give us the direction and magnitude of the earth's total magnetic force.

Another method of making the required determinations is to take a coil of copper wire, which is wound on a circular frame in such a way as to be capable of spinning on a diameter of the circular frame.

Faraday showed that on turning such a coil in a magnetic field a current of electricity is induced in the coil, and the strength of this current is proportional to the number of lines of force cut by the coil. We may describe such an arrangement as a magneto-electric machine, in which the magnet employed is the earth itself.

By means of this instrument we may determine either the horizontal or the vertical magnetic force of the earth. By placing the axis vertical and spinning the coil at a given rate we may determine the horizontal force, and by placing the axis horizontal in the magnetic meridian and spinning the coil at the same rate we may determine the vertical force, the currents produced in the two cases being in the same ratio as the numbers of the lines of force cut in the two positions.

The greater the angle at which the axis of rotation is inclined to the direction of the lines of force the greater will be the number of them included in the revolving circle, and the greater the induced current produced in the coil.

Thus placing the axis in different positions we get currents of different strengths, and may readily see that we get the greatest current when the axis is at right-angles to the direction of the lines of force, *i.e.* to the line of the dip.

We may further make use of such a coil to find the direction of the lines of force, for if we place the axis parallel to the lines of force, the currents in opposite halves of the coil will balance one another, because each line of force is cut twice by the coil, and so no current is produced in the external circuit through the galvanometer.

If then we place the coil so as to get no current when we rotate it, then the direction of the axis of the coil is the direction of the dipping needle, *i.e.* of the magnetic lines of force.

We will suppose now that for some point of time, say June 1st at 12 o'clock midday, the three magnetic elements, *i.e.* the declination, the horizontal force, and the vertical force, have been determined, we have now to consider the changes or disturbances produced in these magnetic elements, and the connection of these changes with other phenomena, and especially the connection between auroras, earth currents, and the larger and more irregular magnetic disturbances.

I have already drawn attention to the declination needle and the balance magnetometer for measuring the changes of declination and of the vertical force.

For measurement of the changes in the horizontal force a special instrument is employed, called a bifilar magnetometer, in which a magnet is suspended by two threads, which are so placed that by their torsion acting against the magnetic force of the earth, the magnet is kept at rest in a horizontal position in a direction at right angles to the magnetic meridian.

This completes the list of instruments for our magnetic observatory.

Any change or disturbance of the horizontal force pulls this magnet round more or less in the horizontal plane, and its change of position is observed as in the other instruments. The results I have to bring before you this evening have been derived from the photographic registrations of similar instruments in different parts of the world, so that the motion of the needle has recorded its own tale on the prepared paper which is wrapped on a cylinder driven by clockwork, and so placed as to receive the spot of light reflected by the moving needle.

First, there are regular daily and yearly changes, showing that the sun produces regular changes in the three magnetic elements which depend on the time of the day and the season of the year, so that the change of position and apparent motion of the sun with respect to the place of observation produce regular magnetic changes. These regular daily changes are accompanied by and have very generally been supposed to be due to electric currents or electric waves traversing the earth's crust, and a discussion by Dr. Lloyd of the observations made by Mr. Barlow in 1847 of currents on telegraph wires showed a very close relationship between the two-hourly changes of the declination needle and the changes of intensity and direction of earth currents on telegraph lines.

Both Dr. Lamont and Dr. Lloyd conclude from their comparisons of earth currents and magnetic changes that the changes of the declination needle cannot be due to the direct action of the electric current traversing the earth's crust, but that these currents or waves, extending to a considerable depth, alter by induction the magnetism of the earth itself, and this change of magnetism causes the observed changes in the declination needle. Thus the magnetic changes are the indirect effects of (not the earth current in its immediate neighbourhood, but of) a change in the magnetism of the earth itself, which may be due to an electric wave extending over a considerable area of the earth's surface.

The point towards which the total earth current is directed follows the sun and seems to lag two or three hours behind, but not the same distance behind at different places.

These earth currents have been ascribed to different causes: thus Dr. Lamont regards them as the results of electric force emanating from the sun; De Saussure regards them as developed by evaporation, the vapour being positively charged, and the water being negative; Dr. Lloyd regards them as effects of solar heat; whilst M. de la Rive ascribes them to chemical actions going on in the interior of the solid crust of the earth, the electricity being transported into the atmosphere by evaporation.

Mr. Ellis of the Greenwich Observatory has shown the intimate relation between solar action and the regular diurnal

magnetic changes of declination and horizontal force at Greenwich Observatory during thirty-five years from 1841 to 1876 by a comparison of the observations of those elements. The results of his observations are shown on a large diagram which has been enlarged from his curves, and they show what a close relationship exists between solar storms and terrestial magnetic changes. There are not only daily and yearly periods of the variations of the different magnetic elements, but there also seems to be in the horizontal intensity a period of twenty-five or twenty-six days, which is the time of rotation of the sun on his axis. Other recent investigations have shown that these regular magnetic changes depend not only on the sun, but that they are also in part due to the action of the moon, and these portions depend upon the length of the lunar day and on the position of the moon with regard to the earth. Just as there are regular earth currents whose direction depends upon the sun, which we may call the solar earth currents, so there are lunar earth currents which go through their changes under the action of the moon, and it has been shown that the effects are produced not immediately under the moon, but there is a lagging behind in the case of the lunar earth currents, just as in the case of the solar earth currents. In the case of the lunar earth currents we cannot attribute the production of the electricity either to heat or to thermo-electric currents from one part to another of the earth's crust, and we must therefore look for some other source. May we not find it in the fact that the moon causes tides in the solid crust of the earth, just as she causes tides in the oceans? The earth's crust is made up of elastic materials and materials capable of yielding and altering their form to a considerable amount with the change in the direction of the pull of the moon upon them. This crust also contains magnetic substances in abundance which alter their form under the moon's attraction, and so from the changes of position of masses of magnetic matter changes are produced in the magnetism of the earth which must give rise to induced currents of electricity or earth currents. Let us imagine a conductor of electricity outside the earth, stretching from the North Pole to the equator and fixed in space, with the earth, a magnetic body, revolving beneath it from west to east; then it follows, from Faraday's laws of induced currents, that the revolution of the earth on its axis would cause a current in the fixed conductor in a direction from the pole to the equator.

If the conductor moved over the surface of the earth from west to east, and the earth did not revolve, or revolved at a slower rate, then the current in the conductor would be from the equator to the pole. The current depends upon the relative motion of the earth and the wire. If then we have an insulated wire running north and south, the tides in the earth's crust, of which I have spoken, will be equivalent to a lagging behind of magnetic matter, and so we may expect in that wire a current of electricity whose general direction would be from the equator to the pole. The position of the wire with reference to the magnetic pole of the earth would modify the direction of these earth currents, and it is quite conceivable that the position of England with regard to the magnetic pole might cause these regular earth currents to be greatest in the south-west and north-east direction. The lagging of the lunar earth currents behind the position of the moon would also be accounted for by the lagging of the tides behind the moon. If this is a true cause for some portion at least of the lunar earth currents, then the same reasoning applied to the sun may in a smaller degree apply to the case of the regular solar diurnal earth currents, and may help to account for the lagging behind of the effects due to the sun, so that the fact that the greatest solar effect happens about 2.30 p.m. may not be entirely due to the fact that that is the hottest part of the day, but may also in part depend upon the tides.

We have now to consider those more sudden changes of the suspended magnets, which are distinguished by the name magnetic disturbances. In 1874 Dr. Lloyd said of them:—"The duration and the magnitude of these oscillations are as yet outside the domain of law, and probably depend upon so many operating causes that, like the gusts and lulls of the wind in an atmospheric storm, they will long baffle all attempts to refer them to their actuating forces, or even to reduce them to order."

Certain facts relating to these disturbances have long been known. From the series of observations started by Gauss in 1834, and made every five minutes at the same times at a variety of places, at first in Europe and afterwards in various parts of the world, the disturbing power was found to increase in northern latitudes; also it was made out that the appearance of a disturbance in several places occurred at the same time, but there were great differences in the results at different places.

In Europe the agreement was very good, and also in America; but the agreement between Europe and America was not so satisfactory.

The force seemed to originate in a certain point in the interior of the earth, and the direction of the disturbing force seemed to be constant: yet sometimes there were great differences in the deviations at places not far apart, and from the result of his observations Weber was led to believe that there was a centre of disturbances which was somewhere in the neighbourhood of St. Petersburg.

However sudden and unconnected single disturbances may seem to be, they still follow certain laws in their occurrence; Sabine found that they had daily and yearly variations from their mean values, and that they have an eleven-year period, which agreed with the eleven-year period of the appearance of spots upon the sun.

Disturbances are more frequent in summer than in winter, and this applies to each hemisphere; and it has been confirmed by various observers that they are also subject to the influence of the moon. Lamont says of these disturbances, "Their cause is a force which is subject to certain laws but which does not act constantly; the mean direction and frequency have yet to be discovered."

Observations have shown that the magnetic disturbances and electric currents on the earth are so nearly related to one another that people naturally look upon the electric currents, either in the crust of the earth or in the atmosphere outside it, as the cause of the magnetic disturbances. These currents in the earth have usually been attributed to changes of temperature, because they also are found to be in some way governed by the sun.

Now let us come to more recent observations of magnetic disturbances with the improved methods of recording observations by photography which are now available. For some years past photographic records have been taken of the magnetic elements, but the curves have been laid aside, and very little use has been made of them; so much so that some three or four years ago a circular letter from Mr. Ellery, Director of the Melbourne Observatory, was sent round to those scientific men who were supposed to be interested in the matter to know whether it was advisable to continue the photographic records of magnetic changes at Melbourne, which is the most southern station, and the only station in the southern hemisphere except Mauritius, where such observations are taken. Mr. Ellery did not for one moment suppose that they were of no value, but as no use was made of them he wished to know whether the money expended might not be better applied to another purpose. This matter has been taken up by the Kew Committee, of which Dr. De La Rue is the chairman, and a recommendation was made that the directors of all observatories which possess instruments of the Kew pattern should be invited to send to Kew their photographic records, or careful tracings of them, for a given period, so that a comparison might be made of the results.

The period chosen was the month of March, 1879, and records for the whole month have been sent from Lisbon, Coimbra, Stonyhurst, Vienna, St. Petersburg, and Bombay, in the northern hemisphere, and from Melbourne and the Mauritius in the southern hemisphere.

A preliminary account of a comparison of the declination curves from the European stations was brought before the British Association last year at Swansea, and this evening I have to bring before you some further points which come out of these comparisons. Let us take the disturbances on March 15-16, 1879, which will illustrate some of the points which I wish to bring out prominently.

Not only do magnetic changes occur at the same time at different stations, but there is a great similarity between them.

It must be remembered that at northern stations the horizontal force is smaller in proportion to the whole force than it is at stations nearer to the equator, so that the same disturbance will produce less effect on the horizontal force or on the declination needle in latitudes near the equator.

Also the needles at different stations are by no means in the same state of sensibility, and even at the same station they change with time, so that they are not always equally sensitive, and when they lose their magnetism they have to be re-magnetised.

We see that soon after 10 a.m. G.T. on March 15, 1879, there is a disturbance wave showing first a diminution and then an

increase in the horizontal force at St. Petersburg, Vienna, Kew, and Lisbon. At Melbourne in Australia there is a similar disturbance at the same time both in the declination and in the horizontal force.

Again, between 2 and 3 and between 4 and 5 p.m. there are very small disturbances showing themselves at the same absolute time in the horizontal force and declination curves. About 5.20 p.m. there is a well-marked increase in the horizontal force and eastward deflection of the declination needles. About 9.30 p.m. G.T. a storm begins which lasts for about an hour. It is felt in the northern and in the southern hemispheres, near to and on both sides of the equator. At all European stations the horizontal force is increased during the first part of the storm, and then diminished.

At Lisbon the vertical force is first increased and then diminished, and at St. Petersburg and Stonyhurst there is a diminution in the vertical force at the same time as at Lisbon. If we regard the declination needles, we find that at St. Petersburg, Zi-ka-Wei, and Melbourne, and at Bombay, the declination westward is first increased and then diminished, whereas at Kew and Lisbon the motions are in the opposite direction.

The declination at Vienna seems to be intermediate between Kew and St. Petersburg, but the curve is incomplete.

At Bombay and the Mauritius, near to but on opposite sides of the equator, the declination needles are deflected opposite ways. The local time at these places was from 1 to 2 o'clock at night.

Now in what way can we account for such magnetic disturbances as this? If we assume that by magnetic induction from some cause or other the earth's magnetism is altered, then the position of the magnet which would produce the disturbance must be such that its pole which attracts the marked end of our needle must lie at the beginning of the disturbance to the east of Kew and Lisbon, to the north of Vienna, and to the north-west of St. Petersburg; the Lisbon vertical force curve also shows it to be below the surface of the earth. Hence an inductive action equivalent to a change of position of the north magnetic pole towards the geographical pole would account for these changes. The strengthening and weakening of a magnet with its north pole to the north on the meridian of Vienna might possibly account for the magnetic changes observed between 9.30 and 10.30 at night, Greenwich time, on March 15, 1879.

If we attempt to explain this disturbance by currents of electricity or discharges of statical electricity in the air above the needles, then we must imagine that at first there is a strong current from the south-west over St. Petersburg, from the west over Vienna, and from the north-west over Kew and Lisbon, the vertical force needle at Lisbon showing that the current from the north-west lies somewhat to the east of Lisbon, that at the Mauritius this current is from the north, and at Bombay from the south.

Hence we must imagine that a current of electricity passes down from north-west to the south-east, going on towards the east over Vienna, and towards the north-east over St. Petersburg. This must be kept up very much along the same line throughout the first part of the disturbance, and then the current or currents must be altered in strength in the same manner at all stations.

We will next consider what would hardly be called a magnetic storm, but a few very small deviations of the magnetic needle, lasting from about 5.30 to 7.30 p.m. on March 26, 1879. Only the comparison of the originals will give the closeness of the similarity of the curves, and the curves for Vienna and Kew are absolutely coincident.

When the declination needle is deflected to the west, the horizontal force needle is deflected with its marked end towards the south, so that in this disturbance the two needles are drawn towards the south-west at the same time with greater or less power, and twelve similar bends are clearly traced out in the Vienna and Kew curves during the two hours. These disturbances are all so very small, that but for the comparison of photographs they would probably be lost sight of; yet we see that the same deflections occur at the same instant at Kew and at Vienna, at St. Petersburg and at Melbourne. From the remarkable similarity in these disturbances and their occurrence at the same time, we should expect that the cause of disturbance is so far removed from the places of observation that the difference of their distances from it need not be considered. This might not unreasonably be urged as an argument in support of a theory that such disturbances are due directly to the action of the sun regarded as a magnetic body. The numerical comparisons of observations made every five minutes on certain days previously fixed upon would probably never have shown the way in which these minute changes of magnetic power of the earth at widely distant places are related to one another.

In one or two cases Señor Capello and Prof. Balfour Stewart had compared the Lisbon and Kew curves for a particular disturbance, but the photographic magnetic records have never before been collected from other stations, and there has been no opportunity of comparing them. From the precise similarity of the forms of the curves in many cases we may say that the *rate of change* of magnetic disturbances at widely distant stations is the same. There is nothing fitful or flashing in such disturbances as these of March 26. We might imagine a current in the crust of the earth or a current or transfer of electricity in the air near to, *i.e.* within twenty or thirty miles of each of these observatories, but to imagine the same current and the same variations of the current at so many different stations, all changing in the same way at the same instant, is difficult, unless it can be shown in what way all these changes are connected with the cause of such a regular electric discharge. It seems easier to imagine that such changes as these are due to a change produced by induction in the magnetism of the earth itself by some distant body. It is easy to show that the magnetism produced by a current in a magnetic substance round which it flows is greater in its action on a small magnetic needle than the direct action of the current itself. Hence a current flowing in the crust of the earth should produce its principal effect on a magnetic needle by the magnetic induction which the current induces in the earth itself.

Sometimes disturbances occur where at the same instant there are similar deflections of the declination needles at stations wide apart, and suddenly at one of the stations the needle no longer goes with the others, but begins to go, and continues for a considerable period to go, in the opposite direction to the others, turning when they turn, and tracing out a similar curve, but turned always in the opposite direction. Such cases occurred frequently during March, 1879, and especially on March 23, about 1.30 and about 7 p.m., Kew time, and on March 29, about 9 p.m. An examination of the principal disturbances seems to show that:

(1) A diminution in the horizontal force is accompanied by greater easterly deflections of the declination needle at St. Petersburg than at Kew.

(2) Increase of the horizontal force is accompanied by greater westerly deflections at St. Petersburg than at Kew, or is sometimes accompanied by a westerly deflection at St. Petersburg and an easterly deflection at Kew.

These cases which I have taken will be sufficient to show how important it is that there should be additional magnetic observatories, especially in the southern hemisphere, where photographic records should be taken, so that we may learn something about the magnetism of the earth. Practically we have to rely on one excellent observatory (Melbourne) for the whole of the southern hemisphere. Surely the time has arrived when there should be photographic registration of the magnetic elements at such an important observatory as the Cape of Good Hope, especially when the French Government has decided within the last few weeks to establish a magnetic observatory at Cape Horn. With observatories at Melbourne, at Cape Horn, and at the Cape of Good Hope, the southern hemisphere would be well supplied, and probably the Russian Government would then soon establish an observatory in the east of Siberia.

Now we can readily show the way in which the magnetic instruments are disturbed in a magnetic observatory by the alteration of the strength of a magnet. Taking magnetic needles to represent the declination needle, the inclination needle, and the bifilar or horizontal force needle, we may place an electro-magnet in a given position with regard to them, and by altering the strength of that electro-magnet may cause these needles to trace out disturbances of a very decided character. In the disturbance of March 26 the greatest motion of the needle was not more than about 2′ of angle at Kew or at Vienna. It would not be possible for me to show you the action on so small a scale.

I have as yet been speaking of only moderate disturbances, but now let us come to some of the larger ones, and I have had the opportunity, through the kindness of the Kew Committee, and the observers at the various observatories mentioned, of studying the curves for the August magnetic storm which began at 10.20 a.m. Greenwich time, on August 11th, and for convenience may be divided into three storms, one lasting from 10.20 on the 11th, to 1 a.m. on the 12th; a second from 11.30 a.m. on the 12th to

7.20 on the 13th; and the third from 11. 50 a.m on the 13th to 7 to 8 a.m. on the 14th of August.

I have prepared a large sheet, on which these curves have been copied as accurately as possible for the first of these storms on the 11th. For this storm I have also the curves from Toronto and from Zi-ka-wei. The first storm began on August 11 at the same instant at all the stations. There is a decided similarity, especially in the horizontal force curves, throughout the first part of this storm, and certain points in it stand out prominently. At Kew, the beginning of the storm is not actually recorded, because the sheets of prepared paper on the time cylinders were changed precisely at 10.20 a.m. when the storm was beginning. The deflections are alike at Lisbon, Kew, Vienna, St. Petersburg, and after the very first sudden deflection, at Toronto also. The greatest effect is produced at St. Petersburg; the similarity between the large disturbances at Vienna and at Toronto in Canada, places differing about 6½ hours in time, is remarkable. About 11.45, 1 p.m., and 2.40 p.m., there are very remarkable points of agreement.

From about 4.30 p.m. to 8 p.m. Greenwich time, *i.e.*, from about 11 a.m. to 2.30 p.m. Toronto time, the deflections are opposed at Toronto, and at Vienna or Kew.

This would rather point to solar action as the cause of disturbance. In this case the Kew curve is not so much deflected as the Vienna curve, because the horizontal needle at Kew is not nearly so sensitive as at Vienna, and the relative strengths of the actual disturbing forces at the two places can only be obtained by comparison of the scale-values at the two places.

I will draw your attention to one other point on this day. At 9 p.m. the disturbances are all in the same direction, but about 11 p.m., whilst St. Petersburg agrees in direction with the others in a very violent phase of the storm, at Toronto the direction of the deflections is reversed, and this reversal of curves continues until about the end of the first of the three storms.

The second storm, beginning about 11.30 a.m. on the 12th, and lasting until the next morning, was the most remarkable of the three. It not only baffles the telegraph clerks, who wish to keep out earth currents from their lines, but it even goes beyond the powers of the magnetic observatories which are specially designed to watch over them. Thus, at Toronto, the line goes off the edge of the paper on which the photographic record is taken. At Melbourne the motion is so rapid, and also at Vienna, that the plate is not sensitive enough to receive the impressions; the motion is too quick even for photography. At the time of greatest disturbance, about 12.20 midday, it is very remarkable that at Lisbon, and at Zi-ka-wei, near Shanghai in China, two places nearly in the same latitude, but nearly nine hours apart in time, the vertical force is increased in precisely the same way and to the same amount at the same instant.

At Zi-ka-wei in China, the sudden change in the horizontal force on the needle amounted to about $\frac{1}{100}$th part of the total horizontal force, and at St. Petersburg the change in the horizontal force amounted to $\frac{1}{85}$th part of the horizontal force, and the total force was changed by about $\frac{1}{86}$th part of its full value.

Hence, any cause for these magnetic changes, in order to be a true and sufficient one, must be capable of producing such intense magnetic changes as these all over the surface of the earth. These magnetic changes are so large as to be quite comparable, as we see, with the earth's total force, so that any cause which is shown to be incompetent from the nature of things to produce the one can hardly be held to account for the other.

Since, as I have shown, the large disturbances and the small disturbances do not follow totally different laws, but agree equally well all over the earth, in so far as they agree we must attribute them to the same cause.

During this August storm, as also during the remarkable storm of January 31 last, great difficulties were experienced in working the telegraph lines, and Mr. Preece has been kind enough to send me particulars of these storms.

I am also greatly indebted to the Astronomer-Royal for sending me traces of the earth-current photographic records taken at Greenwich Observatory during the August storm on two separate wires, one running from the north-east, and the other from the south-east, to Greenwich. The two tracings are bent opposite ways at the same time, so that when a current was running on one line towards Greenwich, on the other it was running away from it, and comparing these curves with the earth-current records from Derby and Haverfordwest and other places, it appears that the general direction of currents during this storm was from south-west to north-east, or from south-south-west to north-north-east, with varying intensity, the agreement being very close between the disturbances of the declination needle and the Blackheath and Greenwich photographic record. From Mr. Preece's record also earth-currents were violent from 10.30 a.m. on the 11th (*i.e.* they were noted within ten minutes of the beginning of the magnetic storm) to about 2.30 p.m., and again from 9 to midnight.

They were very violent on August 12, beginning at 11.30 a.m., the beginning of the second storm, and quieting down about 4.30 p.m., then beginning again at 7.30 and lasting until 9.30 p.m.

Again on the 13th they are strong for 1½ hours, from about 5 in the morning, *i.e.* just about the end of the second magnetic storm.

The general direction of the earth-currents as observed at Derby or Haverfordwest, as well as at Greenwich, was from north-east to south-west.

Again on January 31 last another violent magnetic storm occurred, in which the currents were even more violent than in the August storm.

Intimately connected with magnetic disturbances and earth currents is the phenomenon of the aurora or polar light, which is an electric discharge in the upper regions of the atmosphere. During the August and January storms the aurora was well seen in England; it was also seen at St. Petersburg, and as far east as Siberia. It does not appear to have been seen, although it was looked for, at Zi-ka-wei in China by M. Dechevrens, the director of the observatory, although the magnetic storm was so violent there that the horizontal force was suddenly changed by $\frac{1}{100}$th part of its total amount.

We may arrive at some idea of the character of the aurora by studying electric discharges in vacuum tubes, and Dr. De La Rue has already brought this subject before you in his Friday evening lecture.

We may gradually pass from electric discharges in air of ordinary density, in which we get the well-known electric spark between two surfaces, to air of less density but better conducting power, and then to air of less density still, but of such high resistance that no electricity will pass. Dr. De La Rue has shown that with 11,000 cells of his battery the striking distance between two points is about six-tenths of an inch in air of ordinary density of about 760 mm. pressure.

When the pressure in a hydrogen tube is reduced to 21.7 mm. 8,937 cells will cause a discharge to take place through thirty inches.

When the pressure is reduced to ·642 (about six-tenths of a mm.), 430 cells will cause a discharge through the tube.

When the pressure is still further reduced to ·0065, it requires 8,937 cells to cause the discharge, so that the spark passes more readily at a pressure of ·642 mm. than it does at a higher or a lower pressure. This is also the case with air.

The lower regions of the earth's atmosphere offer great resistance to the passage of electricity, but as we ascend the pressure diminishes and the electric resistance diminishes, until at last, at a height of between thirty and forty miles, a level is reached where the air offers least resistance to the passage of electricity, where the pressure is about ·397 of a mm., and above that level the electrical resistance again increases, so that at a height of about eighty miles the battery of 11,000 cells would not cause a spark to pass.

If we take a tube which has not been very highly exhausted we see that the light from the positive pole extends nearly through the tube, and the dark space around the negative pole is small. As the exhaustion proceeds and the pressure of the air is diminished, the electric spark passes through greater and greater lengths and changes its character, until we get to the pressure corresponding to the least resistance. Beyond that the resistance increases, the dark space around the negative pole expands and the molecules fly about more freely; those on the negative plate being charged with electricity, and being repelled from it proceed for a long distance in straight lines, and possess the power of causing bodies on which they strike to glow. In Mr. Crookes's tubes we get very beautiful effects from this glowing of the glass tube itself, or from the glowing of substances in the path of the stream. We may regard this as a stream of molecules of gas charged with electricity, and we see the difference between this stream and the electric current in a vacuum tube at lower exhaustion by the action of the magnet upon it. In one case the current going through the molecules from pole to pole in the tube is bent out of its course by the magnet, and symmetri-

cally by the two poles, and returns to its path, the line of least resistance, through the molecules, whereas the stream of molecules at the higher exhaustion, carrying their electricity with them, are carried away by the electric charge upon them, and get utterly lost and scattered on striking the side of the tube, yielding up a great deal of energy in the form of heat to the tube or to the glowing platinum or other substance in the tube.

I must now show you the beautiful aurora tube which has been seen once in this theatre, and for which I am indebted to the kindness of Dr. De La Rue. It has been brought to the right state of exhaustion to show just those effects which will help better than any description of mine to give you an idea of the character of the aurora discharge in the middle regions of the atmosphere.

By bringing a magnet to bear upon this discharge we may see the effect of terrestial magnetism on the aurora discharges in the atmosphere.

Aurora Borealis.—The aurora as seen in the north-eastern parts of Siberia, where it is often very brilliant, is described as consisting of single bright pillars rising in the north and in the north-east, gradually covering a large space of the heavens; these rush about from place to place, and reaching up to the zenith, produce an appearance as if a vast tent was spread in the heavens, glittering with gold, rubies, and sapphires.

More exact attempts have been made to describe the aurora, and perhaps I may be allowed to quote Dalton's description of an aurora as seen by him.

A remarkable red appearance of clouds was noticed in the southern horizon, which afforded light enough to read by, and a remarkable effect was expected. He says, "There was a large luminous horizontal arch to the southward, and one or more concentric arches northward. All the arches seemed exactly bisected by the plane of the magnetic meridian. At 10.30 streamers appeared in the S.E. running to and fro from W. to E.; they increased in number, and approached the zenith, when all of a sudden the whole hemisphere was covered with them, and exhibited such an appearance as baffles all description. The intensity of the light, the prodigious number and volatility of the beams, the grand intermixture of all the primitive colours in their utmost splendour, variegating the glowing canopy with the most luxuriant and enchanting scenery, afforded an awful, but at the same time a most pleasing and sublime spectacle. But," he adds, "the uncommon grandeur of the scene only lasted one minute. The variety of colours disappeared, and the beams lost their lateral motion, and were converted, as usual, into the flashing radiations; but even then it surpassed all other appearances of the aurora, in that the whole hemisphere was covered with it."

In his address before the British Association in 1863, Sir William Armstrong speaks of the sympathy between forces operating in the sun and magnetic forces on the earth, and notices a remarkable phenomenon seen by independent observers on September 1, 1859.

"A sudden outburst of light, far exceeding the brightness of the sun's surface, was seen to take place, and sweep like a drifting cloud over a portion of the solar surface. This was attended with magnetic disturbances of unusual intensity, and with exhibitions of aurora of extraordinary brilliancy. The identical instant at which the effusion of light was observed was recorded by an abrupt and strongly-marked deflection in the self-registering instruments at Kew. The magnetic storm commenced before and continued after the event."

The daily and yearly periods of the magnetic changes, the change in the horizontal force depending on the sun's rotation on his axis, the agreement of the eleven-year period of magnetic disturbances, sun-spots, and auroras, show that the sun plays a very important part in causing or governing both the regular and irregular magnetic changes.

If the sun be assumed to be a very powerful magnet, then changes in his magnetism might be expected to affect the magnetism of the earth, although the effect could not be very large, unless the sun is magnetised to an intensity much greater even, compared to his mass, than the earth is magnetised. Then as there are tides in the sea around us and probably in the earth's crust, so there are certainly very large tides in the ocean of air above us: and may not the sun and moon, by dragging this air towards them as the earth revolves, cause that friction between air and earth, and also that evaporation, which together may account for the presence of, and keep up the supply of, positive electricity in the air and negative electricity in the earth? Again, these tides in the atmosphere will cause the mass of it to lag behind the revolving solid earth, and at a height of thirty or forty miles we have a layer of air which, for air, is a comparatively good conductor of electricity. Here then we have not a lagging of the magnet behind the conductor, but a lagging of the conductor behind the magnet, and hence, according to the laws of Faraday, we may expect a current or a gradual heaping up of electricity in the air in the opposite direction to the current in the earth's crust. Thus the regular tidal waves in the atmosphere would cause the gradual transfer of positive electricity from the poles towards the equator. This transfer may be of the nature of a current of electricity or of a mass of air carrying a static charge of electricity with it, for as Prof. Rowland has shown that the motion of a static charge will produce magnetism, so we may expect from the principles of conservation of electricity that a change in the position of a magnet will under such circumstances produce motion of the static charge of electricity. When the air becomes charged up to discharging point, then we may get the sudden discharges such as the aurora in the air and the earth current in the earth; and since the conducting layer of air approaches nearer to the earth in the colder polar regions, possibly within less than twenty miles of the earth's surface, it may be found that the discharge of the aurora may even take place from earth to air by gradual slow discharge, aided as it may be by the state of moisture of the air and by change of temperature and other causes.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—W. H. Caldwell, B.A., of Caius College, has been nominated to study at the Zoological Station at Naples.

Mr. F. M. Balfour, F.R.S., has been elected President of the Cambridge Philosophical Society. Prof. Newton in laying down the office referred with pleasure to the removal and the change in the management of the Society's library. Since the transfer to the new room about 500 volumes have been presented to the library by Professors Humphry and Newton, Mr. J. W. Clark, Mr. F. M. Balfour, and Mr. Horace Darwin.

Open Scholarships for Natural Science have been offered by Trinity College (date of examination, March 22 next); subjects those of the Natural Science Tripos; by St. John's College, subjects, Chemistry, Physics, Physiology, with Geology, Comparative Anatomy, and Botany (the last three only if notice be given beforehand), date, March 22; by Caius College, date March 28, subjects, Chemistry, and either Biology or Physics; by Christ's, Emmanuel, and Sidney Sussex Colleges, at a common examination; subjects, Physics, Chemistry, Biology, and Geology; date March 28.

UNIVERSITY COLLEGE, LIVERPOOL.—The Council of the College have appointed Dr. W. A. Herdman to the professorship of natural history, founded by Lord Derby in connection with University College. Dr. Herdman is a graduate of the University of Edinburgh. He took the degree of Bachelor of Science in 1879 in the department of Natural Science, and in the same year was intrusted by Sir Wyville Thomson with the preparation of the report on the collection of Tunicula obtained during the *Challenger* Expedition. The Council have also appointed Dr. J. Campbell Brown to the professorship of chemistry in the same college. Dr. Campbell Brown has for several years held the office of borough and county analyst, and of lecturer upon chemistry at the Royal Infirmary School of Medicine.

DR. ALEXANDER BAIN, lately Professor of Logic at Aberdeen, has been elected Lord Rector of that University.

SOCIETIES AND ACADEMIES

LONDON

Anthropological Institute, November 8.—Prof. W. H. Flower, F.R.S., vice-president, in the chair.—The following new Members were announced:—Miss Becker, Mrs. R. Crawshay, Mrs. Lloyd, Miss Mary Sheldon, Miss Eleanor E. Smith, Miss Wolfe, Prof. Acland, F.R.S., James Backhouse, William Bowman, F.R.S., Alfred T. Brett, M.D., Rev. H. Canham, John G. Garson, M.D., Hugh T. Hall, F.G.S., Capt. Hozier, W. J. Knowles, E. Llanfair Lewes, Alfred Lingard, M.B., G. D. Longstaff, M.D., William Parkin, H. Seebohm, F.L.S., Mark Stirrup, F.G.S., H. Stopes, F.G.S., Richard Thompson, Prof. E. Perceval Wright, F.L.S.; also Dr. Josef Majer of Cracow as a Corresponding Member.—Dr. J. G. Garson exhibited some improved forms of anthropometric instruments.—Mr. Everard

F. im Thurn read a paper on the animism of the Indians of British Guiana. After defining animism as belief in the existence of spirit in any form, the author stated that the animism of the Indians of Guiana, in common probably with that of many other American tribes, is not only of an exceedingly pure and rudimentary kind, but is much more primitive than has yet been recognised by students of religious evolution. The Indian belief is that each object and phenomenon of the visible world consists of body and spirit; and these countless dual beings differ from each other only in bodily form and in the degree of brute force or cunning which they possess, but are none of them distinguished by the possession of any sort of divine character. There is no belief, of genuine Indian origin, in gods or a God, in heaven or hell, or in reward or punishment after death; nor is any form of worship practised. The author also indicated how in this belief may be found the germs from which all the features of the higher religions have arisen by modification.

PARIS]

Academy of Sciences, November 7.—M. Wurtz in the chair.—The following papers were read:—On the limits of electrolysis, by M. Berthelot. In the electrolysis of sulphate of potash (as also of haloid salts), the smallest sum of energies capable of working the decomposition is far below that which would be required for the previous setting at liberty of the alkaline metal. It is equivalent to the separation of the acid and the base, plus either the separation of the oxygen and the metal at expense of the base, or the decomposition of the acidulated water.—On the combustions effected by bioxide of nitrogen, by M. Berthelot. The property of burning at expense of bioxide of nitrogen under influence of a flame or electric spark, depends mainly (the author shows) on the temperature developed. Of the mixtures tabulated, none that develop a temperature theoretically under 7000° are inflamed; while those developing a higher temperature burn or detonate.—Synthetic experiments in artificial reproduction of meteorites, by MM. Fouqué and Lévy. By igneous fusion they have obtained bodies closely resembling some oligosideric meteorites; one type containing felspathic products, and another not. The former contains anorthite associated with pyroxyene and enstatite (eukrite) or peridote (howardite); the latter peridote, enstatite, oxydulated iron, and a pyroxene exclusively magnesian.—Solution of two questions of maritime hydraulics, by M. Cialdi. One refers to the force governing the sand of banks and harbours; (Cialdi's theory that the undulatory motion is the prime force is now accepted, as against that which puts the littoral current first). The other refers to the method of construction of moles for harbours by the Romans; (they did not make these with apertures where there was exposure to the open sea).—On the comparison of the waters of the Isère and those of the Durance in their hydrographic and agronomic relations, by M. de Gasparin. He controverts some views of M. Dumont.—Report on a memoir of M. Leauté on teledynamic transmissions. The author's solution of the problem is pronounced complete, theoretically and practically.—On silica and silicates of lithine, by MM. Hautefeuille and Margottet. *Inter alia*, three new crystallised silicates of lithine have been obtained (by means of chloride of lithium in fusion), and it is proved that silica may take the form of quartz in presence of a fused chloride.—On the means to employ for destroying the winter-egg of phylloxera, by M. Mayet. He considers it best to operate where the vines have every year galls on the leaves, and to treat (with insecticide) only the wood of two or three years.—A work by M. du Bocage (Lisbon) on the "Ornithology of Angola" was presented.—Elements of Denning's comet (1881 *f*), by M. Schulhof.—On a general formula for development of the principal part of the perturbative function, by M. Baillaud.—On the reduction of Abelian integrals, by M. Picard.—On linear differential equations, the integrals of which verify relations of the form $F[\phi(x)] = \psi(x)F(x)$, by M. Appell.—On the integration of an equation with partial derivatives of the second order, by M. Teixeira.—Mode of transmission, in an isotropic solid (in equilibrium) of the pressure exerted on a very small part of its surface, by M. Boussinesq.—On the possibility of electric equilibrium, by M. Lévy.—On the product and the limit of operation of the transport of force by electricity, by M. Lévy.—Articulated systems, giving rectilinear motion or circular curvature, by Prince Gazarine.—Experimental method of determination of the ohm, by M. Lippmann.—Action of cold on the voltaic arc, by M. Tommasi. He used as rheophores, copper U-tubes (placed *vis-à-vis* horizontally), through which ran cold water. The luminous power is considerably weakened; the arc is very unstable; it does not ignite paper held 0·004m. above it; it is very mobile, and its form is like that of a drop of liquid in the spheroidal state; it is attracted and put out by a magnet; and there seemed to be more ozone than when the arc is not cooled. The flame was slightly green.—On an electric method for determining, with a needle, the position and depth of a projectile, or other metallic substance, in the human body, by Prof. Bell. A fine needle, connected by wire with a telephone, is inserted; and a metallic plate, similarly connected, is applied to the skin. A sound is heard when the ball is reached. A trembler may be introduced into the circuit giving a musical note in the telephone on contact of needle and ball; a battery may also be included.—New demonstration of Riemann's theorem, by M. Croullebois.—The direct-vision spectroscope with calcareous spar, by M. Zenger. He combines a single prism of spar of 75° refringent angle, with a fluid prism (sulphide of carbon, oil of cassia, or other liquid) of the same angle. The dispersion is very great. The red image of the protuberances can be obtained with great intensity, there being small loss by absorption and none by reflection.—On the function which expresses the gaseous state, by M. Gouilly.—On cuproso-cupric sulphite, by M. Etard.—On a hydrate of chromic bromide, by M. Varenne.—Action of hydracids on alkaline chromates, by M. Varenne.—Reply to M. Debray on dissociation of sulphydrates of ammonia, by MM. Engel and Moitessier.—On the vapour-tensions of carbamate of ammonia, by M. Isambert.—Modifications of composition of green fodder kept *en silo*, by M. Lechantier. Maize and trefoil lost a little of their azotised matter, but the loss of glucosides was much greater; the chief loss being now in the glucose and sugar group, now in the starch and cellulose. Fatty matter increased.—Artificial peridote produced in presence of steam, at ordinary pressure, by M. Meunier.—Action of hydriodic acid on chloroiodide of propylene and on chloride of isopropyl, by M. Silva.—On the contagion of tuberculosis, by M. Toussaint. *Inter alia*, tuberculosis becomes more powerful and rapid in its action the oftener it is inoculated.—On the physiological action of codethyline, by M. Bochefontaine. This action seems to consist in an exaltation of the reflex properties of the grey substance of the bulbo-medullary nerve-centres.—Contribution to a study of Flagellata, by M. Kunstler. He has found an organism very like noctiluca, but living in fresh water.—Observations on rotators of the genus *Melicerta*, by M. Joliet.—On the vitality of germs of *Artemia salina* and *Blepharisma lateritia*, by M. Certes.—On the winter spores of *Feronospora viticola*, by M. Prillieux.—Discovery of gypsum in the strata of the superior Eocene formation of Peru, by M. Caraven-Cachin.—On the characters of speech in deaf-mutes taught to articulate, by M. Hément. He maintains that these persons have the accent of their country, indicating organic conformations similar to those of their parents. M. Blanchard disputed this.—On the spontaneous insensibility of the sensitive plant, by M. Musset. A sudden fall of temperature suspends spontaneous movements of the plant (as chloroform, &c., suspend provoked movements)

CONTENTS

ERRATUM.—Vol. xxiv. p. 509, in the letter of Prof. Alex. Agassiz, for *Polydonia* read *Polyclonia*.