THURSDAY, NOVEMBER 13, 1884

WORLD-LIFE

World-Life; or, Comparative Geology. By Alex. Winchell, Professor of Geology in the University of Michigan. Pp. 642. (Chicago: Griggs, 1883.)

AT the present day cyclopædic knowledge has become very rare, and a scientific man is generally like a miner intent on his own special shaft, and too often careless or ignorant of the general plan of the whole mine of science. The work of the collator and summariser is thus continually rising in importance, and care, patience, and judgment are now more requisite than ever before. Although these scientific "consolidation acts" can hardly fail to be open to criticism, yet every man of science must receive them with gratitude, for they afford him a general view of his science, and furnish him with a useful repertory of reference.

In this work Prof. Winchell's field is very wide, when he undertakes to collate astronomy, cosmogony, and geology, in the widest acceptation of these terms. So many subjects does this book touch on that it will only be possible within the limits of an article to give a general view of its scope. The author's reading has been extensive, and we are glad to observe that copious references are provided. He expounds with care, although perhaps sometimes too diffusely, the views of many writers, and thus brings to a focus a great mass of literature, and his own speculations are generally interesting, although not always above criticism.

As already indicated, this work is intended to give a general survey of stellar and planetary systems, to note the marks of evolutionary processes revealed by the telescope, to discuss various cosmogonic theories, to examine the probable life-histories of nebulæ, suns, planets, and satellites, and to consider the influences under which the surfaces of planets are modelled and transformed.

Modern cosmogony is properly a department of physics and dynamics; but when states of matter irreproducible in the laboratory, and the mechanics of systems too complex for rigorous mathematical treatment, are dealt with, moderation in the general reasoning employed has not always been duly observed. No one can doubt that speculation is of the highest scientific importance, but it is also equally certain that in work of this kind a descending scale may be formed, beginning with speculations founded on rigorous mechanical principles and ending with wild and lunatic fancies. Every writer on such topics must, I suppose, sometimes question himself with misgiving as to where in such a list his name would stand. Mr. Winchell appears to treat all speculations with judgment, although one is sometimes tempted to think the exposition over-elaborate and the consideration too patient.

The first part or book is entitled "World-Stuff," and begins with a good account of meteors and meteoric dust. The author thinks that, according to Mr. Aitken's theory of the formation of fog, the highest clouds in our atmosphere reveal the presence there of a very fine dust, probably of cosmic origin. The sunset-glows of last winter appeared to illuminate clouds at an unusual altitude: may not these clouds have owed their existence to the very dust which caused the glow?

The zodiacal light is then described, and is attributed to swarms of meteorites circulating round the sun, and the visibility of the light on both horizons simultaneously is taken as showing that the orbits of some of them are greater than that of the earth. The author also suggests the probability that swarms of meteorites circulate about the planets as satellites.

Comets, whose association with meteorites is now generally accepted, are described. Later (p. 77) the author writes:—

"The phenomena of the tail, especially in the vicinity of aphelion, are such as would result if we could conceive the nucleus of the comet surrounded by an aura extending on all sides as far as the remotest limits of the tail, and could recognise the tail as merely a *luminous shadow* cast by the nucleus in intercepting certain radiant energy proceeding from the sun. . . . The tail would be, therefore, not a material form moving with the comet, but something perpetually renewed, while the older and more distant emanations disappear from visibility."

That theory which divides the tails of comets into three classes, according to the gas of which they are formed, is not given.[1]

The nebulæ are then passed in review, and are well illustrated by drawings. They are classified as amorphous, spiral, spiro-annular, annular, and planetary, and the class is taken as giving an indication of the stage of evolution.

In the case of a spiral nebula, such as that in Canes Venatici (Fig. 8, *op. cit.*), it seems difficult to believe that we view the whole. And we suggest that the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity, according to a well-known hydrodynamical law. From this point of view the visible nebula may be regarded as a luminous diagram of its own stream-lines.

In the second chapter the author enters on the generation of heat in nebular masses. The discussion appears unsatisfactory, and as it is a matter of primary importance, I propose to make some criticisms thereon. The usage of mechanical and thermic terms is loose, so that it is somewhat difficult to determine the author's meaning.

The question is concerning the generation of heat in a contracting nebular mass, and on p. 86, § 9, he concludes:—

"It is true, then, that contraction develops heat, and that its development delays final refrigeration; that is, the progress toward final refrigeration is not as rapid as the amount of radiated heat implies. But it is not true that contraction (from cooling) can have developed the whole amount of heat at any time existing in the mass, or can even maintain a body at a constant temperature."

From this conclusion I venture to dissent, and in order to show my grounds I will give a paraphrase of the author's argument, as far as I am able to grasp it.

Let there be two similar planetary spheres with layers of equal density similarly arranged, and let the linear dimensions of the smaller (or say configuration β) be 1-nth of those of the larger (or say configuration α); or,

[1] This was sketched by Prof. Ball in his late lecture at Montreal, but I have unfortunately forgotten the originator's name

in other words, let a and a/n be any corresponding radii of α and β.

Let the mass, however, contained within radius a of α be equal to that within radius a/n of β; so that β might be formed from α by simple contraction; and suppose both systems to be in hydrostatic equilibrium. Then it is easy to show that if ρ be the density at any point of α, the corresponding density of β is $n^3\rho$; and if p be the pressure at the same point of α, the corresponding pressure of β is n^4p; and lastly, the modulus of elasticity being $\rho dp/d\rho$ at any point of α, the corresponding elasticity of β is $n^4\rho dp/d\rho$.[1]

Now if we suppose the mass to have contracted from a state of infinite dispersion to the configurations α or β, there must in each case be a certain exhaustion of potential energy of mutual attraction of matter, developing heat in the mass. Then it may be shown that if h is the exhaustion of energy of the matter within a radius a in passing from infinite dispersion to configuration α, the exhaustion of energy of the matter within a radius a/n in passing from infinite dispersion to configuration β is nh.[2] The same is also true of any stratum in course of its contraction. If we take a succession of configurations with radii infinity, 1, $\frac{1}{2}$, $\frac{1}{3}$, &c., in harmonic progression, a constant amount of heat will be generated in passing from any one configuration to the next.

Now let us suppose that in course of contraction neither convection, conduction, nor radiation takes place; then if the temperature in the condition of infinite dispersion be zero, and if the specific heat be constant, the temperature of any stratum a of α being θ, that of stratum a/n of β will be $n\theta$. In this case $\rho\theta$, being density multiplied by absolute temperature, becomes, in passing from α to β, $n^4\rho\theta$. If, therefore, the modulus of elasticity varies as density multiplied by temperature, we have the elasticity in β n^4 times that of α. But we have already seen that $\rho dp/d\rho$ is augmented in passage from α to β by the factor n^4. Hence the hypotheses as to arrangement of strata, specific heat, and law of elasticity are such as to insure equilibrium in every configuration, if it holds in any. This law of elasticity is that of the *isothermal* contraction of a so-called perfect gas.

Now Mr. Winchell's argument appears to me to be that, when there is loss of heat by radiation, there is necessarily deficiency of temperature to make up the elasticity, and thus equilibrium is impossible unless we look for heat from other causes. He does not seem to notice, however, that it will be far nearer the truth (if any such physical hypotheses can be said to be near thereto) to take the elasticity from the adiabatic contraction of the perfect gas, which we know to vary as $\rho^\gamma\theta$, where $\gamma = 1{\cdot}408$. With this law the argument breaks down. In any case the constancy of specific heat, the similarity of successive configurations, and the law of elasticity of "perfect" gases are untenable. In order, however, to do justice to the author I must point out that he attributes later the supply of heat to "conglomeration," which differs I presume from "contraction" in the supposed absence of hydrostatic equilibrium in successive stages, and in the irregularity of the masses concerned.

The paragraph in this chapter on nebular rotation appears to clothe the matter in an unnecessary mystery. Surely we may admit that the existence of a nebular mass with an absolute zero of resultant moment of momentum is highly improbable; and if the expanded nebula has finite resultant moment of momentum, then *must* the agglomerated nebula rotate. Even with zero momentum the nebula might perhaps divide into two portions with equal and opposite momenta.

We next come to paragraphs on nebular annulation and the "spheration" of rings. The intractability of these problems to mathematical treatment renders the discussion highly speculative, but the author seems to treat his subject with discretion.

The second main division of the work bears the title of "Planetology." An elaborate survey of the solar system is given, with a consideration of the arguments for and against the nebular hypothesis. The fact that the inner satellite of Mars revolves in a period shorter than that of the rotation of its planet is regarded as a great difficulty in the acceptance of Laplace's theory. Our author, whilst suggesting as an explanation a diminution of the primitive period through the influence of a resisting medium, refers favourably to the theory that solar tidal friction has retarded the planet's rotation whilst leaving the period of the satellite unaltered. I have myself regarded the fact of which we speak as a very striking confirmation of the importance of tidal friction in planetary evolution.

Faye's modification of the nebular hypothesis, in which the planetary annuli are supposed to arise in the interior of the nebula, is criticised by Mr. Winchell with some success. An account is also given of Spiller's theory. That author rejects the annuli entirely, and supposes the planets to arise by a combination of tidal action with centrifugal force. The formation of the planet is supposed to take place after the central mass has reached the condition of igneous fluidity.

"It is manifest that a separated planetary mass must produce a tidal swell of some magnitude upon the fluid central mass. . . . At some perihelion of the planet therefore—concurring perhaps with a conjunction of planets—the centrifugal tendency of the equatorial portion of the central fluid mass would exceed gravitation, and the tidal swell would be lifted bodily from connection with the central mass. . . ."[1]

Neptune generated Uranus, Uranus Saturn, and so on.

Now I venture to say that Spiller could not have made any numerical estimate of the efficiency of a planet's tidal action on the sun, or he could not have proposed this fantastic theory.[2] It would therefore hardly have seemed to me worth while to have referred to this passage had not Mr. Winchell stated that this theory might be regarded as a prototype of one of my own.

I had suggested that when the earth, then without a satellite, was rotating in four or five hours, the free period of oscillation of the fluid planet would be almost the same

[1] The reader acquainted with Laplace's theory of the earth's figure will have no difficulty in proving this, or even a simple acquaintance with hydrostatic principles will suffice.

[2] The exhaustion of a homogeneous sphere of mass M and radius a is $\frac{3}{5}\mu M^2/a$, where μ is the attractional constant. Hence for a heterogeneous sphere we have $\frac{16}{3}\pi^2\mu\int_0^a \rho^2 a^4 da$. If ρ becomes $n^3\rho$ and a becomes a/n, obviously the exhaustion becomes n times as great as before.

[1] P. 213, *op. cit.*

[2] For such an estimate see a paper "On the Tidal Friction of a Planet attended by several Satellites, &c." (*Phil. Trans.* Part 2, 1881). On p. 515 it is shown that, supposing the coefficient of viscosity in the sun to be the same as that in the earth, then the increase of earth's orbital moment of momentum due to earth's tides in the sun is 1/113000th part of that due to sun's tides on the earth. See also Table III. p. 526.

as the period of the solar semi-diurnal tide, and that the solar tide might undergo such kinetic augmentation as to rupture the planet. A piece torn off might form the moon. The suggestion was only thrown out tentatively, and it might perhaps have been better had it been suppressed. The whole essence of the suggestion lies, however, in the approximate identity of the free and forced periods of oscillation, and this reasoning has no place in Spiller's theory.

In considering the history of a cooling planet, the author is opposed to Sir William Thomson, and concludes that the surface would harden into a crust. It seems to me that the time is hardly ripe for a very confident opinion on the point.

A large place is given in this book to the influence of tides in the evolution of a planet. A description is given of the tidal retardation of planetary rotation and the recession of the satellite; and the chapter is in fact principally a *résumé* of my own papers. The author is at one with me in rejecting Prof. Ball's view, that an enormous exaggeration of marine tides can have taken place within geological history. He is inclined to adopt the view that the trends have been imparted to our great continents by means of the wrinkling consequent on tidal friction in a primitively viscous mass; but he hardly notes, as I pointed out, that if this be so we have to accept a continuous adjustment of the general ellipticity of the earth to a figure of equilibrium, without obliteration of the wrinkles. The suggestion is thus perhaps placed in almost too favourable a light.

On p. 282 Mr. Winchell speaks as though solar tidal friction is adequate to cause a sensible lengthening of the year, so that in earlier ages it was sensibly shorter. It is impossible to admit the correctness of this view, as I have elsewhere shown.[1]

In a section on orogenic forces we have, amongst much other interesting matter, an account of M. Favre's experiment, in which a layer of clay is placed on a tense elastic membrane, which is then allowed to contract: an illustration of many of the facts of mountain geology is thus furnished.

In the following chapter the author follows the various lines of argument by which limits are placed on the age of a planet, and by a subsequent geological discussion endeavours to derive a time scale; but I feel incompetent to judge of the worth of the conclusion. We may regret to find the revival in this place of Prof. Haughton's argument, viz. that the absence of a measurable nutation of 306 days proves the enormous antiquity of the elevation of Europe and Asia. The argument is, I think, worthless, as I believe that Prof. Haughton now admits.[2]

The principal topics dealt with in the rest of the book are the geology of the moon, the physical condition and habitability of other planets, and the final effects of tidal friction.

The fourth main division of the book is historical, and contains a review of the evolution of cosmogonic theories, with an exposition of the speculations of Kepler, Descartes, Leibnitz, Swedenborg, Kant, Lambert, William Herschel, and Laplace.

From the account which has now been given of this work it must be evident that Mr. Winchell set before himself a task of portentous magnitude, and that he has performed it conscientiously. The criticisms which have been made should not impair the conviction that the student of this group of subjects will find his work of great value.

G. H. DARWIN

[1] *Phil. Trans.* Part 2, 1881, p. 524: "From this it follows that, if the whole of the momentum of Jupiter and his satellites were destroyed by solar tidal friction, the mean distance of Jupiter from the sun would only be increased by 1/25000th (*misprinted* 1/2500th) part. The effect of the destruction of the internal momentum of any other system would be very much less."

[2] See *Proc. R.S.* February 19, 1878, No. 186, p. 1, "On Prof. Haughton's Estimate of Geological Time."

LETTERS TO THE EDITOR

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

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

The Pentacrinoid Stage of Antedon rosaceus

IN compliance with Prof. Herdman's request, I have to state that my experience—acquired during seven years of consecutive dredging in Lamlash Bay (1855-61)—is in entire accordance with his own. Although the most active period of reproduction in *Antedon rosaceus* is undoubtedly (as stated by Sir Wyville Thomson) the *early* part of the summer, so that the Pentacrinoids which spring from the ova then matured and fertilised are ready to drop off their stems in the succeeding autumn, yet I never failed to obtain *Pentacrinoids* in all stages, as well as *Antedons* still "in fruit," throughout the months of August and September. In fact, the whole of my study of this type—which, as regards the skeleton, is fully recorded in my memoir in the *Philosophical Transactions* for 1865, and of which, as regards the soft parts, a general account is given in the *Proceedings* of the Royal Society for 1876, was carried out during those months; my official duties keeping me in London until after the first week in August.

I may take this opportunity of directing the attention of those interested in Crinoidal structure (1) to a communication I have recently made to the Royal Society (*Proceedings*, May 29) on the Nervous System of the Crinoids; (2) to a paper by Prof. A. Milnes Marshall in the *Quarterly Journal of Microscopical Science* for July last; and (3) to a paper by Dr. Carl Jickeli of Jena, in the *Zool. Anzeiger*, 7 Jahrgang, No. 170.—The doctrine I propounded on this subject nearly twenty years ago (that the quinquelocular organ contained in the centro-dorsal basin of *Antedon* is a *nerve-centre*, and that the radial cords issuing from it, which traverse the calcareous segments of the arms and pinnules, and give off branches to the successive pairs of muscles, are *nerve-trunks*), though supported by the experimental evidence which I published in 1876, and by the careful microscopic investigations of my son, Dr. P. Herbert Carpenter, has not been accepted by Zoologists generally; being for the most part either ignored altogether, or pooh-poohed as "evidently" fallacious, because inconsistent with homological theory. When I made my recent communication (1) to the Royal Society, summing up the very remarkable confirmatory evidence afforded by my son's inquiries, and referring (as Prof. Marshall had kindly enabled me to do) to the then unpublished results of his experiments (2), which entirely tallied with my own, Prof. Huxley, while admitting the strength of my case, remarked that the position I assign to the nervous system of the *Crinoidea* is as anomalous (in relation to that of Echinoderms generally) as it would be for a Vertebrate animal to have its spinal cord lying along its ventral surface. In reply, I asked, "What more proof *can* you ask for, of the nervous function of the quinquelocular organ and radial cords?" The only additional evidence that Prof. Huxley could suggest, was the result of electric stimulation. Before my paper was published in the *Proceedings*, I learnt (3) that this experiment had been actually tried four years ago by Dr. Jickeli, whose results entirely confirmed my doctrine.

It is to be hoped, therefore, that those who have so confidently and persistently clung to a homology, which is in direct contradiction to the most complete and conclusive proof that experiment can afford—supported as this is by the large body of anatomical and histological evidence summarised in my recent paper—will now see that unless they can disprove the statements of Prof. Marshall, Dr. Jickeli, Dr. P. Herbert Carpenter, and myself, they are bound to admit my doctrine, and to show how their theoretical homology is to be reconciled with it.

WILLIAM B. CARPENTER

56, Regent's Park Road, London, N.W., November 3

Natural Science for Schools

THE thoughtful and suggestive paper of Prof. Armstrong in the last number of NATURE (p. 19) is to be commended to the attention both of science teachers and of the head masters of our schools. It is undoubtedly true that, with few exceptions, science is still either completely neglected by our schools or handled in a way which does not at all tend to advance its interests. When it is made a "refuge for the destitute," or considered only fit for those intellectually unequal to the study of classics and mathematics, no wonder that observant head masters conclude that little good is to be got from it.

As a science master of many years' experience (having been in fact responsible for the introduction of science into *two* of the schools named by Prof. Armstrong as exceptions to the universal indifference), you will perhaps allow me to call attention to the importance of Prof. Armstrong's paper, and to give the conclusions to which my own experience has led me.

The importance of clearly understanding the purpose with which science is to be studied, and the distinction to be borne in mind between the best curriculum for those who are to be professed chemists and those who will not carry the study of chemistry beyond their school-days is obvious; but I wish to point out how entirely science masters are at the mercy of examiners, both of University examiners, periodically examining a school, and of examiners for open scholarships. My own experience is to the point. Fully persuaded of the uselessness of attempting to make an analytical machine out of the ordinary school-boy giving two or three hours a week to chemistry for two or three years, and of the very small amount of education to be obtained from such a course, I endeavoured to model my instruction in practical chemistry much upon the lines adopted by Prof. Armstrong, and exemplified in the appendix to his paper. When the examinations came, it was duly explained to the examiner that the course of instruction adopted had been unusual, but, all the same, the papers set were of the usual kind:—"Analyse the mixture A," "Determine the metals and acids present in the solution B," &c. On such a paper, of course, the boys failed, and a depreciatory report was sent up by the examiners, with the result that the governors of the school thought it their duty to interfere, and request that "more attention should be given to practical chemistry." Consequently my attempt had to be abandoned, and we returned to our "test-tubing."

Scholarship examinations, being presumably of those who will carry the study much further, may more reasonably demand a knowledge of the ordinary methods of analysis, but I am glad to see that a considerable change has taken place in the papers set, and that now the questions proposed are often such as to place the mechanical analyst at a disadvantage, and to encourage the intelligent observation and interpretation of phenomena.

Prof. Armstrong of course writes as a chemist. But there can be no doubt that certain portions of physics are educationally more useful, and it seems to be only the difficulty of arranging practical work in physics which has led to the present state of things, where practical science work in schools means nearly always practical chemistry. But Prof. Armstrong's protest against allowing this to degenerate into "test-tubing" should not be disregarded. There seems also no reason why *elementary* instruction in science—whether chemistry, or botany, or physiography—should not deal *first* with the familiar things of every-day life. I think much more training is to be got by determining, as Prof. Armstrong suggests, the composition of air, the relative combining weights of silver and lead, &c., than by seeing made any number of oxides of nitrogen, and listening to a description of their properties. There is, however, considerable difficulty in arranging easy methods of determining chemical equivalents which, in inexperienced hands, shall give results not *too wide* of the mark.

If a boy gets out the atomic weight of oxygen as 9 when the book says it is 16, or finds the latent heat of steam to be 300 and something when it ought to be 536, he begins to disbelieve in the precision of the statements made, and it is unfortunately impossible for a beginner to make *accurate* determinations of combining weights. Less erratic results can, in fact, be obtained in certain selected physical measurements.

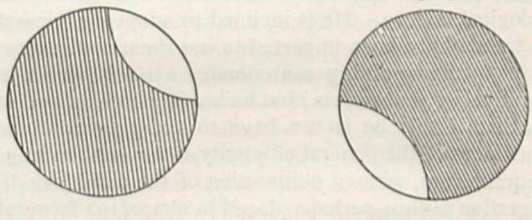
The "bareness" of printed instructions is, as Prof. Armstrong remarks, a distinct advantage to the good student, by compelling him to think for himself, but it is fatal to the unintelligent student, to whom "thinking" is the very hardest work he is called upon to do.

SCIENCE MASTER

The Recent Lunar Eclipse

My object in writing is to confirm in some degree the peculiar appearance of the disk, noticed in your last number (vol. xxx. p. 632). The eclipse was seen here under the most favourable circumstances: the obscuration was so great that the disk could barely be discerned with the naked eye, and the copper colour usually seen was not noticed. Having watched the moon well into the umbra, my attention was diverted for a while, but, on looking again, at 9.35 G.M.T., I was surprised to see a portion of the north-east quadrant pretty strongly illuminated; my attention was again diverted, but on looking a second time at 10.35 G.M.T., I observed a portion of the south-east quadrant

9.35 10.25



illuminated in a somewhat similar manner. At both times the moon was well within the geometrical umbra. But the remarkable feature was that on both occasions the boundaries of the illuminated portions were, approximately, circular, and convex toward the axis of the umbra, indicating that the refracted solar rays producing these illuminations had crossed the axis of the shadow-cone previous to impinging on the lunar disk. The portions of the refracting annulus of the earth's atmosphere concerned in producing these effects were those superincumbent on the Southern Indian Ocean and the North Atlantic.

WENTWORTH ERCK

Shankill, Co. Dublin, November 4

The Sky-Glows

IN using the word "corona" to designate the coloured glare which has accompanied the sun during the past year, I had no intention of employing it in its astronomical sense, but in its ordinary meteorological meaning—which "G. M. H." (NATURE, vol. xxx. p. 633) has overlooked—as referring to the coloured circles on cloud and haze frequently to be seen round the sun and moon, and classed by some observers with halos. By calling the circle now visible round the sun a "corona," I mean that in appearance and probable optical cause it is more like a meteorological corona than like a halo.

May I be allowed to point out a misprint in the first paragraph of my last letter (vol. xxx. p. 633), where it should read "unusual sky phenomena"—the world *universal* having been printed for *unusual*.

T. W. BACKHOUSE

Sunderland, November 8

AFTER sunset this evening there was a peculiar pink flush in the western sky here similar to that which attracted so much attention in England last year. Twenty-five minutes after the sun had gone down, the colour was so vivid as to be reflected from the snows of Mount Baker (10,700 feet), which is about seventy-five miles east of this place. Shortly afterwards it disappeared, but reappeared thirty-five minutes later, prolonging the twilight and making the stars look green, finally dying away very gradually. The weather for the past twelve days has been very wet, and to-night's is the first clear sunset in that time.

Fourteen days ago, when on the Fraser River, eighty miles from here, I saw after sunset a very brilliant aurora borealis. I write this thinking there may be a repetition of the phenomena in England, in which case this note may possess interest.

G. W. LAMPLUGH

Victoria, Vancouver Island, October 13

Peculiar Ice Forms

THE ice structures observed by Mr. Woodd Smith (November 6, p. 5) are evidently the same as were described in vol. xxi. p. 396. I have often seen such fibrous masses since, under circumstances which left no doubt of their being mainly due to prolonged condensation of aqueous vapour from the air; the fibres, white like asbestos, and covered only by a very thin layer of earthy particles, rising from a hard subsoil. The absorption of aqueous vapour by the soil, especially on mountains, seems not yet to be duly appreciated, although it is proved by the many springs issuing at short distances below the summits, and has been insisted upon already in Er. Darwin's "Botan. Garden" and "Phytonomia" (chap. xi. 2). "Rainfall being the source of all water-supply" (NATURE, vol. xxx. p. 375) is a statement hardly to be maintained.

W.

Freiburg, Badenia, November 8

Seismographs—An Apology

I AM just in receipt of the inclosed letter from Mr. Charles A. Stevenson, in which he claims the original idea of the actuating mechanism in the *horizontal component seismograph* I have lately described in these pages, and he includes a copy of his paper to justify his remarks. I therefore think it my duty to offer my apologies to him for not having given him full credit for his invention so far as it goes, although I have *unconsciously* done him wrong. Naples is unfortunately very badly off for modern scientific works and *Proceedings* of Societies, both as regards the National and the University libraries, and as far as I know no copy of Mr. Stevenson's paper exists in the town, except the one he has now sent me.

Perhaps I may be permitted to point out that Mr. Stevenson's seismograph, so far as it is described, would be almost useless for the following reasons:—

(1) The inertia of the upper glass plate would be insufficient not to be affected by the slight movement conducted through the ivory balls to it. This is the reason I use the very heavy lead disk.

(2) No earthquake shock is perfectly horizontal, so that Mr. Stevenson's instrument would only be fit to register the horizontal component of the earth-wave, and would fail to do this, since if the angle of emergence was appreciable it would be jerked up off its supports, and consequently would simply register a series of interrupted lines. This is why I introduced the upper balls and resistance plate.

(3) If the instrument was disturbed by an earth-wave of large amplitude, the registering arm would pass beyond the border of the smoked plate (unless the apparatus was of very great dimensions, so failing to fulfil the conditions of the British Association), where the needle would drop out, or fall so low as to prevent the return of the arm over the plate.

(4) If the earthquake was of some seconds' duration and composed of many varying movements, as is generally if not always the case, a network of irregular curves would remain on the glass that would be quite unintelligible.

If a thing is to be done, it is advisable to do it well, and it is less possible to have accurate registers of earthquake shocks than of the force and direction of the wind, barometric pressure, or any other meteorological phenomena. The requirements of the British Association with regard to expense, size, and portability of seismographs, will not permit anything like an accurate investigation of geodynamics.

In conclusion, should I have overlooked and appropriated the ideas of any other inventor, I shall be happy to fully acknowledge them if sufficient evidence is given (as in the above case) of priority of publication.

H. J. JOHNSTON-LAVIS

November 7

45, *Melville Street, Edinburgh, November* 3

I NOTICED recently in NATURE (vol. xxx. p. 608) an article by you in which you describe a seismograph for recording earthquake shocks, which would appear to be your own invention from reading the paper. No doubt the method of making the record, springs, and upper balls are your own invention, but the *principle* on which the seismograph there described acts is, as far as I know, mine or my father's. I inclose the paper in which it was first described, and I would be glad to learn from you if you forestalled me.

CHARLES A. STEVENSON

Dr. Johnston-Lavis, Naples

Fly-Maggots Feeding on Caterpillars

A FEW months ago I had a caterpillar of *Papilio erythronius*, which I found on a lemon-tree. I put it into a card-box, and fed it daily on lemon-leaves. The box was covered with cloth *tied tightly all round the opening*. After some days, the caterpillar fixed itself to the side of the box, and turned into a chrysalis in the usual way. One day on opening the box, instead of finding the chrysalis changing into its usual colours and markings, it was dark all over. A few days more, on reopening the box, I found six fully-developed cream-coloured maggots at the bottom of the box. I was rather puzzled to conjecture how these maggots got into a box three inches high, with a bit of cloth tied all round the opening. I put the maggots into a little box with some earth under a tumbler. They immediately buried themselves in the earth. In a few days I found six chrysalides, and some days later there were six ordinary house-flies buzzing within the tumbler. I then examined the dark chrysalis of the *P. erythronius*, which was evidently dead, and found it only a *shell*. All its interior had been consumed by the six maggots. It is evident that these maggots in their infant stage had already been in the body of the caterpillar when I boxed it. The latter had gone through its transformation as if nothing was the matter with it, although, if one could have interrogated it, probably it would have complained of mysterious gnawings and creepings in its interior. A time, of course, came when, for want of nerve-centres and other organs, the chrysalis could not go on with its development into the perfect Papilio. The six maggots having had a full meal, found their way out of the Papilio's chrysalis in order to undergo *their* transformation.

I knew that the larvæ of the Ichneumonidæ fed on the live bodies of caterpillars, but I did not know that the larvæ of the house-fly did so also.

E. BONAVIA

Etawah, India, October 18

THE CRYSTALLINE ROCKS OF THE SCOTTISH HIGHLANDS

EVER since the discovery of Silurian fossils in the rocks of North-West Sutherland, it has been recognised that in that region lies the key to the structure of the Scottish Highlands. Accordingly, when in the progress of the Geological Survey, the mapping of the Highlands had to be undertaken, I determined that a detailed survey of the Sutherland ground on the scale of six inches to a mile should be made as a basis for the work. In the summer of last year a surveying party under the charge of Mr. B. N. Peach was stationed there, with instructions to begin by mapping the Durness Basin. This duty was satisfactorily accomplished before the end of the season. The Silurian series of Durness was ascertained to be about 2000 feet thick, and to consist of numerous successive zones, which were traced on the six-inch maps and discriminated in such a way as to be recognisable should they be found to occur in the more complicated region to the east. With this necessary groundwork well established, the Eriboll tract was attacked this summer by Messrs. Peach and Horne. I had never myself had an opportunity of studying the Eriboll sections, which, from the days of Macculloch down to the present time, have been such a fruitful subject of discussion. It was a special injunction to the officers now intrusted with the detailed survey of the region to divest themselves of any prepossessions in favour of published views and to map the actual facts in entire disregard of theory. By the close of this last season the structure of the Eriboll area had likewise been traced upon the six-inch maps, and I then went north to inspect the work. From time to time during the summer, reports had been made to me of the progress of the survey, but, though from the published descriptions of that tract, I was aware that its structure must be singularly complicated, and although apprised of the conclusions to which the surveyors, step by step and almost against their will, had been driven, I was hardly prepared for the extraordinary geological structure which the ground itself presented, or for the great change necessitated in the interpretation of the sections as given by Murchison.

No one cursorily visiting the ground could form any notion of its extraordinary complication, which could only be satisfactorily unravelled by patient detailed mapping such as had never yet been bestowed upon it. With every desire to follow the interpretation of my late chief, I criticised minutely each detail of the work upon the ground; but I found the evidence altogether overwhelming against the upward succession which Murchison believed to exist in Eriboll from the base of the Silurian strata into an upper conformable series of schists and gneisses. The nature of this evidence will be best understood from the subjoined report, which, at my request, Messrs. Peach and Horne have prepared. As the question of the succession of the rocks in the North-West Highlands is still under discussion, I think it right to take the earliest opportunity of making this public declaration. It would require more space than can be given in these pages to do justice to the views of those geologists, from Nicol downwards, by whom Murchison's sections have been criticised, and to show how far the conclusions to which the Geological Survey has been led, have been anticipated. When the official memoirs are published, full reference will be given to the work of previous observers, to which, therefore, no further allusion is made at present.

The most remarkable features in the Eriboll area are the prodigious terrestrial displacements, to which there is certainly no parallel in Britain. Beginning with gentle foldings of the rocks, we trace these becoming increasingly steeper on their western fronts, until they are disrupted and the eastern limb is pushed westwards. By a system of reversed faults, a group of strata is made to cover a great breadth of ground and actually to overlie higher members of the same series. The most extraordinary dislocations, however, are those to which for distinction we have given the name of Thrust-planes. They are strictly reversed faults, but with so low a hade that the rocks on their up-throw side have been, as it were, pushed horizontally forward. The distance to which this horizontal displacement has reached is almost incredible. In Durness, for example, the overlying schists have certainly been thrust westwards across all the other rocks for at least ten miles. In fact, these thrust-planes, but for the clear evidence of such sections as those of Loch Eriboll, could not be distinguished from ordinary stratification-planes, like which they have been plicated, faulted, and denuded. Here and there, as a result of denudation, a portion of one of them appears capping a hill-top. One almost refuses to believe that the little outlier on the summit does not lie normally on the rocks below it, but on a nearly horizontal fault by which it has been moved into its place. Masses of the Archæan gneiss have thus been thrust up through the younger rocks and pushed far over their edges. When a geologist finds vertical beds of gneiss overlying gently inclined sheets of fossiliferous quartzite, shale, and limestone, he may be excused if he begins to wonder whether he himself is not really standing on his head.

The general trend of all these foldings and ruptures is from north-north-east to south-south-west, and the steep westward fronts of the folds show that the terrestrial movement came from east-south-east. Corroborative evidence that this was the direction of the movement is furnished by a series of remarkable internal rearrangements that have been superinduced upon the rocks. Throughout the whole region, in almost every mass of rock, altogether irrespective of its lithological characters and its structure, striated planes may be noticed which are approximately parallel with the thrust-planes, and are covered with a fine parallel lineation, running in a west-north-west and east-south-east direction. These surfaces have evidently been produced by shearing. Again, many of the rocks near the thrust-planes, and for a long way above them, are marked by a peculiar streaked structure which reminds one of the fluxion-lines of an eruptive rock. The coarse pegmatites in the gneiss, for example, as they come within the influence of the shearing, have had their flesh-coloured feldspar and milky-quartz crushed and drawn out into fine parallel laminæ till they assume the aspect of a rhyolite in which fluxion-structure has been exceptionally well developed. The gneiss itself coming into the same powerful mill has acquired a new schistosity parallel with the shearing-planes. Hornblende-rock has been converted into hornblende-schist. Moreover, new minerals have likewise made their appearance along the new divisional planes, and in many cases their longer axes are ranged in the same dominant direction from east-south-east to west-north-west.

Murchison believed that the Silurian quartzites and limestones of Eriboll pass up under, and are conformably overlain by, his upper gneiss. It is quite true that they are so overlain; but the overlying rocks, instead of having been regularly deposited on them, have been pushed over them. What, then, are these overlying rocks? Though they have undergone such intense alteration during the process by which they were moved into their present position that their original characters have been in great measure effaced, lenticular bands occur in them which can certainly be recognised. Some of these bands are unquestionably parts of the Archæan gneiss; others are Silurian quartzite, and in one case we can detect a large mass of the Upper Durness limestone. Traced eastwards, however, the crystalline characters become more and more pronounced until we cannot tell, at least from examination in the field, what the rocks may originally have been. They are now fine flaggy micaceous gneisses and mica-schists, which certainly could not have been developed out of any such Archæan gneiss as is now visible to the west. Whether they consist in part of higher members of the Silurian series in a metamorphic condition remains to be seen. The occurrence of a band of crystalline limestone and calcareous schist, which has been traced for many miles above the great thrust-plane, certainly suggests that it represents the upper part of the calcareous Durness series attenuated and altered by the intense shearing which all the rocks have undergone. This much at least is certain, that the schistose series above the thrust-plane is partly made up of Silurian strata, and has received its present dip and foliation since Silurian time.

Having satisfied myself that Murchison's explanation of the order of sequence could not be established in Eriboll, I was desirous to see again, in the new light now obtained, some of the Ross-shire sections for the description of which I am responsible. Had these sections been planned for the purpose of deception they could not have been more skilfully devised. The parallelism of dip and strike between the Silurian strata and the overlying schists is so complete as to prove the most intimate relationship between them; and no one coming first to this ground would suspect that what appears to be a normal stratigraphical sequence is not really so. But the clear coast-sections of Eriboll, where every dislocation is laid bare, have now taught me that I have been mistaken, for the parallelism in question is not due to conformable deposition. The same kind of evidence of upthrust and metamorphism which these coast-sections reveal can be traced southwards for a distance of more than ninety miles. The task of unravelling the geological structure of these southern regions will be much facilitated by the remarkable persistence of the Sutherland Silurian zones, some of which, with their characteristic features and fossils, are as well marked above Loch Carron as they are at Loch Eriboll.

In south-western Ross-shire the platform on which the Silurian rocks rest is a thick mass of Cambrian red sandstone. In the great upthrow, it is this sandstone platform which has there been pushed over the limestones and quartzites. On the west side of Loch Keeshorn, the red sandstones, in their normal unaltered form, rise up into the colossal pyramids of Applecross; but on the east side, where, at a distance of little more than a mile, they overlie the limestones, they bear so indurated an aspect that they have naturally been classed with the quartzose members of the Silurian series. Traced eastwards they present increasing evidence of intense shearing; fluxion-structure makes its appearance in them, with a development of mica along the divisional planes, until they pass into frilled micaceous schist, in which, however, the original clastic grains are still recognisable. They finally shade upwards into green schists and fine gneiss which merge into coarse gneiss with pegmatite. The short space within which ordinary red feldspathic sandstone and arkose acquire the characters of true schists is a point of some importance in regard to the change from the unaltered Silurian strata of the Southern Uplands into the metamorphic condition of the Highland phyllites, grits, &c.

Obviously the question of chief importance in connection with the structure now ascertained to characterise the North-West Highlands relates to metamorphism. That there is no longer any evidence of a regular conformable passage from fossiliferous Silurian quartzites, shales, and limestones upwards into crystalline schists, which were supposed to be metamorphosed Silurian sediments, must be frankly admitted. But in exchange for this abandoned belief, we are presented with startling new evidence of regional metamorphism on a colossal scale, and are admitted some way into the secret of the processes whereby it has been produced.

From the remarkably constant relation between the dip of the Silurian strata and the inclination of their reversed faults, no matter into what various positions the two structures may have been thrown, it is tolerably clear that these dislocations took place before the strata had been seriously disturbed. The persistent parallelism of the faults and of the prevailing north-easterly strike of the rocks indicates that the faulting and tilting were parts of one continuous process. The same dominant north-easterly strike extends across the whole Highlands, and also over the Silurian tracts of Southern Scotland and the North of England. There is reason to regard it in all these regions as probably due to one great series of terrestrial movements. These must have occurred some time between an early part of the Silurian period and that portion of the Old Red Sandstone period represented by the breccias and conglomerates of the Highlands. In the Central and Eastern Highlands the slates, phyllites, grits, quartzites, and limestones which, along the southern border, are scarcely more altered than their probable equivalents among the Silurian rocks of the Southern Uplands, have been greatly plicated, and have assumed a more or less crystalline structure. But when these changes were brought about, there lay to the north-west a solid ridge of Archæan gneiss and Cambrian sandstone which offered strong resistance to the plication. The thrust from the eastward against this ridge must have been of the most gigantic kind, for huge slices, hundreds of feet in thickness, were shorn off from the quartzites, limestones, red sandstones, and gneiss, and were pushed for miles to the westward. During this process, all the rocks driven forward by it had their original structure more or less completely effaced. New planes, generally parallel with the surfaces of movement, were developed in them, and along these new planes a rearrangement and recrystallisation of mineral constituents took place, resulting in the production of crystalline schists. This metamorphism certainly occurred after early Silurian times, for Cambrian and Lower Silurian strata, as well as Archæan rocks, have been involved in it.

It is obvious that into the problems of Highland geology, always admittedly obscure, a fresh element of difficulty is introduced. At the same time the aid furnished by a minute study of the Sutherland sections is so great that we may hope to attack these problems with more success than has hitherto seemed probable. The work, too, is not of a kind to be attempted in a few hasty scampers over the ground. It will require patient detailed mapping. But when the great base-lines have once been accurately traced, the difficulties will doubtless begin to diminish, and, like the pieces of a puzzle, the various segments of the Highlands will then be found to range themselves in their proper places. ARCH. GEIKIE

Report on the Geology of the North-West of Sutherland

In the north-west of Sutherland the most ancient rocks belong to the Archæan series, and present a great uniformity in lithological characters. They consist mainly of coarse hornblendic gneiss, with distinct zones of gray and pink granitoid gneiss, in which the mica is more abundant than the hornblende. Lenticular veins and bosses of hornblende-rock and hornblende-schist, some at least of which are evidently intrusive, occur in the gneiss, while the presence of small kernels of cleavable hornblende and actinolite forms another characteristic feature of the series. Veins of pink or white pegmatite abound, sometimes parallel with the foliation of the gneiss and sometimes traversing it in all directions. These, however, are distinct from dykes of pink granite, which also intersect the gneiss and coarse pegmatites, and are themselves crossed by later pegmatite-veins. Here and there, indeed, the branches of a pegmatite-vein can be seen to return upon themselves and traverse the main trunk from which they start. Where the Archæan rocks have been recently stripped of their former cover of Silurian quartzite, bands of green epidotic gneiss appear among them, and a soft green mineral with a greasy lustre (agalmatolite ?) is there characteristic of the superficial parts of the pegmatite-veins.

The highly crystalline Archæan rocks are overlain unconformably by a succession of conglomerates, grits, and sandstones, regarded by Murchison as the equivalents of the Cambrian system of Wales. In the course of the work of the Geological Survey in the present region they have been divided into certain zones, which, though they need not be stated here, as they have no bearing on the main question to which this paper is devoted, may prove to be of considerable importance in unravelling the geological structure of the districts further south.

Between the Cambrian sandstones and the overlying quartzites at the base of the Silurian series there is a complete discordance. To the west of the Kyle of Durness, for example, the Cambrian sandstones dip to the north-west, while the overlying quartzites dip to the south of east. Moreover, as the observer passes eastwards to the shores of the Kyle, the Cambrian sandstones are bed after bed transgressed by the quartzites, which eventually rest directly on the Archæan gneiss. The Silurian strata in the Durness area (A in Section) consist of a calcareous series at the top; a middle series, composed partly of calcareous and partly of arenaceous strata; and an arenaceous series at the base. The various sub divisions of the strata are given in descending order in the subjoined tabular statement.

C. CALCAREOUS SERIES ...	VII. DURINE GROUP ...	Fine-grained, light gray limestones, with an occasional dark fossiliferous band.
	VI. CROISAPHUILL GROUP	*c.* Fine-grained, cleaved, lilac-coloured limestones, full of flattened worm-casts; fossils distorted by cleavage. *b.* Alternations of black, dark gray, and white limestone, with an occasional fossiliferous band, like zone (*a*) of this group. *a.* Massive, dark gray limestone, chiefly composed of worm-casts which project above the matrix on weathered surfaces. Near the base are several lines of small chert nodules. This is one of the most highly fossiliferous zones in the Durness Basin.
	V. BALNAKEIL GROUP ...	Alternations of dark and light gray limestone, highly fossiliferous, with occasional impure, argillaceous, unfossiliferous bands. Most of the beds are distinctly cleaved, and contain few worm-casts.
	IV. SANGOMORE GROUP...	Fine granular dolomites, alternating near the top with cream-coloured or pink limestone. Near the base are two or more bands of white chert, one of which is about 5 feet thick.
	III. SAILMHOR GROUP ...	Massive, crystalline-granular, dolomitic limestones, occasionally fossiliferous, charged with dark worm-castings set in a gray matrix; large spheroidal masses of chert near the base. This limestone is locally known as "the Leopard Stone."
	II. EILEAN DUBH GROUP	Fine-grained, white, flaggy, argillaceous limestones and calcareous shales. As yet no fossils have been found in this division.
	I. GHRUDAIDH GROUP ...	Dark leaden-coloured limestones, occasionally mottled, alternating near the top with white limestone. About 30 feet from the base there is a thin band of limestone charged with *Serpulites Maccullochii*, and a similar band occurs at the base.
B. MIDDLE SERIES (partly calcareous and partly arenaceous)...	UPPER ZONE	At the base lies a massive band of quartzite and grit, passing upwards into carious dolomitic grit, crowded in patches with *Serpulites Macculochii*, more especially in the decomposed portions (Serpulite-Grit).
	MIDDLE ZONE	Alternations of brown, flaggy, calcareous, false-bedded grits and quartzites with cleaved shales.
	LOWER ZONE	Calcareous mudstones and dolomitic bands, weathering with a rusty brown colour, traversed by numerous worm-casts, usually flattened, and resembling fucoidal impressions. These beds are often highly cleaved. This and the overlying zone form the "Fucoid-beds" of previous authors.
A. ARENACEOUS SERIES...	UPPER ZONE	Fine-grained quartzites, perforated by vertical worm-casts and burrows becoming more numerous towards the top of the zone ("pipe-rock" of previous authors). These beds pass downwards into massive white quartzites.
	LOWER ZONE	False-bedded flaggy grits and quartzites, composed of grains of quartz and feldspar. At the base there is a thin brecciated conglomerate, varying from a few inches to a few feet in thickness, containing pebbles of the underlying rocks, chiefly of quartz and orthoclase, the largest measuring about 1 inch across.

The Silurian strata in the Durness area are arranged in the form of a basin, truncated on the east side by a fault that brings them against the Archæan gneiss, and thus disconnects them from the Eriboll area, with which they were certainly at one time united. Of the identity of these strata in the two areas there cannot be the smallest doubt. We have recognised them zone by zone, completing the proofs of this identity by detecting in the south and central parts of the Durness Basin the representatives of the middle series, viz. the "Fucoid-beds" and "Serpulite-grit," which had not previously been noted in that area. Though subject to local variations in thickness, these zones are singularly persistent, and, from their marked lithological characters and fossil contents, constitute admirable horizons in unravelling the complicated geological structure of the region. A rich assemblage of fossils has been obtained by the Survey from the various fossiliferous bands indicated in the foregoing table, comprising Trilobites, Annelids, Cephalopods (*Nautilus*, *Lituites*, *Orthoceras*, *Piloceras*, &c.), Heteropods, Gasteropods, Lamellibranchs, Brachiopods, Corals, Sponges, and Foraminifera. As yet this collection has not been examined in detail, but from observations in the field it is probable that some of the limestone zones will be found to be characterised by particular fossils.

A striking feature of the Durness Basin is the amount of displacement of the strata by faults; indeed, this feature is so characteristic of the highest limestone zones that it is difficult satisfactorily to compute their thickness. But from the base of the quartzite to the top of the calcareous series the total thickness of Silurian strata cannot be less than 2000 feet. In Sangomore Bay, near the village of Durine, the highest limestone zone is overlain by shattery quartzite, striped fissile schist, frilled schists, and gneiss. Though unquestionably resting upon the limestone and sharing in the normal faulting of the district, these crystalline strata do not prove a conformable upward succession, as has been naturally enough supposed. The key to the reading of this and of the corresponding section at Farrid Head is to be sought in the Eriboll tract.

The Silurian rocks of the Durness Basin are separated from those of Loch Eriboll (B in the Section) by a prominent ridge of Archæan gneiss, the eastern slope of which is covered by a cake of quartzite. Along the crest of the chain the basal breccia is exposed, overlain by the lower zone of false-bedded grits (No. 3) and the upper zone of "pipe-rock" (No. 4 in Section). As the eastern declivity of the ridge is greater than the dip of the quartzites, the observer, on descending the slope, crosses the basset edges and dip-slopes of the latter strata, and eventually finds himself again on the old platform of Archæan gneiss exposed by denudation (see Section). Both the zones of quartzite are then once more met with in their normal order, the highest beds exposed on the western shore of Loch Eriboll belonging to the horizon of the "pipe-rock." On the eastern shore, at Ant Sron, on the crest of a low anticlinal arch of the "pipe-rock," there is an excellent section of the middle series between the quartzites and the limestones. The two subdivisions of the "Fucoid-beds" (No. 5) and the "Serpulite-grit" (No. 6 in Section), which are typically developed at that locality, pass underneath the Serpulite-limestone at the base of Group I., exactly as they do at Durness. The dark leaden-gray limestones of the lowest group (I.) are then rapidly succeeded by flaggy limestones (Ant Sron, Chorrie Island, Heilim) and dolomitic limestones which, probably the equivalents of the Eilean Dubh Group in Durness, are the highest members of the series here represented (No. 7 in Section). A careful search among the Eriboll limestones has failed to bring to light any organic remains save *Serpulites* and certain minute spherical bodies which may prove to be Foraminifera. A similar dearth of fossils characterises the two lowest zones in Durness, so that this feature is common to both areas. The non-occurrence of the higher fossiliferous limestones in Eriboll may be accounted for by the remarkable geological structure of that region which is now to be described.

As the observer passes eastwards along the magnificent quartzite sections on Crag Dionard and Conamheall, south of Loch Eriboll, he cannot fail to note the numerous flexures of the strata, and especially the peculiar type of their sharp anticlinal folds. As a rule, the eastern limb of each of these folds dips at a gentle angle to the south-east, while the west limb is highly inclined, vertical, even inverted, or sometimes broken by a reversed fault the effect of which is to bring lower over higher beds. These reversed faults (*t t t* in Section) become more numerous eastwards. They are admirably displayed both in ground-plan and dip-section on the shore north of Heilim, where they repeat the various zones ranging from the "pipe-rock" to the Eilan Dubh limestone (Group II.). The strike of the reversed faults ranges on the whole with that of the strata traversed, and their hade is inclined at a higher angle than the dip of the latter, the difference generally amounting to about 10°. Inland from the coast-section, north of Heilim, to the base of Ben Arnaboll, the zones just mentioned are constantly repeated by a complicated system of reversed faults and folds, the general inclination of the strata being towards the south-east. As that hill is approached, the displacement produced by these faults increases in amount; hence the observer meets with beds occupying a lower geological horizon the further east he travels. At length this intricate system of faults and folds culminates in a great dislocation which, for convenience

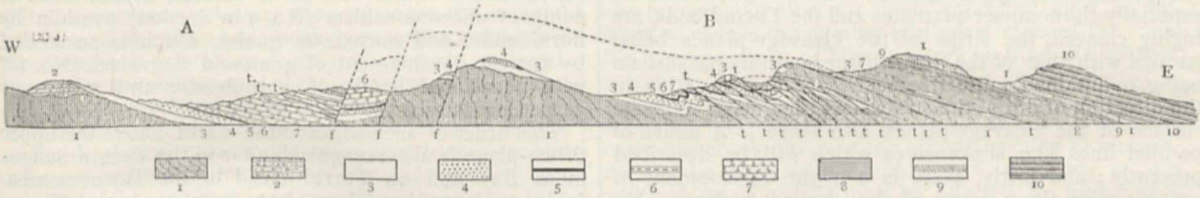


DIAGRAM-SECTION OF DURNESS-ERIBOLL REGION.

10. Gneissose Flagstones, &c. ⎫ 9. Siliceous Schist ... ... ⎬ NEWER GNEISS 8. {Frilled Schist ... ... / Green Schist ... ...} ⎭	7. Durness-Eriboll Limestone—Upper Series 6. Serpulite-Grit ... ... } Middle ,, 5. Fucoid-Beds ... ... 4. Piped Quartzite ... ... } Lower ,, 3. {False-bedded Quartzite ... / Basal Breccia ... ...} LOWER SILURIAN SERIES	2. Sandstones and Conglomerates—"CAMBRIAN" 1. Gneiss, Granite, Pegmatite, &c. } ARCHÆAN GNEISS

A = Durness Area; B = Eriboll Area; ff = Normal Faults; t t = Reversed Faults and "Thrust-planes"; Dotted lines = continuation of normal faults and thrust-planes.

of description, and to distinguish it from the ordinary reversed faults, may be termed a *Thrust-plane*. By means of it a great mass of coarsely crystalline gneiss with pegmatite-veins, in places upwards of 400 feet thick, is placed above the Silurian rocks (see Section). A careful examination of the mass which caps Ben Arnaboll clearly proves that *it rests transgressively on all the zones of the Silurian rocks, from the lower zone of the quartzites (false-bedded grits) up to the limestone which overlies the Serpulite-grit.* Owing partly to its low hade and partly to subsequent folding, the outcrop of this thrust-plane resembles that of an ordinary overlying formation cut into a sinuous line by denudation. It is admirably seen in dip-section on the east and north slopes of Ben Arnaboll, whence it can be followed round the west face of the hill, descending into the valley on the west, then bending back on itself, winding round the north slope of Druim Tungi, and entering Loch Eriboll in Heilim Bay. It reappears at the base of Crag-na-Faolinn, and has been traced still farther to the south, while northwards it can be followed to the Whitten Head, at the mouth of Loch Eriboll.

That the gneiss thus brought up on Ben Arnaboll and elsewhere is in reality the Archæan gneiss is evident, for two reasons. First, its lithological characters agree with those of the typical Archæan area to the west, save in certain cases where the original features have been effaced by the crushing to be afterwards described. Near the thrust-plane, this effacement is complete, but in the heart of the mass the normal characters of the Archæan rocks, including in some instances their characteristic north-west strike, are retained. The rocks consist of coarsely crystalline hornblendic gneiss and pink granitoid gneiss, with lenticular veins of hornblende-rock and kernels of cleavable hornblende, while massive veins of pink pegmatite are well developed. The soft greenish mineral (agalmatolite?) already mentioned as characteristic of the gneiss, where now or lately covered with quartzite, occurs here in the pegmatites, and veins of epidosite are abundant. Second, at various localities the brecciated conglomerate and false-bedded quartzite at the base of the Silurian strata are found resting on these crystalline rocks. Further, the unconformable junction can on one line be traced continuously for more than a mile. There can be no doubt, therefore, that this mass is really a fragment of the old platform of Archæan rocks on which the Silurian strata were deposited.

The occurrence of this Archæan gneiss in its present position above much younger rocks is doubtless to be ascribed to the same cause which elsewhere has resulted in foldings of the strata. In the present instance we see an attempt, as it were, to establish an anticlinal fold of the type already described as occurring to the west, with a steep westward and gentle eastward slope. The west limb however has here given way along a great dislocation or reversed fault, while the eastern limb has been driven forwards until the Archæan rocks have been carried over the truncated edges of the Silurian strata. The vertical beds of the basal quartzites are still to be seen on the west limb of the anticline on Ben Arnaboll, Crag-na-Faolinn, and on Whitten Head (see Section). The quartzites on Druim Tungi, and indeed all the Silurian strata on the east side of Loch Eriboll, between Heilim and Crag-na-Faolinn, form part of a syncline which has been pushed westwards in front of the anticline along this thrust-plane. This structure explains the origin of the inversion of the Silurian rocks along the junction line east of Camas-an-Duin, and the occurrence of the lower limestone groups in a shallow trough at Eriboll. Of special interest is the occurrence of a small outlier of Archæan gneiss on the crest of a hill (Sithean-na-Cuag) north-west of Ben Arnaboll. This mass rests on the Fucoid-shales, Serpulite-grit, and limestone. Though now isolated by denudation, it was obviously originally continuous with the mass on Ben Arnaboll, and it thus furnishes striking proof of the westward extension of displaced gneiss, and of the thrust-plane on which it travelled.

The effects of this great movement on the Silurian strata which have been over-ridden by the gneiss are somewhat remarkable. The pipes or vertical worm-tubes in the quartzites have been flattened, drawn out, and bent over in a direction perpendicular to the strike of the thrust-plane. The false-bedded grits and quartzites present a streaky appearance resembling fluxion-structure, due to the elongation of the fragments of orthoclase-feldspar and the quartz grains. The fine-grained rocks, especially the compact quartzites and the Fucoid-beds, are highly cleaved, the strike of the cleavage-planes being parallel with that of the thrust-plane, and this parallelism being maintained quite irrespective of any variation in the direction of dip of the strata next the gneiss. On the surface of the cleavage-planes also there is a series of parallel lines like slickensides which will be described presently; and lastly, there is a slight development of sericitic mica along many of the cleaved surfaces. No less important is the alteration produced on the overlying Archæan gneiss. In the heart of the mass, as already stated, there is little apparent change, but near the thrust-plane the beds are drawn in towards it till their strike roughly coincides with that of the thrust-plane. The inclosed hornblende merges into a green chloritoid product, the hornblendic gneiss has been converted into a fine green schistose rock, while the quartz and feldspar of the pegmatites have been drawn out into streaky or wavy lines, so as to assume somewhat the appearance of rhyolites. Finally a new set of divisional planes has been superinduced on the mass, the strike of which is parallel with that of the plane of thrust.

Again, there is clear evidence to show that the thrust-plane just described was followed by minor movements of a similar nature in the gneiss itself, whereby different portions of the mass were made to slip over each other. Occasionally a thin lenticular mass of the bottom-quartzite has been caught in these planes of disruption.

But all these evidences of displacement are merely the precursors of a still more powerful thrust-plane, which has been traced continuously from the shore east of Whitten Head to the crest of Crag-na-Faolinn, and at intervals for many miles to the southward into the Assynt country. The strike of the strata overlying this plane is, on the whole, north-north-east and south-south-west, with a general east-south-easterly dip, usually at comparatively low angles. Though roughly parallel with it, this greater thrust-plane here and there overrides the lower or more westerly one, for the rocks on its upper side may be seen to pass across all the zones of the Silurian series up to the limestones. A recognisable and tolerably persistent order of succession has been observed in the rocks on the upper side of this thrust-plane. At the base, and resting on different platforms, there usually lies a belt of striped fissile schist, followed by green schist with alternations of gneiss, which, though it has lost nearly all trace of its original foliation, is probably a portion of the Archæan gneiss. A number of lenticular masses of Silurian quartzite occur on this horizon between the Whitten Head coast and Crag-na-Faolinn. In some cases, the basal breccia and portions of the overlying false-bedded grits are clearly seen resting on the Archæan rock, the planes of foliation of the gneiss being roughly parallel with the bedding of the quartzites. On closer examination, however, it is observable that successive folia of the gneiss impinge against the basal breccia. In other cases, wedges of the false-bedded grits, without the basal breccia, are caught between two thrust-planes. Perhaps the most remarkable example of these isolated masses of Silurian rocks, is the limestone intercalated among the green schists, on the hill-slope above Eriboll House. This mass appears to belong to one of the higher limestones of the Durness Basin which have not elsewhere been noticed in the Eriboll area. It lies not far above the great thrust-plane, and though its relations to the schists are not as well shown as could be desired, its presence here is evidently due to the same series of movements that brought in the intercalations of quartzite. Passing eastwards we find, next in order, a belt of frilled green schists (No. 8 in Section) with a well-marked calcareous zone near the top, which has been traced from the shore east of Whitten Head for a distance of ten miles in a south-west direction, and which extends still further to the south. To these succeed a thin band of compact siliceous schists (No. 9 in Section) overlain by hornblendic and micaceous gneiss, which is succeeded by a great development of gneissoid flagstones (No. 10) with occasional bands of hornblendic and micaceous garnetiferous schists.

This order of succession in the rocks above the upper thrust-plane is also recognisable far to the west in Sangomore Bay and on Farrid Head in the Durness area. It is evident that there has been an extraordinary amount of movement of these rocks along the upper thrust-plane, since they override all the other rocks pushed forward by the lower thrust-planes in the Eriboll area, and rest directly on the limestones of the Durness Basin. The thin band of shattery quartzite between the striped fissile schist and the limestone in Sangomore Bay is a fragment of the false-bedded quartzite zone which has been pushed forward along the surface of the thrust-plane,—a characteristic feature of the thrust-planes in Eriboll.

The microscopic characters of the rocks from the different zones above the upper thrust-plane have yet to be studied. Much fresh light may thence be expected on the *modus operandi* of the processes involved in the extraordinary lithological changes which the rocks have undergone. Meanwhile a careful examination of the various masses in the field points very clearly to the nature of these processes. The striped green fissile schist which occurs along the thrust-plane presents an exceedingly compact texture with a remarkable streaked structure which at once recalls the fluxion-lines of an eruptive rock. Still more conspicuously is this structure displayed by the masses of pegmatite in the gneiss; they lose their ordinary character and assume more that of rhyolite. The intercalations of quartzite are marked likewise by the same streaked appearance, their component particles of quartz and feldspar being all elongated in one common direction. The gneiss associated with the schists above the thrust-plane, though its original foliation can still in places be detected, has had a new set of schistose planes superinduced in it which are on the whole parallel with the thrust-plane. Bands of hornblendic gneiss merge into hornblende-schist and chlorite-schist, and these again into finely-frilled schists. All these new structures, which are quite independent of the original bedding or foliation of the rocks, were obviously connected with the production of the great thrust-plane, with which they lie more or less parallel. They point to enormous mechanical movements under which, as the rocks sheared, the individual particles were forced over each other in one common direction, viz. from east-south-east to west-north-west. Further evidence of this mechanical movement is supplied by certain abundant fine parallel lines, like those of slickensides, which occur almost everywhere on the foliation-surfaces or other parallel divisional planes. They are especially well developed among the striped fissile schists and the gneissose flag-stones. These lines run in the same general direction already mentioned (E.S.E. to W.N.W.). In many cases it may be observed that the component particles of the rocks are oriented in this same direction, while original quartz-veins are drawn out into parallel rods. Another important feature connected with these rocks is the development of minerals along the new planes of schistosity. In particular, the abundance of sericite mica is noteworthy, the longer axes of the crystals of which lie in a direction parallel with the slickenside-lines. Other micas, hornblende, actinolite, and garnets have also made their appearance along the same planes. This re-crystallisation becomes more pronounced the further east one advances from the outcrop, or passes upwards from the great thrust-plane.

This accumulated evidence points to the conclusion that in the north-west of Sutherland the rocks have been powerfully affected by one grand series of terrestrial movements whereby new structures have been super-induced upon them. Among these changes the original characters of the rocks have been more or less completely effaced, and new crystalline structures have been produced. Although a normal upward succession from the Silurian strata into an overlying series of schists cannot be maintained in the north of Sutherland, it is nevertheless certain that the displacements and metamorphism here described are later than Lower Silurian time. It is also evident that these great changes had been completed before the time of the Lower Old Red Sandstone, the conglomerates and breccias of which rest upon and are made up of fragments of the crystalline schists.

One final feature of the Durness and Eriboll area remains to be noticed. The geological structure of this region has been further complicated by the subsequent folding of the strata, and by a double system of normal faults (*ff* in Section) which affect the strata and thrust-planes alike. One set of normal faults trends north-north-east and south-south-west, while another, which appears to be newer, trends more or less at right angles to these. By these two systems of later dislocations, the thrust-planes with their low hade have been intersected and shifted precisely as if they had been ordinary boundary-planes between two geological formations. Much of the difficulty, indeed, which has been from the first experienced in unravelling the complicated structure of this region may be traced to the effect of the intricate network of reversed and normal faulting. The very preservation of the Durness Basin is due to two normal step-faults, one of which lets down the quartzites more than 1200 feet, while the other brings the whole Silurian Basin down to the sea-level.

B. N. PEACH
JOHN HORNE

THE GENESIS OF AN IDEA, OR STORY OF A DISCOVERY RELATING TO EQUATIONS IN MULTIPLE QUANTITY

I VENTURE, even at the risk of appearing egoistical, to call the attention of a wider circle of English mathematical readers than are likely to notice it in the pages of the *Comptes Rendus*, to what appears to me a remarkable discovery in the theory of matrices, or, in other words, of multiple quantity which has lately presented itself to me. It seems to me the more necessary to do so because the nature of the theorem which constitutes the discovery would hardly be suspected from the leading title of the note in the *Comptes Rendus* in which it is contained, being indeed an after-thought, so that the sting of the paper has to be sought for in its tail.

Hamilton, of immortal memory, has given, in his "Lectures on Quaternions," a solution of a certain quadratic equation in *quaternions*, those algebraical entities which (building upon a suggestion in Prof. Cayley's ever-memorable paper[1] on matrices, in the *Philosophical Transactions* for 1858 or thereabouts) I have, with the general concurrence of all who have given attention to the subject, found means of identifying with binary matrices or algebraical quantities of the second order, and thus succeeded in determining the True Place of Quaternions in Nature. Now, what Hamilton has done for an equation of the second degree of quantities of the second order, the theorem in question effects in a much more simple and complete manner for a similar sort of equation of any degree and relating to quantities of any order.

The history of the discovery in question constitutes in itself, it seems to me, an interesting chapter in Heuristic. This is how it came about. Hamilton's equation admits of six solutions or roots, which arrange themselves naturally in three pairs, and stand in immediate, and what we algebraists call rational relation to the three roots of a cubic equation, or rather to the six square roots of those three roots. From this it follows immediately that one single condition must be sufficient to reduce the number of distinct roots of the equation in quaternions or binary matrices from six to four, inasmuch as, when two roots of the cubic referred to become equal, two *pairs* of roots of the original equation must coincide. It naturally therefore became an object of interest to obtain the quantity which, equated to zero, expresses the condition of equality of two roots of this cubic, which of course may be effected by means of a well-known formula for finding the discriminant of a cubic equation; but the quantity so obtained directly from the cubic is of an exceedingly complex form, and leaves the mind entirely unsatisfied as to its true internal composition, just as from a handful of diamond dust it would be impossible to infer the crystalline form which constitutes the true idea, the *raison* or *façon d'être* of the glittering gem.

Again and again my mind reverted fruitlessly to the subject until, on September 28 last, pacing the deck of the splendid Dover and Calais boat, the *Invicta*, under the vivifying and genial rays of a bright and benignant sun, the idea occurred to me that the form to be determined must be subject to satisfy a certain partial differential equation, and without the aid of pen or pencil I succeeded, ere the voyage was half over, in identifying the discriminant of the cubic with that of a biquadratic of the simplest imaginable constitution possible: in technical language, supposing $px^2 + qx + r = 0$ to be the equation in question, I discovered virtually that the desired discriminant is identical with that of the biquadratic form which is the determinant of the binary matrix (or the tensor squared of the quaternion) $px^2 + qx + r$ treated as if x were an ordinary quantity. Starting from this point it was easy to infer all the possible cases of equality which could occur between the six roots; and, more than that, to classify under thirteen classes all the principal cases that could present themselves in the solution of the equation, not merely for the general case when there are six actual and determinate roots, but even for those cases when some of the roots pass off into infinity and become conceptual instead of actual, or else, without passing to infinity, remain actual but contain arbitrary constants.

This more-than-anticipated complete solution of the problem before me was in part suggested by the opening

[1] This paper constitutes a second birth of Algebra, its *avatar* in a new and glorified form. See introduction to "Lectures on Universal Algebra" in the sixth volume of the *American Mathematical Journal*.

lines of a memoir by M. Darboux on the solution of a biquadratic equation in Liouville's journal, with which its eminent author, my colleague in the Institute of France, providentially presented me shortly after my arrival in Paris, and which led me to see that the three pairs of solutions of the Hamiltonian equation must stand in immediate conceptual relation to the three pairs of sides of the complete quadrangle formed by a certain conic related to the form $px^2 + qx + r$ (in fact the determinant of the matrix $pu + qv + rw$) with the fixed conic $v^2 - uw$.

Now comes the turning point, the ἀναγνώρισις of this strange eventful history.

> "There's a Divinity that shapes our ends,
> Rough-hew them how we will."[1]

Seized with a sudden and fortunate attack of bronchitis, which confined me to my bed, and in the access of nocturnal fever which that state induces, my thoughts reverted with increased activity to this geometrical figure. It became clear to my inner sense that there ought to be an immediate relation between the biquadratic determinant of the form $px^2 + qx + r$, spoken of above, and the three pairs of its roots, and seizing my courage with both hands, I made bold to declare to myself that the functional parts of the six *identical equations* to the six roots ought to be the three pairs of conjugate quadratic factors of the biquadratic in question.

But if this should turn out to be true, it became impossible not to suspect, or even more than half believe, that an analogous statement must admit of being made for a unilateral equation (*i.e.* one in which, as in Hamilton's, the multipliers of each power of the unknown matrix x lie all on the same side (whether to the right or left) of it) whatever might be the degree of the equation, and whatever the order of the matrices concerned. In other words, supposing $fx = 0$ to be such equation, and $\phi x = 0$ to be the identical equation to any one of its roots, ϕx ought to be contained as an algebraical factor in the determinant of the matrix fx when, for the moment, x therein is regarded as an ordinary quantity. If this were so, then the reciprocal theorem would necessarily be true (on account of the determinant referred to being in general irreducible), viz. that, supposing ω to be the order of the matrices concerned, every algebraical divisor of it, say ϕx, of the degree ω, must be the identically-zero function to one or the other of the matrices x which satisfy the equation $fx = 0$, and consequently it would be only necessary to combine, according to a well-known method of elimination, the given form fx with each in succession of the derived forms, which constitute a brood or litter as it were, issuing "de son propre sein," to obtain all the roots of fx by solving the ordinary algebraical equation det. $(fx) = 0$, and that thus the solution of the unilateral equation would depend on the solution of an ordinary equation of the degree $n\omega$, n being the degree of f in x, and ω the order of the matrices concerned: the number of the roots of fx would therefore be the number of ways of combining $n\omega$ things ω and ω together, *i.e.* $\frac{\Pi(n\omega)}{\Pi\omega\,\Pi(n-1)\omega}$. But herein arose a self-created difficulty, a phantasmal projection of my own brain, to block up the way, and throw doubt and discredit on all that precedes. Supposing $\omega = 2$, the number of roots thus ascertained would be $\frac{2n(2n-1)}{2}$, or $2n^2 - n$, and for $n = 3$ would be 15. Now, in the *London and Edinburgh Philosophical Magazine* for May last, whilst I had shown that $2n^2 - n$ is the number of roots of a unilateral equation in quaternions of the degree n, and of the trinomial or Jerrardian form, I thought I had proved the number of solutions of a *complete* cubic equation in quaternions to be 21 (upon which I based the formula $n(n^2 - n + 1)$ for a unilateral equation of quaternions of the degree n. There was then the choice to be made—to abandon the conjectural theorem, or to admit an error in the supposed determination of the number 21. I felt no hesitation in making my election, especially as there was a loop-hole for error in such numerical determination, inasmuch as no actual arithmetical calculations had been made, but the order of a certain system of equations which ought to be equal to the number of roots of fx was inferred from calculations in which all numerical quantities were left in blank; it was therefore quite possible (however unexpected the fact) that some of the leading coefficients of the resolving equation of the degree 21 might become zero,[1] and consequently that the order might fall below (although it could not rise above) that number. To my gratified surprise my faith met with its reward, for I soon found an easy proof of the remarkable theorem which I have ventured, in emulation of a phrase of Cauchy, to call a "*pulcherrima regula*," which will appear in the number of the *Comptes Rendus* next forthcoming after this date, and which may be summed up approximately in the following words:—*Every latent root of every root of a given unilateral function in matrices of any order, is an algebraical root of the determinant of that function taken as if the unknown were an ordinary quantity, and conversely every algebraical root of the determinant so taken is a latent root of one of the roots of the given function.*[2] This constitutes a marvellous extension (to a matrix implicitly given by a unilateral equation) of the already no-little-marvellous Hamilton-Cayley theorem of the identical equation to a matrix given explicitly.

My good genius met me on the deck of the *Invicta*, and only left me three weeks later on board the returning steamer from Boulogne. There my pleasing algebraical dream came to an end.[3] J. J. SYLVESTER

New College, Oxford, October 26

[1] It is one of Descartes' "self-evident primary truths" that nothing which has happened could not have happened otherwise.

OUR FUTURE WATCHES AND CLOCKS

HOWEVER long the use of the letters "a.m." and "p.m." for distinguishing the two halves of the civil day may survive, it seems probable that the more rational method of counting the hours of the day continuously from midnight through twenty-four hours to the midnight following may before long come into use for a variety of purposes for which it is well adapted, even if it should not yet be generally employed. It seems proper, therefore, to consider in what way ordinary watches and clocks could be best accommodated to such a change in the mode of reckoning. To place twenty-four hours on one circle round the dial instead of twelve hours as at present seems the most natural change to make; but, in addition to a new dial, it would involve also some alteration in construction, since the hour hand would have to make one revolution only in the twenty-four hours instead of two. And there would be this further disadvantage, that the hours being more crowded together, the angular motion of the hand in moving through the space corresponding to one hour would be less—in fact, one-half of its present amount. It is to be remembered that, in taking time from a clock, persons probably pay small attention to the figures, either those for hours

[1] Or else that its functional part might be composite and throw off an irrelevant factor.

[2] In terms more precise as regards the *converse* the theorem runs as follows:—*The identically-zero function to a root of* f x *is a factor of the determinant to* f x, *and conversely every factor of that determinant of degree equal to the order of* x *is identically zero.*

[3] A letter just received from M. Hermite informs me that M. Poincarré, in a paper presented by him to the Institute on Monday last, takes up the wondrous tale of multiple quantity so largely treated of by me in recent articles in the *Comptes Rendus*. The subject could not be in better hands. The ball is served, and the most skilful and practised players—the Cayleys, the Lipschitzes, the Poincarrés, the Weyrs, the Buckheims (and who knows how many more?)—stand round ready to receive it, and keep it flying in the air.—November 8.

or minutes, the relative position of the hands on the dial probably at once sufficiently indicating the time to most persons without any need of reference thereto, but it would be by no means so easy to pick up the hour from a circle containing twenty-four, and especially in the case of public and turret clocks. There is also the question of change of the motion-work to which allusion has been already made—necessary if the hour-hand is to make one revolution only in twenty-four hours—a practical question in regard to which the watch- or clock-maker could probably best speak.

There is another way of adapting ordinary watches and clocks to the twenty-four hour system, which, if the watch is intended only for the reckoning of local time, seems deserving of consideration. It consists in making the hour figures shorter, not necessarily at all less distinct, and placing two circles of figures round the dial, an inner circle with hours from 0 to 11, and an outer circle with hours from 12 to 23. The hour-hand would thus point to 1 and 13 and to 2 and 14, &c., at the same time, it being understood that the hours 0, 1, 2, &c., would be reckoned in the morning, and the hours 12, 13, 14, &c., in the afternoon, a convention to which people would probably soon accommodate themselves. On such a plan a watch would only require a new dial, no change of wheelwork being necessary, so that it could be very readily applied to existing watches, and so sooner promote the use of the twenty-four hour system. Persons might perhaps object to the introduction of two hour-circles from an artistic point of view. But, after all is said, the question whether one circle containing twenty-four hours, or two circles having twelve hours in each, be preferable, is one to be settled only by a consideration of the relative advantages and disadvantages of the two proposals, in regard to which it would be interesting to learn what business men and others on the one hand, and practical watchmakers on the other, may have to say. There are conditions under which the one circle of twenty-four hours would certainly be the more advantageous, and clearly it would be well that one system only should if possible be used.

As regards clocks, there is the further question of striking the hours. For public clocks we could not go on to twenty-four. It may be a question whether in large towns one stroke only at each hour might not be a sufficient indication, though even this rule probably could hardly be universally applied.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

AT a meeting of the General Committee of the British Association held at the Royal Institution on the 11th instant, Sir Lyon Playfair was elected President for the meeting at Aberdeen next year. It was resolved to request the following to accept the office of Vice-President for that meeting:—The Duke of Argyll, the Duke of Richmond and Gordon, the Earl of Aberdeen, the Earl of Crawford and Balcarres, Sir William Thomson, James Matthews, Lord Provost of Aberdeen, Dr. Alexander Bain, Lord Rector of the University of Aberdeen, the Very Rev. Principal Pirie, and Prof. W. H. Flower. The following were elected Local Secretaries: Prof. G. Pirie, Dr. Angus Fraser, and Mr. J. W. Crombie; Local Treasurers: Messrs. Robert Lumsden and John Findlater. The following appointments were also made:—General Treasurer: Prof. A. W. Williamson, Ph.D., F.R.S.; General Secretaries: Capt. Douglas Galton, C.B., F.R.S., and A. G. Vernon Harcourt, F.R.S.; Secretary: Prof. Bonney, D.Sc., F.R.S.; Ordinary Members of the Council: Capt. W. de W. Abney, Prof. W. G. Adams, Prof. R. S. Ball, J. F. La Trobe Bateman, Sir F. J. Bramwell, Prof. W. Boyd Dawkins, Dr. Warren De La Rue, Prof. J. Dewar, Capt. Sir F. J. Evans, Prof. W. H. Flower, Dr. J. H. Gladstone, J. W. L. Glaisher, Lieut.-Col. H. H. Godwin-Austen, J. Clarke Hawkshaw, Prof. O. Henrici, Prof. T McK. Hughes, Dr. J. Gwyn Jeffreys, Prof. H. N. Moseley, Admiral Sir E. Ommanney, W. Pengelly, Dr. W. H. Perkin, Prof. Prestwich, the Right Hon. George Sclater-Booth, Dr. H. C. Sorby, Sir R. Temple; Auditors: John Evans, D.C.L., Treas.R.S., Dr. Huggins, F.R.S., and George Griffith, M.A.

Invitations for the year 1886 were received from Birmingham, Bournemouth, and Manchester, and after a discussion (in which the representatives of Manchester expressed their willingness to withdraw in favour of Birmingham for the year 1886, but their earnest hope that the Association would not fail to visit them in 1887), it was agreed, *nem con.*, to accept the invitation from the town of Birmingham for the year 1886.

The report of the Council relating to the rules concerning the representation of local scientific societies at the meetings of the Association and the establishment of a Permanent Committee as a means of union between them and the Association were sanctioned, and it was resolved in accordance with a recommendation from the Council to present the die for the medal which is about to be founded at McGill University, Montreal, in commemoration of the visit of the Association to Montreal.

THE NEW VOLCANIC ISLAND OFF ICELAND

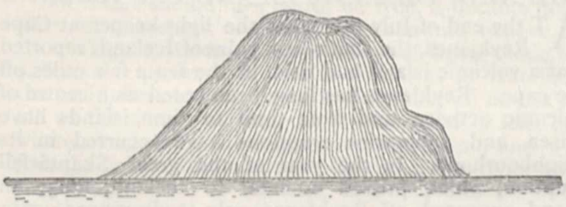
AT the end of July this year the light-keeper at Cape Reykjanes, the south-west point of Iceland, reported that a volcanic island had risen in the sea a few miles off the cape. Reykjanes has long been noted as a centre of volcanic activity, and from time to time islands have arisen and submarine eruptions have occurred in its neighbourhood. In the year of the great Skaptárfell eruption, which proved so fatal to Iceland, 1783, an island appeared off Reykjanes, only to disappear again after a very brief existence. Only a year or two ago an eruption of considerable violence occurred in the sea, not far from the spot where the new island appeared. Columns of steam and clouds of dust, mingled with occasional glowing masses of fused rock, were seen to rise out of the sea, and large quantities of pumice were thrown up and drifted ashore on the neighbouring coast.

Being desirous to learn as much as possible about the new island, I visited Reykjanes on September 9. The cape, like the greater part of the surrounding district, is entirely covered with lava; not far from the sea lie a number of boiling pools of considerable size, from whose agitated muddy waters arise the columns of steam that give the cape its name, Reykjanes (Smoking Cape); over a large area surrounding the pools the earth is perforated by steam jets and small mud boilers, and the traveller must pass warily over its treacherous surface, for under the thin and yielding upper crust lie beds of soft many-coloured clays, boiling hot, permeated by steam and mixed with sulphur. On a projecting cliff about 150 feet high stands the lighthouse, a low octagonal stone house, and from the point a line of islands, four in number, runs out to the south-west, the nearest being about seven miles, and the farthest about sixteen miles, from the cape. Of these only the nearest two, Eldey or Melsækken (the Meal-sack, so called from the guano deposits that whiten the top of its bleak cylindrical mass), and Eldeyjardrangur, are usually visible from the lighthouse. The farther two, Geirfuglasker and Geirfugladrangur, are chiefly interesting as having been formerly frequented by the Great Auk or Gare-fowl (*Alca impennis*), now apparently extinct.

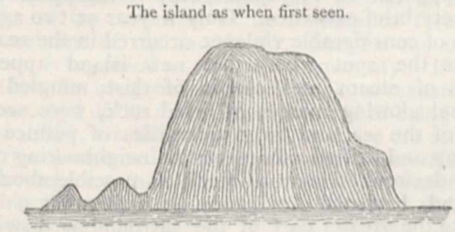
When I reached Reykjanes, rain and mist obscured the sea, Eldey could only with difficulty be seen, and the new island was quite invisible. I waited patiently for better weather, employing the time in examining the boiling springs and hot clay-beds, which are similar to those of the Krísuvík sulphur mines, and the so-called "porcelain rock," a bed of very pure, white, and compact siliceous sinter or geyserite, deposited by some long extinct boiling spring. It was not till the afternoon of the next day that the weather cleared up a little, and a long and patient watch from the top of a hill behind the lighthouse was at length rewarded by the discovery of a dim spot on the horizon, which close observation through a good telescope showed to be the new island. It was quite invisible to the naked eye, but the light-keeper assured me that he had often seen it in clear weather, without a glass. When first seen, on July 29, its shape was that of a truncated cone with a slight depression on the top, and a considerable hollow half way down the slope on the north side.

On August 5 and 6 a series of violent earthquake shocks occurred, which shook and split the masonry of the lighthouse and damaged the lamps. For several days the new island was obscured by mist and rain, and when it again became visible its shape was considerably altered: a large part of the slope on the south side had slipped down into the sea, where it now lies, forming two little hillocks close to the foot of the main mass, and leaving a steep face nearly perpendicular towards the bottom. On the north side there is shoal water extending some distance from the island. The length of the island is about one-

The New Volcanic Island off Reykjanes, Iceland.



The island as when first seen.



The island as it now appears.

third greater than its height. It lies nearly west-south-west of Reykjanes, and considerably to the north-west of Eldey. Two French naval officers who visited Reykjanes and made observations of the island about a fortnight before I arrived there, estimate its distance from the coast at nine or ten miles, but I believe it to be considerably greater. When first seen the island was perfectly black, but now the light-keeper tells me he can in clear weather distinguish by the aid of his glass a perceptible whitening of the upper part, due no doubt to the droppings of the myriads of sea-fowl which frequent the neighbouring islands and coast, and have apparently at once taken possession of the new island.

It is a singular fact that none of the usual volcanic manifestations seem to have announced or accompanied the rise of the new island; no earthquakes were felt, no smoke or fire seen, and no pumice found floating on the sea. The island seems to have risen calmly and silently, without a soul being aware of its appearance, till, on July 29, the light-keeper happening to look out to seaward, discovered it. For aught that any one knows to the contrary, it may have been there for many days before he happened to see it. No one has yet visited the island itself; the sea off Rekjanes is almost always rough, and the currents are very strong round the cape; the islands are surrounded by shoals and reefs; landing is at all times difficult and dangerous, even in the best weather, and quite impossible if the sea is at all disturbed; and as, since the discovery of the island, the weather has been for the most part stormy, intending explorers have been deterred by the dangers of the passage. Singularly enough, a French war-vessel and a Danish gun-boat which passed Reykjanes shortly before my visit failed to see the new island. From the direction in which the new island lies, and its apparent distance from the coast, I am inclined to think that it must lie near to the Geirfuglasker (Gare-fowl Skerry), one of the four islands above mentioned, which lies somewhat to the north-west of the line formed by the other three, and which, being low and flat, cannot be seen from Reykjanes. It is not impossible that the new island is merely an addition to or upheaval of the old Geirfuglasker, which, by heightening it so as to make it visible from the shore, would produce the impression that a new island had risen. This view is held by some of the fishermen on the coast who are familiar with the islands, but the point cannot be definitely settled till the island is visited.

W. G. SPENCE PATERSON,
H.B.M. Consul for Iceland

British Consulate, Reykjavík, September 27

TELESCOPES FOR ASTRONOMICAL PHOTOGRAPHY

I.

BEFORE giving any suggestions as to the best kind of telescope to use, and the best methods to follow in the application of photography to astronomical observation and record, it may be more convenient to mention briefly what can be done in this way, particularly as the subject will be new to many who have not followed closely what has been recently done.

I wish to mention (1) That photography has now shown itself to be capable of giving us pictures of nebulæ that are superior to those made by eye and hand. (2) That anything that can be seen by the eye with a telescope of a certain size can be photographed, and, further than this, stars that are too faint to be seen in this telescope can yet be photographed by it with sufficient exposure. (3) That portions of the heavens of several degrees extent each way can be photographed, and stars therein of a magnitude smaller than that shown on the best existing charts or maps, pictured in their proper relative positions and magnitudes in a quicker, better, and more accurate manner than by the plan hitherto used. (4) That it is possible thus to make a complete series of such pictures embracing the whole heavens, that will be practically free from human error. (5) That each individual nebula, cluster, or group of stars, can also be taken on as large a scale as possible, and form a supplement to the picture-maps on the smaller scale. (6) That though such pictures may differ slightly from the eye observations, owing to the different colours of light not affecting the eye and the sensitive plate in the same manner, they would have the enormous advantage that they could be compared directly with other pictures, taken, after the lapse of any number of years, under conditions that there would be no difficulty in making almost identical. (7) That there are other applications of this new power, as in direct enlargements of the surface of the moon piece by piece, of the planets, of double stars, and close clusters, and indirectly in the discovery of planets, either major or minor, by the simple process of direct comparison of star pictures taken at intervals, when the actual position of a planet will be recorded at each date. If there be a planet beyond Neptune, such a plan as this is perhaps the only way to detect it, especially if it is now near the Milky Way, where stars of its probable magnitude cluster so thickly that no process other than this could be used to chart the stars and detect movement. If these things can be done, and I most confidently say they can, then it must be admitted that nothing short of a revolution in observational astronomy must result, to the enormous gain of astronomy.

I speak relying entirely on my own work and experiments, which I shall refer to in detail further on, and I am strengthened in my opinions by what I hear has been done in a similar direction elsewhere, though I have not, except in one case, seen any of the actual work done.

The possibilities that are thus opened out really border on the marvellous. As has been already said by some one else, a library may now be made, not of books full of descriptions and figures, the accumulated work of many men working many years, each on his own system, but of pictures written on leaves of glass by the stars themselves.

Such a work will mark an epoch in astronomy, and its value increase as long as astronomers exist. No one can doubt for one moment the importance of such a work, nor the fact that, now it is possible, any delay in doing it will be a direct loss to astronomy. How it is to be done—whether by the slow process of letting it be done by the disjointed efforts of many amateurs of astronomy, or by being properly taken in hand and finished by united effort and proper means in the course of a few years—remains to be seen.

I propose to make some suggestions as to the practical part of this work in the selection of the best kind of telescope and mounting, the methods of working, the work to be done, and some other matters in connection. The most important matter is no doubt the selection of the best instrument to work with: of the two kinds of telescopes now in use, the reflector seems to be the most suitable for this work, though a reservation may be made in favour of the refracting principle where large fields on a small scale are required. Both kinds of telescopes when of moderate dimensions, that is, not more than 18 inches aperture, are so nearly alike as optical instruments that the chief distinction worth noting, neglecting for the present one or two points where they differ, and altogether such points as are rather matters of individual prejudice on the part of the observer than qualities or defects in the instruments, is that of cost, the reflector being very much less expensive to make. It is true that the refractor has been hitherto generally considered the most satisfactory in use, and has been preferred when expense has not been a consideration of importance. I think this may be rather due to the greater care that is bestowed upon the more expensive instrument, both in the making of the object-glass and the mounting, than to any real difference that there is between them. The first cost of the raw material alone differs immensely. For the reflector one disk of glass alone is required, and if it is only properly annealed it need not be optically pure. There is only one surface to work, though it is of importance that this should be properly figured, this is not a difficult matter, yet there is little doubt it has often been very imperfectly done in many so-called reflecting telescopes.

For the refractor two disks of glass are required; they must be optically pure, and their first cost alone is more than is often spent on the reflector, including the mounting. These disks must be wrought on four surfaces to proper curves, and time often spent afterwards in perfecting the object-glass; when this is done, the cost is found to be so great that it is felt to be worth a costly mounting. We cannot then be surprised that the better made and mounted telescope should be chosen, but that does not decide the question, Which is the best optical instrument? Nor can this question be decided definitively, because the images formed by each differ. If we look with a reflector at a bright star, the image is seen as an intensely bright point of light, dazzling to the eye if the telescope is large, and we see rays or coruscations round it of an irregular shape that are never steady. I think this effect is not due to the telescope, but is entirely subjective, and caused by this extremely small point of light exciting only a very small portion of the retina; for by proper precautions the light can be reduced, and these rays and the dazzling effect got rid of. With stars less bright it is not so pronounced, and on planets or objects of sensible magnitude it ceases entirely. The image of such a bright star in the refractor is quite of another kind: it is seen as a small disk of light of *sensible diameter* surrounded by the well-known system of diffraction rings and outstanding colour. This disk of light though small, has a different effect on the retina: it can be seen as a shape, pretty steady and free from too much dazzling glare. It is here that the refractor has such an advantage for micrometrical work, permitting bisections to be made with such precision.

The adjustments of the object-glass are considered more constant than those of the speculum, and though the troubles attending the reflector are much exaggerated, they have existed in the arrangements usually adopted. For certain instruments such as the transit-circle, where the connection between the optical axis and some part of the instrument has to be maintained, the object-glass is superior to the speculum; a tilt of the former that would not have an appreciable effect on the position of the image of a star would in the other displace this image twice the amount of tilt.

Both kinds have certain advantages, according to the use they are put to, and it is really not of much consequence which is the best instrument of this size. It is when we begin to consider the effect of increased size and all its attendant difficulties that the question of the suitability of either for the purpose of photography has to be answered.

With the reflector increase of size means proportionate increase in other qualities, in light-grasping power, in defining, and in separating power. With the refractor the greater absorption of light due to increased thickness reduces the light-grasping power, and definition becomes a matter depending not upon the optician but upon the glass-maker; the correction for colour, which even in theory is approximate only, becomes more difficult, and the defects due to the necessarily imperfect correction become more apparent—and these two facts alone show that as the refracting telescope gains in size it becomes more and more unsuitable for photography.

Moreover, when the aperture of the two kinds of telescopes under consideration is the same, the focal length of one must be something like twice that of the other, and that means that the image is four times less bright, and there does not seem to be any indication that the focal length of refractors can be very much reduced. This is only one part of the question, the next and most important one is that of actual cost or difficulty of construction. In the case of the refractor the preliminary difficulty in getting the lumps of glass out of which the lenses have to be made is so great that the increase of the size beyond 30 inches seems at the present moment very doubtful—they may reach 3-foot, or even 4-foot aperture, but it is most unlikely: the cost alone, good or bad, would be simply enormous, and such a size may be for the present left out of consideration. With the reflector the case is entirely different: from what has been said, it is easy to see that the gain by increase of size is proportionate here, and that only mechanical difficulties have to be met. Mirrors of glass covered or coated with silver for the reflecting surface are now in existence of 3- and 4-foot aperture; larger are in hand, and can be made at a cost absurdly below the cost of even a possible refractor: the only limit that I can see here is that of glass, and the limit in this case stops not at 30 inches, as with the refractor, but at something like 70 inches, and that and nothing else of a constructive character prevents the reflector being made much larger, and size is a great thing in photography. It is, in the case of eye-observation, a fact that you could positively have a telescope too big for the eye to use, but any increase that is at present possible in the reflector would only add to its photographic power.

The optical arrangements of the reflector are so varied that I propose to treat of them in detail for the purpose of indicating the most suitable. A. AINSLIE COMMON

NOTES

THE following is the list of officers, &c., to be proposed at the anniversary meeting of the Royal Society, December 1, 1884:—President, Prof. Thomas Henry Huxley, LL.D. Treasurer, John Evans, D.C.L., LL.D. Secretaries: Prof. George Gabriel Stokes, M.A., D.C.L., LL.D., Prof. Michael Foster, M.A., M.D. Foreign Secretary, Prof. Alexander William Williamson, LL.D. Other Members of the Council: Capt. W. de Wiveleslie Abney, R.E., William Henry M. Christie, Astronomer-Royal, Prof. George H. Darwin, M.A., F.R.A.S., Warren De La Rue, M.A., D.C.L., Robert Etheridge, F.R.S.E., F.G.S., Sir Frederick J. O. Evans, K.C.B., Prof. William Henry Flower, LL.D., Prof. George Carey Foster, B.A., Sir Joseph D. Hooker, K.C.S.I., Prof. Henry N. Moseley, M.A., F.L.S., Hugo Müller, Ph.D., Capt. Andrew Noble, R.A., C.B., Lord Rayleigh, D.C.L., Prof. J. S. Burdon Sanderson, LL.D., Lieut.-Gen. R. Strachey, R.E., C.S.I., Prof. J. J. Sylvester, M.A., D.C.L., LL.D.

PROF. LIVERSIDGE, of the Sydney University, sends to the local press a suggestive communication in connection with the recent meeting of the British Association in Montreal, and the invitation forwarded by the Victorian Premier to visit Melbourne next year. Feeling how insurmountable for the present are the obstacles to such a visit, the writer proposes what appears to be a very wise alternative. Instead of looking forward to a near visit from the Association, he suggests, as a preliminary step, a federation of the various scientific societies in Australia, Tasmania, and New Zealand into an Australasian Association for the Advancement of Science on the lines of the British Association. A first meeting of the new Association might be held in Sydney on the hundredth anniversary of the colony, which with the combined attractions of an International Exhibition might induce a fair number of scientific visitors from England to take part in the proceedings. After the first meeting gatherings could take place annually, or every two or three years, as might be agreed upon by the members, in various parts of Australasia. The writer concludes with the remark, which few will gainsay, that such an Association would tend greatly to advance the sciences in the colonies, and in many ways materially favour their progress elsewhere.

ACCORDING to *Science*, Prof. E. S. Holden, Director of the Washburn Observatory of the University of Wisconsin, has lately collected all the data available for the discussion of the law of distribution of the fixed stars, so far as this is determinable from the method of star-gauging. The data were collected from a comparison with the results of a series of star-gauges in progress with the 15-inch equatorial of the Washburn Observatory; and they included (1) the 683 previously published gauges of Sir W. Herschel, with the places brought down from 1690 to 1860; (2) the 405 unpublished gauges of Sir W. Herschel, extracted from his observing-books, and generously placed at Prof. Holden's disposal by Lieut.-Col. John Herschel (these also reduced to 1860); (3) 500 counts of stars from the published charts of Dr. C. H. F. Peters; (4) 983 counts of stars from the unpublished charts of Dr. Peters, from the Paris charts as revised by him, and from the unpublished ecliptic charts of Prof. Watson; (5) 856 counts of stars from the unpublished and published charts of Dr. J. Palisa. These, with the data from Sir J. Herschel's 605 southern gauges, and Celoria's *Durchmusterung* of the stars between 0° and + 6^d, complete the very valuable collection of data which Prof. Holden has brought together in convenient tabular form, and from which one of his most important conclusions is, that the method of star-gauging must be applied to the study of comparatively small regions, and that the results from these are then to be combined into larger groups. Prof. Holden hopes that these tables may serve the valuable end of finally disposing of the fundamental assumption that the stars are equally scattered in space, and may bring about the study of their distribution on a more general basis.

THE Boston Society of Natural History have adopted a policy with regard to their library which, if generally followed, would make scientific libraries more generally useful. The Society send such books as can be replaced to students in any part of the country. The receivers of course pay the cost of carriage, and, in addition, strangers are required to deposit a sum equal to twice the market-value of the books so lent, as a guarantee against loss.

A BUREAU of scientific information has been formed in Philadelphia, composed of officers and members of the Academy of Science, whose duty shall be the imparting, through correspondence, of precise and definite information upon the different departments of science. The organisation is purely voluntary. The Secretary is Prof. Angelo Heilprin, of the Academy of Science.

THE new buildings of the Central School at Paris were opened last week by M. Rouvier, the new Minister of Commerce and Agriculture. A number of speeches were delivered on the occasion, from which we learn that as many as 5000 French engineers owe their training to this institution since its foundation fifty years ago by the late M. Dumas and others. The object contemplated by the erection of this institution was to check the predominatingly theoretical character of the instruction imparted by the Government schools and to remodel the engineering education in France according to the English standard. About ten years ago the establishment was purchased by the Government, but the teachers have held as closely as possible to the lines on which its teaching was originally laid down.

MR. STANFORD, of Charing Cross, has issued a reprint of the paper on the Ethnology of Egyptian Sudán, contributed by Prof. A. H. Keane to the November number of the *Journal* of the Anthropological Institute. This monograph, which will be welcome to all interested in the eventful drama now in progress in the Nile Valley, contains a summary but comprehensive survey of all the races between Egypt and the Equator, which are grouped in five main divisions: Bantu, Negro, Nubian, Semitic, and Hamitic. Much light is thrown on the obscure relations of these peoples to each other, and a clear picture presented to the reader of the manifold ethnical conditions in those regions. The tabulated schemes of all the Sudánese races with their numerous subdivisions, seem to be very complete, and will help to a better understanding of the reports daily received from the scene of the operations undertaken for the relief of Gen. Gordon and the Egyptian garrisons in the Sudán.

THE first annual meeting of the New England Meteorological Society was held in Boston on the 21st ult. The papers read were:—On rain-gauges, by Mr. Fitzgerald; rainfall maps, by Mr. Davis; weather observers in New England, by Prof. Upton; the establishment of a meteorological station on Blue Hill, Mass., by Mr. Rotch.

WITH reference to our recent note to the effect that Prof. Hugo Gyldén, Director of the Stockholm Observatory, had accepted a professorship at the Göttingen University, we are informed that the celebrated astronomer will, in consequence of the generous offer made to him by the King of Sweden, remain in his native country.

PROF. F. E. NIPHER finds, according to *Science*, from data taken from Dr. Engelmann's observations at St. Louis, Mo., lasting over a period of forty-seven years, that the duration of maximum rains is inversely proportional to the violence, or that the product of violence into duration is constant. This constant is the amount of water which may fall in a continuous rain, and is, for Dr. Engelmann's series of half a century, about 5 inches. A rain of 5 inches per hour may last one hour. A rain of 4 inches per hour may last an hour and a quarter; and such a rain Dr. Engelmann observed. A rain of 2½ inches per hour may last two hours, and several such rains were observed. A rain of 1 inch per hour may last 5 hours. Each of these cases would be a 5-inch rain. For a longer period of time than fifty years it is likely that greater rains than 5 inches may be observed. The same is to be said if observations are to be taken over a wider area of country. In fact, a rain of 6 inches in three hours occurred near Cuba, Mo., some years since. This would increase the value of the constant from five to six, but otherwise the relation will probably remain unchanged. The importance of this law, *Science* points out, is very great in engineering, where the capacity of sewers, culverts, and bridges must be such as to carry the water. A more general investigation which Prof. Nipher is now making will determine the relation between the violence, duration, and frequency not only of maximum but of all rains. This work, when completed, will enable an engineer to construct the water-ways of bridges of such a capacity that they will probably stand a definite number of years before they are washed away. This number of years will be so determined that the interest on the invested capital during the probable life of the bridge will equal the possible damage when the destructive flood comes which the engineer determines shall destroy his work. The running expense of maintaining the bridge is then the least possible.

IN the October number of the *American Journal of Science* Mr. Lewis discusses the validity of observations on supposed glacial action at eleven points in Pennsylvania south of the terminal moraine, all of which he has visited. He concludes that they are all non-glacial, some being simple water-worn gravels, others being ice-rafted boulders, while the scratches reported in two localities are pronounced to be plant-fossils. The glacial action reported in Virginia needs, it is said, similar re-examination.

THE Meudon balloon made its third trial trip last Saturday. Starting at 12.15 noon, when a slight south-west breeze was blowing, it drifted in the direction of the Boulogne racecourse, and after arriving in the vicinity of that place, a distance of about a mile from its starting-point, obeying the motive power controlling its movement, it retraced its journey and alighted at the place from which it had ascended at one o'clock, having thus taken three-quarters of an hour to finish its trip of two miles altogether, going and coming. It is said, however, that the motive power of the voltaic elements was not quite so efficient as had been anticipated.

AT the last meeting of the Geographical Society of Hamburg, Dr. Sievers gave a short sketch of a journey of a year's duration which he intends making in the Cordilleras of Merida in Venezuela. Geographical investigation has, so far, not touched this region. Humboldt travelled through the eastern part from Cumaná to Carácas, the llanos of Carácas and Calabozo, and the districts in the Upper Orinoco, but he did not visit the Cordillera region of Merida. Later travellers also, including Godazzi, whose work was otherwise thorough, did not reach the place. Dr. Sievers will examine the region geologically, and obtain as many measurements of heights as possible.

THE report of a journey from Seul, the capital of Corea, to Songdo, by Mr. Aston, a consular official in Corea, has been published. The difficulties of travel in the country appear to have been much exaggerated; the people are friendly to strangers, and the discomforts are not greater than in China.

ACCORDING to a telegram from Calcutta, Mr. Griesbach, the geologist with the Afghan Boundary Commission, describes the route between Quetta and the Helmund as presenting features very similar to those in the Pishin valley and Candahar, namely, a system of precipitous, deeply eroded ridges, extending from north and south to north-east and south-west. Extensive post-Tertiary deposits fill the intervening valleys. The south-west extremity of the Ghazarband range is composed of sandstone shales and grits of the Flysch facies of Eocene rocks. A series of low hills and valleys stretch between Canjpai and Nushki, which from their composition appear to be merely continuations of the Kojah Amran range, but near Galiahah the formation is distinctly younger, the epoch being mostly trap-rock, which in places bursts through the Cretaceous limestone overlying it, and locally converts it into white marble.

NOT the least valuable of the many excellent reports published in the course of the year by the Chinese Customs department is that of the medical officers on the health of the various ports at which they are stationed. These gentlemen deal frequently with subjects of wider interest than the sanitary condition and health of certain limited portions of the Chinese Empire. Thus in the last reports, Dr. Macgowan, of Wenchow, gives an account of the cholera epidemic which visited China last year. He states, on the authority of a native author, that Indian or Asiatic cholera first made its appearance in China in 1821, medical tradition attributing its origin to the Straits of Malacca, whence it was brought to Fokhien in a junk. It subsequently spread southward to Canton, and from thence to other provinces. In 1825 a great outbreak occurred at Ch'un-Ching. on the Yangtsze, and thence the disease travelled slowly northward, visiting Corea and Japan, where it became extremely virulent. It has since been endemic in China, sometimes becoming epidemic, occasionally extending over the whole of Eastern Asia, and at other times confining itself to a province or part of a province. Dr. Macgowan states that the native doctors treated the disease as common cholera, and did not cure one in a hundred; and he concludes that Indian cholera in China differs from the common cholera of the country only in its epidemic character, the former being migratory, the latter stationary.

IN the *Archives des Sciences physiques et naturelles*, Prof. Forel of Morges has a paper on the solar corona of the spring of 1884, of which the following is a summary. In Switzerland, in the course of the present year, has been observed an extraordinary optical phenomenon consisting of a reddish corona of large diameter surrounding the disk of the sun, as well as of a reddish tint on the white clouds. This corona has been visible since the beginning of the year, and during the months of July and August it was constantly seen. Visible from high altitudes whenever the sky was clear, it was generally lost lower down, hidden probably by the light from lower layers of dust in the atmosphere. The corona is probably occasioned by dust settling in the higher layers of the atmosphere where they are protected from meteorological variations of the lower layers. This dust would be of uniform dimensions, and of a mean diameter of about 0·003 mm. In the absence of any other explanation, M. Forel refers this phenomenon to the brilliant crepuscular illuminations of last winter, and attributes these luminous objects to the volcanic dust of the eruption of Krakatoa of August 27, 1883. In *La Nature* M. Tissandier describes the corona as observed in two balloon ascents on October 23 and 24.

M. Henri Maget is about to publish in Paris an atlas of the French colonies and foreign possessions. The work, which will consist of twenty-five maps, will be brought out with the assistance of eminent French colonial geographers. The maps will be of large size, in three or four colours, and some of them have obtained a silver medal and a diploma of honour, at the recent Geographical Exhibition at Bar-le-Duc. It will be completed in five parts, the first of which has already appeared. This contains maps of (1) New Caledonia, (2) Central Africa (the Congo and the Gaboon), (3) Tonquin, (4) Madagascar, (5) the Grand Duchy of Luxemburg. The second part will contain maps of Réunion, Tahiti, Guadaloupe, Senegal, and the New Hebrides.

We have again to welcome the appearance of a new edition (the tenth) of Prof. Morren's most useful "Correspondance botanique." Since the appearance of the ninth edition (in 1881) the list of "gardens, chairs, museums, and botanical reviews and societies throughout the world," including also the addresses of all private working botanists known to the editor, has again undergone considerable enlargement—we hope an indication of a gradual spreading of interest in botanical science.

Dr. Brudenell Carter has issued in a separate form his now celebrated letter to the *Times* on "Eyesight and Civilisation" (Macmillan and Co.). He has taken the opportunity to introduce a few explanatory diagrams.

Prof. F. W. Putman has sent to the *Leader* a full account of his recent explorations amongst the so-called Liberty Group of Mounds on the Harness estate, Ohio, first surveyed and described by Squier and Davis in 1840. In their great work on "The Ancient Monuments of the Mississippi Valley" these archæologists describe five small mounds within the great square of twenty-seven acres. Most of these, as well as three others represented on their plan just outside a "gateway" on the east side of the larger forty-acre square have been much reduced by cultivation. All have now been carefully examined, two—evidently burial-places—yielding objects of considerable interest. The human bones were much decayed; but amongst the other finds were copper plates, ear-rings, and celts, slate and stone ornaments, some large beads covered with copper, and in one instance with silver over the copper, and many other objects, all of which have been deposited in the Museum of Cambridge University. In another large mound north of the same spot an extensive bed of ashes and charcoal yielded much pottery, pieces of cut mica, some grass matting with charred seeds, nuts, acorns, and bones. Near the eastern corner of the great square stands the largest mound of the whole group, which in future Reports of the Peabody Museum will be referred to as the "Big Mound of the Liberty Group." It is 160 feet long by 80 to 90 wide, and 13 to 18 high, and appears from the portion so far examined to be a burial-place of a remarkable character. Some 40 feet from the centre, at the northern end, twelve chambers were opened, and yielded charred mats and cloth in which the bodies had evidently been wrapped, besides various burnt objects, such as copper plates, ear-rings, shell beads, and long flint knives. In two of the chambers skeletons were found stretched at full length, with a copper plate on one of them, the action of which had preserved the structure of a finely-woven piece of cloth. In the other chambers the bodies had been burnt on the spot, as shown by the relative position of the bones and by the fact that in two instances portions of the bodies had fallen beyond the fire, and so escaped burning. Other discoveries made early in the present year in two of the pits by some boys, under the guidance of Mr. Wilson, yielded a great variety of objects which have also been secured for the Peabody Museum. Important links have thus been obtained between the builders of this great mound and neighbouring earth-works in the Scioto Valley and the constructors of the remarkable group on the Turner estate in the Little Miami Valley.

Mr. Ellis, of 90, New Bond Street, has now on exhibition a number of garments, fur-lined and fur-covered, which were used by the German Polar Expeditions of 1882. In both cases the furs were hardly worn at all. The first expedition, which wintered from August 21, 1882, to September 12, 1883, in Kingawa Fjord, Cumberland Gulf, 60° 15′ W. longitude and 60° 36′ N. latitude, and as there was a perfect calm through the winter, the furs were not necessary; similarly the second expedition, which wintered in the island of South Georgia (36° 5′ W. longitude and 54° 32′ S. latitude) from August 21, 1882, until September 5, 1883, found the temperature equally mild. The furs were lent for exhibition by the Imperial German Polar Commission.

The last census of Roumania gives a total population of 4,424,961, of which 2,276,558 are males, and 2,148,403 are females. According to religious sects there are 4,198,664 orthodox Greeks, 134,168 Jews, 45,152 Roman Catholics, 28,903 Protestants, 8734 Gregorians, 8108 Armenians, and 1323 Mohammedans. The foreign element in the population is composed as follows:—28,128 Austrians, 9525 Greeks, 3658 Germans, 2822 English, 2706 Russians, 2631 Turks, 1142 French, 167 Italians, and 539 of various nationalities—in all 51,138 persons. The urban population numbers only 781,170, while the rural population is 3,643,783.

On October 16 a mirage was seen at Lindesberg, in Central Sweden. It represented a large town with four-storied houses, a castle, and a lake. The phenomenon was observed for about fifteen minutes.

The red sun-glows have recently been observed in the far north of Sweden.

The additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus innuus*) from North Africa, an Anubis Baboon (*Cynocephalus anubis*) from West Africa, a Siamese Blue Pie (*Urocissa magnirostris*) from Siam, presented by Mr. R. B. Colom; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. C. M. Courage; six Alexandrine Parrakeets (*Palæornis alexandri*), a Blossom-headed Parrakeet (*Palæornis cynocephalus*), a Banded Parrakeet (*Palæornis fasciatus*), from British Burmah, presented by Mr. Eugene W. Oates, F.Z.S.; two Ring-necked Parrakeets (*Palæornis torquatus*) from India, presented respectively by Mr. W. G. Burrows and Miss Perry; a Weka Rail (*Ocydromus australis*, white var.) from New Zealand, presented by Mr. J. Satchell Studley; a Brown Capuchin (*Cebus fatuellus*) from Guiana, two Pronghorn Antelopes (*Antilocapra americana* ♂ ♀) from North America, deposited; a Great Grey Shrike (*Lanius excubitor*), six Curlews (*Numenius arquata*), British, purchased; a Blue-winged Teal (*Querquedula cyanoptera* ♂) from South America, received in exchange.

VARIATION OF THE ATOMIC WEIGHTS

THE annexed list contains all the elements except a few very little investigated. If the whole numbers in columns are taken to be each the weight of nine atoms in the gaseous state, and a comparison is made with the best determinations of vapour-densities on record, the result is as follows. The first nineteen determinations are Deville and Troost's, and are to be found in *Comptes Rendus*, xlv. (1857) p. 823; lvi. (1863) p. 893; lx. (1865) p. 1222; lxiii. (1866) p. 20.

		Vols.	Observed at	Calc. sp. gr.	
$3P_6$	=	1		4·433	
			500° & 1040°	4·425	
$3As_6$	=	1		10·529	
			564	10·6	
$3Se_2$	=	1		5·5416	
			1420	5·68	
$3Te_2$	=	1		9·0513	
			1390 & 1439	9·04	
$3Cd$	=	1		3·9253	
			1040	3·94	
$3Al_3Cl_3$	=	1		9·3514	
			350 & 440	9·348	
$3Al_3Br_3$	=	1		18·772	
			440	18·62	
$3Fe_2Cl_3$	=	1		11·3834	
			440	11·395	
$3Ta_4Cl_4$	=	2		9·836	
			350	9·6	
$3Nb_3Cl_5$	=	2		9·5208	
			350	9·6	
$3Nb_3Cl_3O_2$	=	2		7·654	
			440 & 860	7·88	
$3Zr_4Cl_4$	=	2		8·0815	
			440	8·15	
$3Hg_2Cl$	=	2		8·3085	
				8·21	
				8·35	Mitscherlich
$3H_4N_3Cl$	=	4		0·9294	
			350 & 1040	1·005	
$3H_4N_3Br$	=	4		1·7144	
			860	1·71	
$3H_4N_3I$	=	4		2·5457	
			440	2·59	
$3H_3N_3C_4H_5Cl$	=	4		1·4143	
			350	1·44	
$3H_4N_3ClHgCl$	=	4		3·3134	
			440	3·5	
$3H_4N_3IHgI$	=	4		6·546	
			350	6·49	
$3Cl$	=	1		2·47	
				2·47	Berzelius
$3Br$	=	1		5·611	
				5·54	Mitscherlich
$3I$	=	1		8·9358	
				8·89	V. Meyer
$3Hg$	=	1		7·0655	
				7·03	Mitscherlich
$3HgCl$	=	1		9·536	
				9·8	Mitscherlich
$3As_6O_6$	=	1		13·854	
				13·85	Mitscherlich
$3P_3S_5$	=	1		7·758	
				7·67	V. & C. Meyer
$3P_3Cl_3$	=	2		4·814	
				4·85	Mitscherlich
				4·875	Dumas
$3As_3Cl_3$	=	2		6·3383	
				6·3	Dumas
$3Bi_3Cl_3$	=	2		11·1871	
				11·16	Jacquelain
$3PbCl$	=	1		9·536	
At 1046°–1089° mean of 4 exp. =				9·5	Roscoe[1]
$3Ti_4Cl_4$	=	2		6·8808	
				6·836	Dumas
$3Sn_4Cl_4$	=	2		9·1898	
				9·199	Dumas
$3Si_4F_4$	=	2		3·6944	
				3·6	Dumas
$3Si_4Cl_4$	=	2		5·9572	
				5·939	Dumas
$3Sb_3Cl_3$	=	2		8·07	
				8·1	Roscoe & Schorlemmer ("Chemistry")
$3Sb_3(C_4H_5)_3$	=	2		7·3773	
				7·438	Löwig & Schweitzer[2]
$3In_2Cl_3$	=	2		7·7698	
				7·87	V. & C. Meyer

[1] *Proc. Roy. Soc.* xxvii. p. 427. [2] *Journ. Chem. Soc.* v. p. 69.

The agreement in all cases is such that, considering the difficulties with which the determination of vapour-densities is attended, it is not likely that other atomic weights could be chosen to obtain like good results. If now the weights in column *t* are taken to be the weights of a single atom for each element in a certain solid or liquid state, the percentages of oxygen in the following chlorates agree closely with the values found by experiment,[1] to wit:—

$100AgClO_6$	contain	25·0525 O	
	found	25·0795 O	Stas
	,,	25·088 O	Marignac
$100AgBrO_6$	contain	20·34 O	
		20·349 O	Stas
$100AgIO_6$	,,	16·9619 O	
		16·9747 O	Stas
		17·047 O	Millon
$100KBrO_6$	,,	28·7307 O	
		28·6755 O	Marignac
$100KIO_6$	,,	22·4227 O	
		22·473 O	Millon
$100NaClO_6$	,,	45·0672 O	
		45·0705 O	Penny

The agreement in these instances is as good as with the adopted weights; but it is complete also in the following cases, in which there are great discrepancies with the prevailing atomic weights:—

$100PtCl_2KCl$ contain	69·362 $PtCl_2$ and		30·638 KCl		
	69·417	,,	30·583	,,	Berzelius
	69·318	,,	30·682	,,	Seubert
Mean	69·3675	,,	30·6325	,,	
,, ,, yield	... ...		117·825 AgCl		
			117·9606	,,	Seubert

The agreement of the mean of the percentages of Berzelius and Seubert with the calculated values is complete; the discrepancy between the amounts of silver chloride is small and within the limits of errors of observation. But the percentages of platinum and chlorine in $PtCl_2$ arrived at by the two experimenters are widely different, viz.:—

40·424 Pt; 28·993 Cl Berzelius
40·107 ,, ; 29·211 ,, Seubert

The true weight of the chlorine follows from Seubert's analysis of the ammonium salt—

$100H_4N_3PtCl_3$ yield 194·954 $(AgCl)_3$
Seubert obtained 192·846 ,,

His rate between the silver chloride and the potassium salt gives ... } Pt = 195·002 Clarke
His rate between the silver chloride and the ammonium salt gives ... } ,, = 196·871 ;

the latter rate is therefore at fault, and 100 parts of the ammonium salt correspond to 194·694 AgCl, if the rate is the same as with the potassium salt; the difference between this number and 194·954 is within the limits of errors of observation. The rate $\frac{100}{194·954} \times (AgCl_3)_3$ gives $H_4N_3ClPtCl_2 = 70·84883$, and the rate $\frac{69·362}{30·538} \times KCl$ gives $PtCl_2 = 53·95833$; H_4N_3Cl is therefore 16·8905, and as the weight of H_4N_3 is not in doubt and = 5·74468, Cl is = 11·14583, as in column *t*. With this weight of chlorine all discrepancies disappear, while the weights recalculated from the same data vary between Pt = 194·314 and 196·871. It is moreover minutely confirmed by the results obtained from all the other elements of the same group.

$100OsCl_2KCl$ contain } 41·0226 Os; 28·5027 Cl; 30·4747 KCl
40·638 ,, 28·9024 ,, 30·4596 ,, Berzeliu

Berzelius's percentage of chlorine is again too large, very nearly to the same extent as the chlorine found by him in the potassio-platinum chloride, while the percentage of the potassium chloride is very exact.

[1] The experimental values are those recalculated by Prof. F. W. Clarke ("Smithsonian Miscell. Coll.," vol. xxvii.).

100 $IrCl_2N_3H_4Cl$ contain	44·3691 Pt	
	43·732 ,,	Seubert
100 $IrCl_2KCl$ contain	40·3874 ,, ; 28·8097 Cl ; 30·803 KCl	
	39·88 ,, 29·291 ,, 30·82 ,,	Seubert
	29 ,,	Berzelius

The same discrepancies as in the case of the platinum salts present themselves: as the percentage of the potassium chloride is exact, that of $IrCl_2$ follows; and, as to the weight of the chlorine, the difference of the percentages found by the two experimenters shows that there is the same cause of error as in the corresponding platinum salt.

100 $PdCl_2.2KCl$ contain	32·678 Pd ; 21·4512 Cl ; 45·8708 KCl	
	32·69 ,, 21·416 ,, 45·892 ,,	Berzelius

The agreement is here as good as complete; but the values actually derived from these data vary from Pd = 104·674 to 110·796, owing to the value of the weight assumed for chlorine.

100 $Rh.Na_3Cl_3.Cl_3$ contain—

27·1468 Rh ; 45·6215 NaCl ; 27·2317 Cl		
27·094 ,, 45·577 ,, 27·329 ,,		Berzelius

100 $Rh.2KCl.Cl_3$ contain—

29·1276 Rh ; 41·6537 KCl ; 29·2187 Cl		
28·989 ,, 41·45 ,, 29·561 ,,		Berzelius

The agreement is almost complete in the case of the sodium salt, and not doubtful in the other, because the weight of KCl is certain. The values for rhodium derived from the sodium salt are very discordant, varying from 102·98 to 105·696.

100 $Ru.2KCl.Cl_3$ contain	28·9984 Ru ; 41·7297 KCl ; 29·2719 Cl	
Numbers actually found	28·96 ,, 41·39 ,,	Claus
Mean of the 3 experiments	28·78 ,, 41·09 ,, 30·17 ,,	Claus

The calculated amount of ruthenium is undoubtedly the actual percentage, because 28·91 Ru were found in the second experiment as 28·96 in the first; and the weight of KCl not being doubtful, that of chlorine can only be as calculated. The results which have been derived from these data are most discordant, viz. Ru 96·854—107·19.

The weights of column *s* give $O_6 = 16$ and $S_3 = 16$; those of column *t*, $O_6 = 15·31914$, $S_3 = 15$. . . . There is consequently a difference of the chemical proportions in the two states which explains many anomalies encountered in analytical work, and among others the following:—Berzelius observes (*Pogg. Ann.* viii. p. 16) that, from causes which he has been unable to discover, the atomic weight of sulphur cannot be derived from the specific gravities of the gaseous compounds H_2S and SO_2, the numbers obtained being so high that the discrepancies exceed the limits of possible errors of observation. He had obtained S = 201·165 from the analysis of $PbSO_4$; Thénard and Gay-Lussac's weighing of H_2S gave S = 203·9; his own weighing of SO_2, 207·58. His weight for O being 100, these 207·58 S represent 407·58 SO_2, which with $S_3 = O_6$ give S = 203·79, practically the same as the value derived from the other gaseous compound. The two numbers 203·9 and 203·79 reduced to the value of the weights of column *t* give respectively 191·056 and 191·053. Berzelius's number 201·165 corresponds to the value of column *t*, H being = 1; with H = 0·95745, the actual weight, it becomes 192·605. The three numbers in hydrogen units—15·292, 15·284, and 15·408—though from different causes all too large, agree with each other as well as can be expected under the circumstances, and the difficulty disappears therefore with the adoption of the weights of columns *s* and *t* for the two different states.

This being so, it is to be expected that for other states the weights will also be still further different, and this conclusion is fully confirmed by the facts. Let the weights of column *t* be = 1, then the weights of the states *a*, *b*, and *c* are as follows:—

$$a = 0·999104 ;\ b = 0·997338 ;\ c = 0·99468.$$

Instead of such loss of weight there may be a gain to the same extent, as, for instance, in the state $\frac{1}{b} = 1·002662$. There are still other variations which are multiples of *a*, *b*, *c*, as—

$$a^{\frac{3}{2}} = 0·99866 ;\ \frac{c^2a}{2} = 0·99424 ;\ cb = 0·99203.$$

The evidence of the reality of these weights appears from the following comparison with some of the very best experiments on record. The numbers marked with an asterisk are derived by the volumetric method, which, in consequence of variation of the atomic weights, yields in all cases more or less faulty results.

100 $KClO_6$ contain ...	60·87379 KCl = 1	
	60·81927 ,, = *a*	
Mean	60·84653	
Mean of all experiments on record	60·846 ,,	Berzelius, Penny, Pelouze, Marignac, Gerhardt, Maumené, Stas
100 Ag = *c* yield ...	132·8426 AgCl	
Mean of all experiments on record	132·8418 ,,	Berzelius, Turner, Penny, Marignac, Maumené, Dumas, Stas
100 Ag correspond to	69·0244 KCl = *a*	
*	69·062 ,,	Marignac
*	69·10345 ,,	Stas
100 Ag yield	114·8733 AgS = $a^{\frac{3}{2}}$	
	114·8581 ,,	Dumas, Stas, Cooke
100 AgCl yield ...	86·4733 ,, = $a^{\frac{3}{2}}$	
	86·4733 ,,	Berzelius, Svanberg, and Struve
100 Ag correspond to	54·1258 NaCl	
	*54·2076 ,,	Pelouze, Dumas, Stas
100 Ag yield	157·4707 AgN_3O_6 = *b*	
Mean of 7 experiments	157·472 ,,	Stas
Mean of all experiments on record	157·479 ,,	Penny, Marignac, Stas
100 AgN_3O_6 correspond to	84·35994 AgCl	
	84·3743 ,,	Turner, Penny
100 AgN_3O_6 correspond to	43·8331 KCl = *a*	
	*43·8715 ,,	Marignac, Stas
100 KCl = *a* yield ...	135·6532 KN_3O_6 = *c*	
	135·6423 ,,	Stas
	135·6345 ,,	Penny
100 $KClO_6$,, ...	82·5033 ,,	
	82·500 ,,	Penny
100 $NaClO_6$,, ...	79·8917 NaN_3O_6 = $\frac{c^2a}{2}$	
	79·8823 ,,	Penny
100 NaCl ,, ...	145·435 ,, = $\frac{c^2a}{2}$	
	145·4164 ,,	Penny
	145·4526 ,,	Stas
100 $AgC_4H_3O_4$ = *c* contain	64·6608 Ag	
	64·664 ,,	Marignac
	64·6065 ,,	Liebig and Redtenbach
100 $AgC_4H_2O_6$ = *c* contain	59·3367 ,,	
	59·2806 ,,	,, ,,
100 $AgC_4H_2O_5$ = *c* contain	62·0621 ,,	
	62·0016 ,,	,, ,,
100 BaCl yield ...	138·0494 AgCl	
	138·07 ,,	Berzelius
,, ,, ...	112·251 $BaSO_4$	
	112·19 ,,	Turner
	112·175 ,,	Berzelius
100 $CaCO_3$ = *c* yield	56·0312 CaO = 1	
General mean ...	56·0198 ,,	Dumas, Erdman, and Marchand
100 $CaCO_3$ = *c* yield	136·0037 $CaSO_4$ = 1	
	136·0525 ,,	Erdman and Marchand
100 Pb ,,	146·4418 $PbSO_4$	
	146·4262 ,,	Berzelius, Turner, Stas
100 PbO ,,	135·853 ,,	
	135·804 ,,	Turner

100 $PbSO_4$	yield	109·2444 $PbN_2O_6 = b$	
		109·307 ,,	Turner
100 Pb	,,	159·98 ,,	
		159·9743 ,,	Stas
100 $PbN_2O_6 = b$	,,	67·3799 $PbO = 1$	
		67·4016 ,,	Svanberg
100 AgCl correspond to		29·5607 $LiCl = \frac{1}{b}$	
		29·584 ,,	Mallet, Troost
100 Ag correspond to		39·2692 ,, $= \frac{1}{b}$	
		*39·358 ,,	Stas
100 $LiCl = \frac{1}{b}$	yield	162·6508 $LiN_2O_6 = c$	
		162·5953 ,,	Stas
100 Tl yield		130·38969 $TlN_2O_6 = b$	
Experiment 8 ...		130·3897 ,,	Crookes
Mean of 10 experiments		130·391 ,,	,,
100 $G_2O_3(SO_3)_3.12HO = c$ contain—			
		14.1694 GO	
		14·169 ,,	Nilson and Pettersson
100 $MgC_2O_4H_2O_2 = c$ contain—			
		27·338 $MgO = 1$	
		27·3665 ,,	Svanberg & Nordenfeldt
100 $MgCO_3 = c$ contain		47·6 ,,	
Mean of 19 experiments		47·627 ,,	Marchand and Scheere
100 $H_4N_2.SO_4.3AlOSO_3.24HO = cb$ contain—			
		11·2814 AlO	
Mean of 10 experiments		11·2793 ,,	Mallet
100 $H_4N_2SO_4.3GaOSO_3.24HO = cb$ contain—			
		18·9325 GaO	
		18·9596 ,,	Lecoq de Boisbaudran

These determinations include the most classical labours on record, and the general agreement with the calculated numbers is surprising, and the more conspicuous in the cases in which the efforts of the experimenters to exclude error have been pushed to the utmost limits, as in Stas's syntheses and in Prof. Crookes's synthesis of thallium nitrate. Notwithstanding the difficulty in this case, because the element is the heaviest of all so far discovered, one experiment has yielded the identical calculated number, and the mean of all deviates from it only by 0·00131. Moreover the same weights recur in similar compounds; all nitrates, for instance, have a lower value than the corresponding chlorides and sulphates, and the value is the lower the greater the composition, as in the alums. The evidence is such that no doubt seems to be admissible as to the reality of a variation of the atomic weights. This conclusion is independent of any value of the atomic weights; for the discrepancies exhibited in the results of Prof. Clarke's recalculations from the same experimental data above quoted are inevitable if the variation of the atomic weights is not taken into account. In c units Ag is 108·09679 if H = 1, calculated from the weights of column t; Cl in the gaseous state is = 35·66; the calculated weights correspond therefore, within the limits of experimental errors, to the atomic, but the weights are those of different states.

The difference between the weights of the gaseous and the other states is very considerable; the weight of 3 molecules of $H_4N_3I.HgI$, for instance, is = 378 in the state of gas, 354·734 in t units, 352·847 in units = c; the discrepancies are so great that they exceed by far the limits of possible errors, and as from the comparisons made it appears certain that the different values are realities, the only explanation is that the atomic weights vary. If in new experiments, in which the possibility of variation is kept in view, all discrepancies which actually exist should disappear, variation will be established beyond all doubt. It will then be in order to inquire into its cause. How the weights of the table have been obtained is, for the present, unessential; it is only necessary to add that column v contains Prof. Clarke's recalculated weights, and column u the same values calculated from the weights of column t, column x giving the number of atoms represented in each instance. Column w shows the corresponding weights of the gaseous state. These columns have been added for the sake of comparison.

	s	*t*	*u*	*v*	*w*	*x*
Li	22	2·36559	7·412	7·0235	7·333	3
Ca	58	6·23656	39·0824	40·082	38·666	6
Na	70	7·52688	23·5842	23·051	23·333	3
K	118	12·68817	39·7564	39·109	39·333	3
Rb	256	27·5269	86·2424	85·529	85·333	3
Mg	36	3·8537	24·15	24·014	24	6
Sr	132	14·1303	88·5498	87·575	88	6
Ba	206	20·05183	138·1915	137·007	137·333	6
Pb	306	32·7566	205·2748	206·946	204	6
Ag	324	34·683467	108·6748	107·923	108	3
Cs	398	42·605	133·496	132·918	132·666	3
H	3	0·31915	1	1·0023	1	3
N	14	1·48936	14	14·029	14	9
O	24	2·55319	16	16	16	6
F	58	6·04166	18·93	19·027	19·333	3
Cl	107	11·14583	34·9236	35·451	35·666	3
Br	243	25·3125	79·3125	79·951	81	3
I	387	40·3125	126·313	126·848	129	3
B	11	1·14583	10·771	10·966	11	9
G	14	1·45833	9·072	9·106	9·333	6
C	18	1·875	11·75	12·0011	12	6
Si	22	2·29166	28·722	28·26	29·333	12
Al	28	2·9166	27·416	27·075	28	9
P	32	3·3333	31·33	31·029	32	9
Ti	42	4·375	54·833	49·961	56	12
La	44	4·5833	143·61	138·844	146·666	30
S	48	5	31·33	32·058	32	6
Di	50	5·20833	146·875	144·906	150	27
Yt	60	6·25	88·125	90·023	90	13·5
Yb	62	6·45833	182·125	173·158	186	27
Ce	64	6·6666	139·26	140·747	142·222	20
Sc	66	6·875	43·0833	44·081	44	6
Zr	68	7·0833	88·7777	89·573	90·666	12
Ga	72	7·5	70·5	68·963	72	9
As	76	7·9166	74·417	75·09	76	9
V	78	8·125	50·9166	51·373	52	6
Cr	80	8·3333	52·222	52·129	53·333	6
Mn	84	8·75	54·833	54·029	56	6
Fe	86	8·9583	56·139	56·042	57·333	6
Ni	90	9·375	58·75	58·062	60	6
Co	91	9·4792	59·403	59·023	60·666	6
Sn	92	9·5833	120·11	117·968	122·666	12
Cu	96	10	62·666	63·318	64	6
Nb	98	10·20833	95·95833	94·027	98	9
Zn	100	10·4166	65·278	65·054	66·666	6
Ta	106	11·04166	184·5186	182·562	188·444	16
Se	120	12·5	78·333	78·978	80	6
Sb	126	13·125	123·375	120·231	126	9
W	142	14·79166	185·3888	184·032	189·333	12
Mo	150	15·625	97·9166	95·747	100	6
Cd	170	17·7083	110·972	112·092	113·333	6
In	176	18·3333	114·888	113·659	117·333	6
Th	178	18·54166	232·389	233·951	237·333	12
U	184	19·1666	240·222	239·03	245·333	12
Te	196	20·4166	127·945	128·254	130·666	6
Au	204	21·25	199·75	196·606	204	9
Bi	216	22·5	211·5	208·001	216	9
Ir	300	31·25	195·833	193·094	200	6
Pt	304	31·6666	198·444	194·867	202·666	6
Hg	306	31·875	199·75	200·171	204	6
Os	308	32·0833	201·056	198·951	205·333	6
Ru	318	33·125	103·7916	104·457	106	3
Rh	320	33·3333	104·444	104·285	106·666	3
Pd	326	33·95833	106·403	105·981	108·666	3
Tl	618	64·375	201·708	204·183	206	3

San Francisco, California, July 24 E. VOGEL

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following gentlemen were on Monday, November 3, elected to Fellowships at St. John's College:—C. M. Stewart, M.A., First Class in Natural Sciences Tripos of 1879, author of several papers on chemical subjects, and Master at the Newcastle School, Staffordshire; J. Brill, B.A., Fourth Wrangler in 1882, Assistant Professor of Mathematics in University College, Aberystwith; W. F. R. Weldon, B.A., First Class in the Natural Sciences Tripos of 1881, author of a number of papers in Zoology and Comparative Anatomy, formerly Demonstrator to the Professor of Zoology and in the Morphological Laboratory; A. R. Johnson, B.A., Sixth Wrangler and First Division in the Mathematical Tripos of 1882–83 (new regulations), author of papers in the *Messenger of Mathematics*, &c.; G. F. Stout, B.A., First Class in the Chemical Tripos of 1881–82 (new regulations), and First Class (with distinction in Metaphysics) in the Moral Sciences Tripos of 1883; G. B. Mathews, B.A., Senior Wrangler in 1884, Professor of Mathematics in the University College of North Wales, Bangor. It is worth noting that Pure and Applied Mathematics, Chemistry, and Biology have been markedly recognised by this election.

Dr. Donald MacAlister has been appointed University Lecturer in Medicine, and Dr. Bushell Annington University Lecturer in Medical Jurisprudence.

Mr. Walter Heape has been approved by the Board for Biology and Geology as Demonstrator in Animal Morphology, on the nomination of the Lecturer in that subject, Mr. Sedgwick.

Prof. Sidgwick, Prof. Adamson (Owens College), and Messrs. James Ward and J. S. Nicholson are appointed Examiners for the Moral Sciences Tripos.

Mr. A. R. Forsyth of Trinity College is appointed Examiner in the Mathematical Tripos (Third Part) in January next, in the place of the late Mr. R. C. Rowe.

In reference to our note a fortnight ago (vol. xxx. p. 649), we should state that, at Trinity College, Major Scholarships of the value of 80*l*. a year, which may be raised to 100*l*. subsequently, are open for competition in Natural Sciences as well as in Classics and Mathematics to persons not yet in residence, with the usual restriction as to age.

SHEFFIELD.—Another step has been taken in the formation of the new Engineering School at Firth College, Sheffield, in the appointment of Mr. W. H. Greenwood to be Professor of Metallurgy and Mechanical Engineering, and Mr. Ripper to be Assistant Professor of Engineering. It may be in the memory of our readers that the City and Guilds of London Institute made a grant about eighteen months ago of 300*l*. a year to the Firth College in aid of the establishment of a Chair of Engineering. Since then additional subscriptions have been promised for five years to the amount of 550*l*., together with a capital sum of over 10,000*l*. A site for laboratories and shops has been obtained, and these will be proceeded with as soon as possible. It is hoped that the special advantages of Sheffield will make it the central school of metallurgy, especially for iron and steel, in the kingdom, and the Committee intend to spare no efforts in rendering it a complete and effective one.

SCIENTIFIC SERIALS

The American Journal of Science, September.—On the amount of the atmospheric absorption, by S. P. Langley. From numerous observations taken at sea-level or at an altitude of nearly 15,000 feet, the author is led to infer that the mean absorption of light as well as of heat by our atmosphere is probably at least double the usual estimate of about 20 per cent. He also believes that fine dust particles, both near the surface and at a great altitude, play a more important part in this absorption, both general and selective, than has been hitherto supposed.—A study of tornadoes, by Henry A. Hazen. In this paper the author examines some of the ordinary theories that are advanced for explaining the origin and development of these destructive phenomena. After showing some of the seeming difficulties involved in these theories, he proceeds to point out a few of the characteristics of the outbursts, with a view to opening up fresh lines of investigation, upon which a further advance may be made towards a true knowledge of the forces underlying them. He is inclined to think that J. Allan Broun's theory, attributing tornadoes to the direct influence of the sun's electricity upon the moisture of the air, or possibly to the indirect effect from the sun's heat, is more satisfactory than the numerous theories of friction, evaporation, condensation, sudden changes of temperature, and the like.—On the absorption of radiant heat by carbon dioxide, by J. E. Keeler. The author considers it probable that to the action of CO_2 in the atmosphere is due one or more of the great gaps in the invisible part of the solar spectrum which the discoveries of Prof. Langley show to be much more extensive than had hitherto been supposed. He further regards it as certain that some other agent than this gas contributes essentially to the total absorptive power of the atmosphere, so that a method of analysis based on this power, in which the effect of the second agent is neglected, cannot lead to correct results.—Note on the Triassic insects from Fairplay, Colorado, by Samuel N. Scudder. These fossil remains present an assemblage of forms altogether different from anything hitherto found in the Palæozoic series on the one hand, or in the Jurassic beds on the other. They seem to show a commingling of strict Jurassic forms with a larger proportion of types which may be called Upper Carboniferous or Permian, with a distinct Jurassic leaning. Hence the probability that the beds in which they occur belong to the Triassic or intermediate formation.—On the flexibility of Itacolumite, by Orville A. Derby. From observations made on this extensive series of quartzose rocks occurring in the gold and diamond regions of Minas Geraes, Brazil, the author infers that the peculiar property of flexibility attributed to them is not an original characteristic, but only a surface character, a phase of weathering or decay brought about by percolating waters.—On the age of the glazed and contorted slaty rocks in the vicinity of Schodack Landing, Rensselaer County, New York, by S. W. Ford.—On the relations of the mineral belts of the Pacific slope to the great upheavals, by Geo. F. Becker. The views of H. P. Blake and Clarence King regarding the parallelism of the series of mineral belts on the Pacific slope to the great mountain ranges, and attributing the deposits themselves to the solfateric action accompanying the ejection of igneous rocks, have since been mainly confirmed. But, independently of any theory, a conclusion of economical importance evidently follows from the fresh facts recently brought to light. A great majority of all the rich ores west of the Wahsatch Range occur in belts following the western edges of distinct geological areas—the Cretaceous in Utah, the Palæozoic and Carboniferous in Nevada and Arizona, the Jura-Trias in East California, &c. Hence analogy points to the neighbourhood of the still unexplored portions of these contacts as the most promising for future discoveries of the precious metals.—Notice of the remarkable marine fauna occupying the outer banks off the southern coast of New England, No. 9, by A. E. Verrill.—Brief contributions to zoology from the Museum of Yale College, No. lv.—Work of the steamer *Albatross* in 1883.—Geology of the Blue Ridge, near Balcony Falls, Virginia, by John L. Campbell.

October.—On the duration of colour-impressions upon the retina, by Edward L. Nichols. Taking up the subject where it was left fifty years ago by Plateau's researches, the author concludes, from a protracted series of experiments: (1) that the study of the duration of colour-impressions produced by different portions of the spectrum tends to confirm Plateau's results; (2) that the persistence of the image is a function of the wave-length producing it, being greatest at the ends of the spectrum, and least in the yellow; (3) that it decreases with the intensity of the ray producing it; (4) that it is not the same for all eyes; (5) that the duration is in inverse order to the luminosity of the colours producing it; (6) that each wave-length of the visible spectrum produces three primary impressions, red, green, and violet, of which the green is the most evanescent, violet the most persistent; (7) that the duration of the retinal image depends upon the length of time during which the eye has been exposed, decreasing as the exposure increases.—Description of a fulgurite from Mount Thielson, Oregon (one illustration), by J. S. Diller.—On the paramorphosis of pyroxene to hornblende in rocks (two illustrations), by Geo. H. Williams.—On the southward ending of a great synclinal in the Taconic Range (with a map and several illustrations), by James D. Dana. The section of the Taconic Range here dealt with extends about 150 miles along the western border of New England, mainly between Middlebury, in Central Vermont, and Salisbury, in North-Western Connecticut. The conclusions arrived at regarding the synclinal character of the system and the Lower Silurian age of the rocks agree with those of Sir William Logan, except that he made the limestone to precede instead of to include the Trenton group.—On supposed glaciation in Pennsylvania, south of the terminal moraine (with a map), by Prof. H. Carville Lewis. The author considers that all the existing surface phenomena may be explained by the action of running waters and other causes independent of glaciation.—History and chemical analysis of a mass of meteoric iron found in a head-stream of the Red River, Wichita County, Texas, by J. W. Mallet. The analysis yielded iron over 90 per cent., nickel over 8, a little cobalt, tin, phosphorus, copper, sulphur, graphitic carbon, silica, and a trace of manganese.—The life and work of Jean-Baptiste-André Dumas, by J. P. Cooke.—Account of a new meteorite discovered in Grand Rapids, Michigan, on May 15, 1883, by J. R. Eastman. The analysis of the fragment now in the Smithsonian Institute yielded: iron 94·543, nickel 3·815, cobalt 0·369, insoluble residue 0·118.

Rivista Scientifico-Industriale, September 15-30.—Origin of atmospheric electricity, of thunder-clouds and volcanic eruptions, by Giovanni Luvini.—Description of an automatic and continuous registrator of electric energy transmitted at a given part of a circuit, by Prof. Rinaldo Ferrini.—On the electric conductivity of greatly diluted saline solutions, by Dr. Giuseppe Vicentini.—On a system of electro-chronometric bells adapted to private residences, by Giuseppe Bianchedi.—Note on the Walker railway-carriage break, by Angiolo Villa.—On a new system of simultaneous telegraphy and telephony, by M. Van Rysselberghe.—Descriptive notes on the fauna of Sardinia, by Prof. A. Costa.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, November 6.—Dr. Perkin, F.R.S., President, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting of the Society (November 20).—The following papers were read:—On the action of aldehydes and ammonia upon benzil (continued), by F. R. Japp and S. C. Hooker. In previous papers two general reactions have been studied relating to the joint action of aldehydes and ammonia upon similar bodies; in addition, a third totally distinct reaction occurs, which is investigated in the present paper. The authors have studied the action of salicylaldehyde and ammonia upon benzil. A condensation-product, $C_{28}H_{24}N_2O_4$, was obtained, which proved to be dibenzoyldihydroxystilbenediamine. By the action of dilute hydrochloric acid, the hydrochloride of a new base, $C_{14}H_{16}N_2O_2$, was formed; its platinum salt, picrate, sulphate, diacetyl derivative, &c., were prepared and examined. The authors have also studied the action of furfuraldehyde and ammonia upon benzil.—Isomeric modifications of sodium sulphate, by S. U. Pickering. The author has determined the heat of dissolution of effloresced sodium sulphate heated to various temperatures. He concludes that there are two modifications: one formed by not heating above 150°, the other being produced at temperatures from 150° to the fusing-point of the salt.—On some vanadates of the amines, by G. H. Bailey. The author has prepared and studied a considerable number of these bodies, and has compared them with the corresponding vanadates of the alkalies—Contributions to our knowledge of acetoacetic ether, part 1, by J. W. James.—On magnesium hydrosulphide solution and its use in chemico-legal cases as a source of hydrogen sulphide, by E. Divers and T. Shimidzu. The authors prepare this solution by passing ordinary hydrogen sulphide into a flask containing magnesia suspended in water. By heating the solution to 60°, a steady stream of hydrogen sulphide free from hydrogen and from hydrogen arsenide is obtained.—On the origin of calcium thiosulphate: an emendatory note to a paper on calcium hydrosulphide, by E. Divers. The author concludes that there is essentially only one method of forming the thiosulphate, *i.e.* by the union of sulphur with calcium sulphite.

Physical Society, November 8.—Prof. Ayrton in the chair.—Mr. Kavargee was elected a member of the Society.—Prof. F. Guthrie read a paper on certain phenomena attending mixture. In a previous paper Dr. Guthrie had noticed the increase of volume attending the separation of triethylamine and water effected by heat. The present paper is an account of a more thorough examination of this and allied phenomena. Experiments conducted with a number of different liquids showed that mixtures can be arranged in two distinct classes. Of the first a mixture of water and ether is an example: when shaken up together they mix, heat is *evolved*, and a *diminution* of bulk takes place. If any excess of ether present is poured off, and the lower clear liquid *heated* in a sealed tube, it becomes turbid owing to the separation of the ether. This is accompanied by an *increase* of bulk and *absorption* of heat. Triethylamine and water and diethylamine and water are mixtures belonging to this class; the temperature of separation is a function of the ratio in which the two liquids are present. A typical case of the second class is a mixture of alcohol and bisulphide of carbon. These mix with one another in all proportions above 0° C. with *increase* of bulk and *absorption* of heat. Upon being *cooled* to about − 17° C. they separate. The separation of a mixture of ether and water and of a mixture of alcohol and the bisulphide was shown. In these cases the action is regarded as a chemical one, and generally an excess of one liquid or the other is present. To determine the combining proportions two methods were used. In the first a number of mixtures of the same two liquids in different proprtions were taken, and the rise or fall of temperature produced by their mixture measured. When this was a maximum, there might be assumed to be no "dead matter" present. In the second method, which is more delicate, but more laborious, and which was used when the approximate combining proportion had been found by the first, the change of volume produced by mixture was noted; when this increment is a maximum, the liquids are present in their combining proportion. These experiments gave very concordant and definite results: for example, the molecular compound of ether and carbonic sulphide is represented by the formula $C_4H_{10}O,2CS_2$, and that of chloroform and carbonic sulphide by $CHCl_3,CS_2$. A striking confirmation of this view is afforded by the behaviour of the vapour-tension of a mixture. The temperature being constant, if the vapour-tension is plotted with the percentages of the more volatile liquid as abscissæ, the curve is, for a mixture of two liquids which have no chemical action upon one another, as the iodide and bromide of ethyl, a straight line. For ordinary mixtures, however, this is not the case. A curve is obtained in which there is observable at a certain point an irregularity. The corresponding abscissa indicates the molecular combination found by the previous experiments.—Dr. C. R. Alder Wright read a paper by himself and Mr. C. Thompson, on voltaic and thermo-voltaic constants. In a former paper the authors had stated that in a cell set up with two metals immersed in pure solutions of their corresponding salts, a given increment in the strength of the solution surrounding the metal acquiring the higher potential causes an increment (*a*) in the E.M.F. set up (*e*), while an increment in the strength of the other solution causes a decrement (*b*) in the E.M.F. This law is now substantiated; it is, however, found that for dilute acids, instead of metallic salts, (*b*) may be negative. The authors also find that it is possible to represent the E.M.F. of a cell by the difference of two quantities which they term the voltaic constants. These are quantities, one relating to each plate and its surrounding liquid. The voltaic constant of a metal and a liquid is a function of the nature of the metal surface, the strength of the solution, and the temperature, but is independent of the opposed plate and its liquid; it is practically defined as the E.M.F. set up when opposed to a zinc plate in a solution of the corresponding salt of the same molecular strength. The authors further conclude that the E.M.F. of a given combination usually *stands in no simple relationship to the chemical action taking place in the cell*, but that it may be expressed by the sum of the mechanical equivalent of the chemical action per electro-chemical equivalent, and the difference of two quantities, one being related to each metal and its surrounding liquid, and being constant for that metal and liquid termed *thermo-voltaic constants*. This thermo-voltaic action may act with or against the chemical action in producing E.M.F. In some cases, as in that of a cell composed of iron in ferrous sulphate and cadmium in cadmic sulphate solutions, the E.M.F. is against and greater than that produced by chemical action; consequently the cell works backwards with absorption of heat. At the close of the paper Prof. Ayrton and Dr. Guthrie remarked upon the apparent exception here shown to the second law of thermodynamics.

PARIS

Academy of Sciences, November 3.—M. Rolland, President, in the chair.—Observations of the new planet 244 made on October 22 to 24 with the equatorial *coudé*, with remarks on the efficiency of this instrument, by M. Lœwy. The author gives a full account of the performance of this equatorial, which has now been installed in the Paris Observatory for the last two years. His opinion of its excellent qualities is supported by the testimony of Dr. Gill and Mr. Norman Lockyer, the latter of whom pronounces it one of the instruments of the future.—A first study on the parallax of the sun, by M. Bouquet de la

Grye. This paper is based on the calculations made in Mexico by the author and M. F. Arago during the late transit of Venus. From the measurements then taken there results a mean parallax of 8·76 with an apparent approximation of 1/100 of a second.—Studies made at the Physiological Station on the locomotion of men by means of the odograph, by M. Marey. These studies have been undertaken mainly with a view to practical results. One of the objects has been to determine the most favourable conditions under which military forced marches can be accomplished most rapidly and with the least expenditure of muscular energy. The paper is accompanied by two illustrations, showing the readings of the odograph for a man walking at the rate of sixty paces per minute, and the curves of velocity and of the length of stride under various conditions.—A fresh contribution to the study of the Permian reptiles, by M. A. Gaudry.—Note on complex numbers, analogous to the quaternions of Hamilton, by M. H. Poincaré. The various problems connected with this subject are reduced to the following: to find all the continuous groups of linear substitutions variable to *n*, whose coefficients are linear functions of *n* arbitrary parameters. This problem is here dealt with.—On the involution of higher dimensions, by M. N. Vanecek.—On some general properties of algebraic surfaces of any degree, by M. Maurice d'Ocagne.—Note on algebraic equations, by M. Berloty.—On the conditions of equilibrium of a liquid mass subjected to electro-magnetic action, by M. G. Lippmann.—Conditions of a helicoidal element for the maximum of efficiency in a screw propeller, by M. Ch. Hauvel.—A comparison of the weighted thermometer with the tubular thermometer, by M. Em. Barbier. The author presents a fresh proof of the proposition already demonstrated by M. Regnault, that if the two instruments agree at the two fixed points, they remain in agreement at all fixed temperatures.—Description of two portable electric lamps, invented by M. G. Trouvé. The author, who gives two illustrations, describes two types of electric lamp, one suited for domestic purposes, the other for workshops, factories, mines, &c. Superiority over all others is claimed for both, on the ground of lightness, portability, convenience, and absolute security even in the most explosive atmospheres.—On the decomposition of the oxide of copper by heat, by M. E. J. Maumené.—Experimental researches on the temporary preservation of various virulent agents in animal organisms, where they remain in a quiescent state, by M. G. Colin. From these experiments it appears that the virus, in passing to animals where it is harmless, may preserve its properties intact for one or two weeks even under unfavourable conditions. It is also shown that in certain refractory cases the virus may give rise to serious and even fatal disorders without any apparent analogy to those caused by it in normal subjects; and further that the same animals may serve several times at varying intervals for the transmission of the poison, although a first inoculation may not have produced in them the attenuating effects of vaccination.—On the employment of the sulphate of copper for the destruction of mildew, by M. P. de Lafitte.

BERLIN

Physiological Society, October 31.—Herr Aronsohn presented a report of experiments which he had instituted in conjunction with Herr Sachs, and which had led to the discovery of a thermal centre in the cerebrum. Starting with the idea that in consequence of a diabetic prick of the medulla oblongata an increase of temperature would manifest itself in the liver, and finding by experiment no confirmation of this conjecture, Herr Aronsohn pushed his investigations for other thermal centres in the brain, and in the course of these researches came upon a spot where, on wounding it with a needle, a very considerable rise of temperature quickly set in. The speaker was not able to specify more exactly the spot at which it was necessary to make the prick in order to produce this effect. It was at all events certain that it was rather limited, and should be determined by more minute anatomical examinations of a number of brains of animals preserved in chromic acid after being operated on. Equally deep pricks made at every other spot of the cerebrum had either produced no effect on the temperature of the body, or had lowered it somewhat. In all the successful cases the corpus striatum was pierced by the needle; in all the unsuccessful cases the corpus striatum remained untouched. There was yet, however, no warrant from this circumstance to conclude where the exact seat of the thermal centre was situated. —Dr. Rawitz described some observations he had made with reference to the copulation of snails, a subject which had hitherto no been investigated. He further communicated from his own experience that snails (*Helix pomatia* and *hortensis*) could, in a state of captivity, be fed on paper. Dr. Kossel confirmed this statement from his own observations, and related that, on feeding snails with highly calcareous paper, abnormal calcareous deposits were observed in their monstrously developed shells.

VIENNA

Imperial Academy of Sciences, October 9.—Preliminary communication on monocyclic systems, by L. Boltzmann.—On the anatomical process of tabes dorsalis, by A. Adamkiewicz.—On double refraction of light in liquids, by E. von Fleischl.—On the comets recently discovered by Barnard (Nashville) on July 16, and by Wolf (Heidelberg) on September 17, and on their ephemerides and elements as computed by K. Zelbr at the Vienna Observatory, by E. Weiss.—On the development of the walls of arteries, by B. Morpurgo.—On the perception of sound, by E. Bruecke.—On the action of benzoyl-hyperoxide on amylene, by E. Lippmann.

STOCKHOLM

Society of Natural Sciences, October 18.—Prof. Sandahl, President, in the chair.—On foreign physiological institutions, by Dr. Tigerstedt. Referring to the development of physiology during recent years, the speaker described some of the principal institutions abroad, having visited forty of this kind. A similar one, on a smaller scale, was being established at the Carolina Institute in Stockholm.—The President, announcing the death of Dr. Regnell, the Brazilian Mæcenas, referred to the valuable botanical collections he had presented to the Upsala University.—Prof. Aurivillius exhibited a collection of butterflies, preserved by Herr E. Holmgren by removing the intestines and inflating the specimens. They were in splendid condition, the colours being particularly bright.—On the habits of the eider-duck and the dottrell, by Dr. Sundström. The speaker stated that careful study had proved that the eider-hen does not, as is so generally supposed, take her young during the summer into the ocean, but remains among the islands on the coast. The bird had greatly increased in the south of Sweden during the last few years.—On thunderbolts, by the same.—Herr Neves reported the receipt from Finland of eggs of the eagle, *Aquila clanga*, and the snipe, *Terekia cinerea*.

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