THURSDAY, JULY 17, 1884

PROFESSOR TAIT'S "LIGHT"

Light. By P. G. Tait, M.A., Sec.R.S.E., formerly Fellow of St. Peter's College, Cambridge, Professor of Natural Philosopy in the University of Edinburgh. (Edinburgh: Adam and Charles Black, 1884.)

THE issue of Prof. Tait's book on "Heat," recently reviewed in these pages, has been quickly followed by that of his book on "Light."

The book we are told is "not designed for those who intend to make a special study either of theoretical or of experimental optics, but for ordinary students who wish to acquire familiarity with the elements of the subject;" and its author has, as was to be expected, made it most interesting and suggestive.

The first three chapters are introductory and explanatory; we wish more space could have been found for the subject dealt with in Chapter II.—the theory of vision and colour perceptions; students' notions in general are so hazy on these points, and they are not well treated of in most of the books we are acquainted with. They fall between two stools; the physiologist considers them to belong to physics, the physicist to physiology.

In Chapter IV. the usual division of the subject into geometrical and physical optics is made. "For the explanation of the ordinary phenomena of light," says Prof. Tait, "even with accuracy sufficient for the construction of the very finest telescopes and microscopes, it suffices that geometrical optics based on laws *nearly* verified by experiment be followed out to its consequences. The residual phenomena then come in to be treated by the undulatory theory." Geometrical optics then is in the main the subject of the next 140 pages of the book, the remaining 106 pages being chiefly devoted to physical optics.

The geometrical part is excellent, and it, along with the whole book, has been rendered much more interesting by the frequent quotation, when describing some of the most important steps in the subject, of the actual words of the authors, or at all events of a close paraphrase of them. Thus Newton's celebrated discovery of the dispersion of white light is given as a quotation from his letter to Oldenburg. We in the present day gain much by reading the very words of the heroes of science, and learning with what feeble instrumental means they made their great discoveries; there is a tendency, which it is well to check, to think that nothing can be done in the way of scientific observation and discovery without the most elaborate instruments. Fresnel's apparatus in his country-house at Matthieu was the work of the village blacksmith, and Newton needed only his prisms, a measuring tape, and some screens to prove that "light is not similar or homogeneal, but consists of difform rays, some of which are more refrangible than others."

The whole chapter on refraction—Chapter IX.—and specially that part of it which deals with refraction through a prism (§ 129) and the relation between the deviation of a ray and the angle of incidence (§§ 125, 132, &c.) is particularly clear and good. The proof of the law that the deviation of a ray passing through a prism in a principal plane is a minimum when the angles of incidence and emergence are equal (§ 134) is much more elegant than any we have seen before.

But for all this we confess to a strong feeling of regret when we read the statement of the author, quoted already, as to the sufficiency of geometrical optics, a feeling which was greatly intensified when we found how little use was to be made of the all-important law of least time stated for reflection in § 85, or in the more general form in § 82, viz. "If l be the length of the part of a ray which lies within a medium whose refractive index is μ, the sum $\Sigma \mu l$ is the same for each ray of the group between any two wave surfaces;" that is, according to the undulatory theory at least, the time is the same along all rays from one wave surface to another of the same system.

For the fundamental formulæ for mirrors and lenses can be deduced by a simple geometrical method from this law; this Prof. Tait points out, while the method has the great advantage that it can be extended readily to include problems in which spherical aberration is considered. Lord Rayleigh's investigations in optics, published recently in the *Philosophical Magazine*, are a distinguished example of its powers. The full treatment of spherical aberration is of course outside the limits of Prof. Tait's work, but still it would have added to its completeness had the book contained some elementary examples of the use of the method in question. Besides the law is the real link between geometrical and physical optics. That *one* ray of light can exist only when accompanied by other contiguous rays is a fact on which the teacher can scarcely too often insist, and the solution of problems in geometrical optics by the method of least time forms the best introduction to the important principle of interference required in physical optics. Prof. Tait's method is admirable for a book on "Geometrical Optics"; in a book on "Light," however, we look for something more than he gives us on the connection between geometrical and physical optics; and the development of some of the elementary consequences of this law of minimum time seems to us to afford the most suitable opening for considering that connection.

Chapter XII. on absorption and fluorescence, is made specially interesting by the quotation of an account of an experiment of Fox Talbot on anomalous dispersion, made about 1840, but not published till 1870. The method he adopted to obtain prisms of a substance showing anomalous dispersion is very beautiful and ingenious. The account is too long for quotation; we must refer the reader to Prof. Tait's book, p. 156.

The fundamental difficulty of the undulatory theory—the rectilinear propagation of light—is assailed in Chapter XIII., and in dealing with this subject we think that Prof. Tait gives too much credit to that great physicist, Huyghens. He was of course the first to give the undulatory theory definite form, and *if we allow him to make one great assumption*, he explained perfectly correctly the rectilinear propagation, the reflection, and the refraction of light. But in Huyghens' work there is an assumption the importance of which it is impossible to overlook.

"What Huyghens did not see sufficiently clearly," says Verdet ("Optique Physique," tome i. pp. 33, 34), "was why each of the elementary waves is only effective at the point in which it touches the envelope. He contented himself with saying that the movement which exists over each of the elementary waves must be infinitely feeble compared with that which exists over the envelope," while he remarks that "this must not be examined with too much care or nicety"—"recherché avec trop de soin ni de subtilité" are his words. Nor was Huyghens in a position to give the necessary demonstration, for, to quote again from Verdet, "He never supposed that there was any general relation between the movements of these successive waves, he never combined their effects, and in particular the notion of the constant interference of two sets of vibrations bringing to one and the same point movements of opposite phase is absolutely foreign to him."

Now it is this notion of interference, which is entirely due to Thomas Young, combined with the excessive smallness of the wave-length of light that renders Huyghens' assumption correct.

"It is to Young," says Verdet, "that the honour belongs of having first applied to optical phenomena the principle of interference," and Prof. Tait recognises to the full Young's claims to this distinction; the point on which we would insist is that this principle is needed for the elementary explanation of the rectilinear propagation, the reflection, and the refraction of light, as well as of diffraction and the colours of thin plates; the principle is due to Young, and Huyghens' explanation rests on an assumption which he did not prove.

Prof. Tait regrets in his preface that his book was all in type before Prof. Stokes's "Burnett Lectures" appeared. We will quote a few lines from them bearing on the point. After referring to Huyghens' principle, he says (p. 19):—"This principle does not by itself suffice for the explanation of rays. It proves, or at least appears to prove, too much. It is as applicable to sound as, on the supposition that light consists in undulations, it is to light; and if Huyghens' explanation of rays were complete, there ought equally to be rays of sound, and sound ought to present the same sharp shadows as light."

As Young received in his own day the most unjust treatment at the hands of the leaders of scientific opinion, it is but fair that the full importance of his work should be made clear, and that he should be given all the credit he so richly deserves. In Chapter XIV. Prof. Tait brings out and illustrates with his usual force and vigour the value of Young's principle in explaining the phenomena of the colours of striated surfaces and of thin plates. Young's own attempt to account for the diffraction effects produced by a wire or straight edge was incorrect; it was left for Fresnel in his great memoir on diffraction to show that they too followed as a direct consequence of interference.

It is of course impossible to give without the aid of analysis a full explanation of the phenomena of double refraction, and so Prof. Tait contents himself with showing how Huyghens' construction, combined with the fact that light-vibrations lie in the wave front, enables us to account for many of the observed facts.

When considering the subject of polarisation by reflection (§ 268, &c.), we miss any reference to the experiments of Jamin and others, besides Brewster, on the reflection of light from transparent media. Jamin has shown that Brewster's law requires some modification, for in general there is no angle at which light is *completely* plane-polarised by reflection from a transparent surface. For substances in which the refractive index is about 1·4, an angle of complete polarisation exists, but only for these.

A short chapter on Radiation and Spectrum Analysis concludes the book. R. T. GLAZEBROOK

OUR BOOK SHELF

A Pocket-Book of Electrical Rules and Tables. By John Munro, C.E., and Andrew Jamieson, A.M.I.C.E., F.R.S.E. (London: Charles Griffin and Co., 1884.)

COLLECTIONS of rules and tables adapted to the wants of civil and mechanical engineers have existed for a considerable time, and now that practical applications of electricity are becoming so many and important, a want has been felt of a useful hand-book for those engaged in this comparatively new branch of engineering. The "Pocket-Book" before us is intended to supply this want, and in many respects it does so very well indeed. It is neat in appearance, handy in shape, and contains much information in the form of tables of practical data, useful rules and recipes, and specifications and directions as to the performance of many different kinds of work.

But although doing good service to electrical engineers by collecting together so much that is useful in the form of practical results, the compilers have, in their endeavour (a mistaken one we think) to render their manual a guide also in points of theory, fallen into many errors which render the book an unsafe one to put into the hands of any one who is capable of being misled in such matters. In the first place there are many—we cannot call them typographical—mistakes in equations given in different parts of the work. As the process by which these equations are obtained is not given, and the formulæ are intended for reference and to be used in computation, there is nothing to warn an inexperienced user of the work of possible danger. The errors might, however, be excused in a first edition if it were not that the formulæ in question are simply inaccurate copies of results given in other works. For example, at pp. 123 and 125, a single glance at "dimensions" is enough to show that several of the formulæ for the localisation of faults in aërial telegraph lines are erroneous, and the same remark applies to the formula given at p. 174 for the calculation of the distance of a fault in a submarine cable from the testing station.

As to the more theoretical portions of the work, we have first a chapter headed "Definitions of Units." This is in great part taken *verbatim* from Prof. Blyth's new edition of Ferguson's "Electricity." An alteration on Prof. Blyth's statement is made on p. 10, and confounds the well-known and perfectly definite velocity v with the velocity which is the proper expression of any given resistance in electro-magnetic measure. Again, at p. 13, v is said to be the ratio of the *electro-static* to the *electro-magnetic unit* of quantity. The "derivation" of the practical units—volt, ohm, ampere—given in the table at p. 13 is a perfect maze of vicious circles, and in the same table the "joule" is given as alternatively "volt × coulomb" and "ampere^2 × ohm,"—the confusion, which would seem inveterate, between work and activity or rate of working. The velocity of light, we may remark, is given at p. 11 as 3×10^{10} cm. per second, and at p. 382 this is given as the "French value" of 192,000 miles per second! Here, as elsewhere in a few cases (the values of g given at p. 42 for example, where, besides, g is expressed as a *velocity*), numbers evidently culled from different sources and supposed to express the same quantity in different units are given without verification of their equivalence. In p. 43 the venerable pendulum formula is terribly misprinted and *g*, without its being so stated, is taken in *inches* per second per second.

A chapter headed "Testing of Electric Light Dynamos, Accumulators, and Transmission of Power," given at pp. 176-86, is not at all satisfactory. The tests for the efficiency of a secondary battery are neither clearly nor fully given, and the score of lines devoted to this important subject close with a most remarkable sentence, which we quote :—"The total work done in charging and discharging may also be measured by a suitable voltameter joined up as a shunt to the secondary battery, so as to pass a known fraction of a current through it."

At p. 373 there are two or three misprinted formulas, but in the first line of p. 374 we have perhaps the most extraordinary equation ever given in a work on electrodynamics. The difference of potential at the terminals of a dynamo (a shunt-dynamo we presume is meant) is there stated to be equal alternatively to the product of the current in the field magnets multiplied into their resistance, to the current in the external circuit multiplied into the resistance of the external circuit, and *to the current in the armature multiplied into the resistance of the armature!*

The calculation (p. 345) relative to the electrolytic decomposition of copper sulphate involves also serious theoretical errors. Mr. Jamieson, multiplying together the electro-chemical equivalent of copper and the heat of combination of copper and oxygen, makes the "electromotive force required to deposit copper from a solution of sulphate of copper" to be ·836 volt. In the particular case, not however referred to, of a cell having a platinum anode and copper kathode, this would be the approximate electromotive force required on the cell to produce electrolysis. But the author actually goes on to use this result as the electromotive force required on an ordinary electro-plating bath to effect the electrolysis, and bases on it some conclusions as to the efficiency of a Siemens machine depositing copper in commercial work, or in the stereotyping of ordinary printed matter. In these cases of course both anode and kathode are copper plates, and the calculated electromotive force has no application whatever.

A single remark on another subject we would make before taking leave of this work. In many places where the compilers are under obligations to other authors due acknowledgment is wanting. For examples we may refer to several parts of the chapter on submarine telegraphy, to pp. 369-76 on dynamos and transmission of power, and to part of p. 403, where, by the way, the very serious errors inherent in the method of determining (?) the intensity of a magnetic field by counting the oscillations of a magnetic needle are not alluded to.

In conclusion we have no hesitation in saying that with a careful weeding of the tables, minute verification and correction of the algebraical work, deletion of a good deal of the "theory" given, and lastly, copious references to original sources, both as a matter of convenience to the user and of literary justice, this "Pocket-Book" will be made a very valuable *vade mecum* for electrical engineers. As it is, it will no doubt be found of service, but, as we have indicated, its statements must on several subjects be received with caution.

A. GRAY

The Non-Bacillar Nature of Abrus Poison. By C. J. H. Warden, Surgeon I.M.S., and L. A. Waddell, Surgeon I.M.S. (Calcutta, 1884.)

THIS pamphlet is an exhaustive treatise on the nature, physiological and chemical properties of the seeds of *Abrus precatorius*, called Jequirity by the South Americans, and used to cure granular lids. As is now well known through de Wecker of Paris and Prof. Sattler, this popular remedy of the South Americans produces, when used as an infusion and applied to the conjunctiva, severe ophthalmia, in the course of which granular lid (trachoma) is brought to cure. In India it is used by the natives for subcutaneous injection into cattle, wherewith to produce a kind of septicæmia and death. The nature of the poison has been thought by de Wecker and Sattler, and later by MM. Cornil and Berlioz, to be due to a bacillus (the Jequirity bacillus), the spores of which are derived from the air; and, although harmless at first, assume pathogenic properties when grown in an infusion of the Abrus seeds. It has been conclusively proved, however, that this is not the case, that the active principle of the Abrus seeds is present before any contamination with the bacillus could have taken place, and further, that the Jequirity bacillus, when freed from the infusion, possesses no power of producing ophthalmia.

Messrs. Warden and Waddell have carefully examined the chemical nature of the seeds, and they find that the active principle, abrin, is a proteid, closely allied to native albumen, and obtainable not only from the seeds, but also from the root and stem.

E. K.

A Text-Book of Pathological Anatomy and Pathogenesis. By Ernst Ziegler. Translated and edited for English students by Donald Macalister, M.A., M.B., &c. Part II. Special Pathological Anatomy, Sections I.-VIII. (London: Macmillan and Co., 1884.)

THE enormous success that has attended the first part of this work will, we feel sure, in no way abate with the present volume. Like its predecessor it is a masterly exposition of all that is known concerning the pathological anatomy of the parts treated. In this last volume the special pathological anatomy of the blood and lymph, the vascular mechanism, the spleen and lymphatic glands, the serous membranes, the skin, the mucous membranes, the alimentary tract, the liver and pancreas, are described with great clearness and thoroughness. The subjects are treated in a detailed and systematic way, without incumbrance with self-understood details. The illustrations are very admirable, and while not profuse, are nevertheless thoroughly representative. The bibliography, particularly of the more recent works, is, in the English edition, thanks to Dr. Macalister, a most valuable improvement on that in the German edition. While a help to the learner, it will no doubt prove also a valuable companion to the teacher.

E. K.

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 Late M. Dumas

I HAVE received from M. Pasteur, President of the Committee, a letter informing me that it is proposed to erect a statue to the memory of Dumas at Alais, his native town. The name of Dumas is so prominent in the history of our science that no words of mine are needed in support of such a proposition, and I merely express the hope that many English chemists will be willing to contribute to this memorial. Subscriptions will be received by the secretaries of the Chemical Society, Burlington House, Piccadilly.

W. H. PERKIN, P.C.S.

The Cholera Germ

THE latest enunciations of Prof. Koch from Toulon and Marseilles concerning the relation of his "comma-shaped" bacillus to cholera are so contradictory, that it is worth while to take notice of them.

Koch, as was mentioned in your last issue (p. 237), maintains that the "comma-shaped" bacillus is the cause of cholera; and finding it in the alimentary canal only, but not in the blood and tissues, of persons affected with cholera, he is necessarily forced to assume that the alimentary canal is the exclusive organ into which the cholera poison enters and in which it has its breeding ground. On the other hand, Koch has ascertained that the "comma-shaped" bacillus is fatally affected by acid. This result, having been established by direct experiment, is naturally perfectly trustworthy, and is, besides, in complete harmony with what is known of other bacilli, both pathogenic and non-pathogenic, which, as is well established, succumb to the influence of acid.

Now, these three propositions, (1) that the "comma-shaped" bacillus is the cause of the cholera, (2) that the alimentary canal is the exclusive organ of entrance of the cholera virus, and (3) that the "comma-shaped" bacillus is neutralised and killed by acid, appear to me to be in hopeless contradiction.

The first two propositions are assumptions, the third is based on direct experiment, and is, as just stated, perfectly in harmony with other observations. If, then, this third proposition be true, the other two cannot be true, that is to say, if it is true—and there can be no doubt about it—that the "comma-shaped" bacillus succumbs to the action of acid, then it cannot be true that the "comma-shaped" bacillus is the cholera virus, nor that the alimentary canal is the sole entrance of the cholera virus. How, we may ask, can the "comma-shaped" bacillus pass unscathed the acid contents and the acid secretion of the stomach? To maintain, as Koch is reported to have done, that in all persons attacked by cholera the stomach must have been previously so deranged that its contents and secretions are not acid must appear to every one who has had any experience during a cholera epidemic an untenable proposition. On the one hand, it is known that such a serious disorder of the gastric mucous membrane as the total absence of acidity is of comparatively rare occurrence, while, on the other, in every cholera epidemic numbers of persons become affected with the disease in whom such a gastric condition, antecedent to the infection, can with certainty be excluded.

E. K.

The Mountain System of the Malayan Peninsula

SOME new facts with regard to the mountain system of the Malayan peninsula may be of interest to many of your readers. In exploring through the native State of Perak I find that, in addition to the main range, which occupies about the centre of the territory and runs in a north and south direction, there are two other ranges belonging to quite different systems, and, as I think, of different geological age. The first is close to the coast. It is a series of ridges parallel to each other, but detached, having a north-north-east or south-south-west trend. These ridges are of granite, and rise to a considerable height, such as Gunong (Malay for mountain) Inas, over 5000 feet; Titi Wangsu, nearly 7000 feet; Gunong Hijau, 4400 feet, and Gunong Bubu, or Bubor, 5600 feet. The two latter I have ascended. Though they are detached from each other, they form a watershed between the coast and the inland drainage, and thus the River Perak has to drain an immense valley in a north and south direction until it finds an outlet to the south of the Dindings.

To the east of the Perak there is a small range about twenty-five miles long, perfectly detached from the other systems, and having generally a north and south direction, but sending off spurs a little west of south. This also is granite, but on its lower shoulders has thick deposits of stratified limestone, above and below which tin is worked. To the north this range is bounded by the valley of the River Plus, which here joins the Perak, and to the south by the mouth of the Kiuta. The latter river runs in a valley to the east of this range, and where it ceases joins the Perak. To the east of the Kiuta again comes the main range with many peaks over 7000 feet high; Gunong Riam probably reaching over 8000 feet.

The first series of ranges have their origin in the State of Keddah, just where the Malayan peninsula begins to widen out. This widening out is entirely due to this mountain system. The island of Penang is a part of it, and so are the islands called the Dinding Group. Were the coast to subside about 300 feet, we should have a narrow peninsula fronted by a series of large and very elevated granite islands having their longest diameter north-north-east and south-south-west. The second mountain chain has a different direction, and nowhere rises above 3000 feet; but both ranges are rich in tin. The first series has at its base Palæozoic schists, slates, and clays. The second has limestone. The Palæozoic rocks are rich in tin at the junction with the granite. The tin in the second range lies above and below the limestone, and has been derived from the older formation. The Palæozoic clays resemble very closely the gold-bearing slates and schists of Australia. To the south they are nearly denuded away, but in Lower Siam, from specimens I have seen, they are full of auriferous quartz reefs.

It is singular that in this mountain system we have the closest resemblance to the tin-bearing districts of north-eastern Australia. When exploring geologically the Wilde River district in 1881 and the Daintree River in 1879, I found that the sources of the tin were in detached granite mountains or groups of mountains—granite islands, so to speak, much higher than the present watershed of the country, but, being detached from each other, allowed the rivers to pass round and between them. I have referred to the same thing in Tasmania in my account of the physical geology of that country. Geologists in England can say if there is any resemblance to this state of things in the tin-bearing granites of Cornwall. I am inclined to think that we have in these rocks the remains of a former and very ancient mountain system.

I may add that it is a pity that we still find in recent books of high authority the statement reiterated that the highest mountain on the Malayan peninsula is Mount Ophir, near Malacca (4360 feet). Here are the heights of a few in Perak:—Slim Mountains, 6000 or 7000 feet; Titi Wangsu, 6900 feet; Riam, 8000 feet at least; Hijau, 4400 feet; Bubor, 5630 feet; Gunong Rampip, 7800 feet; Gunong Rajah, 6500 feet; besides many others in Reman and Pahang which have not been explored.

Arang Para, Perak, June 2 J. E. TENISON-WOODS

Chalk and the "Origin and Distribution of Deep-Sea Deposits"

IN consequence of Dr. Gwyn Jeffreys' letter, I feel it incumbent, in the interests of geology, to restate the position with regard to the question of the depth of the ocean in which the White Chalk of England was deposited. The cause that led to its deposition over a former land surface was indubitably a great though gradual depression of that area. The process commenced with the Neocomian age, when two seas encroached from north and south, until they were probably only separated by some relatively unimportant ridges or islands to the north of London. The depression seems to have been checked for a long period, but recommenced in the Gault age in a more serious manner. Now, according to Renard and Murray, the *Blue Mud*, with which I assume the Gault is to be identified, if with anything, is formed around shores and in partially inclosed seas, passing into a true deep-sea deposit at a distance from land. The limits of depth at which *Blue Mud* is formed are not stated, but the Mollusca of the Gault, if not indicating a very great depth, are quite against its being a very shallow-water formation. There are several deep-water genera, such as *Neæra*, *Leda*, *Limopsis*, *Cadulus*, *Dentalium*, *Eulima*, in it, and I believe that when the smaller Mollusca from it have been reinvestigated by the light of our present knowledge, a far greater similarity between them and deep-water forms will be apparent. The Gault also contains a very large number of Foraminifera and several Encrinites and other Echinoderms, which are not, I believe, characteristic of long-shore deposits; while there is a remarkable absence in it of the more distinctly shallow-water shells that abound in the Neocomian, and it has none of the coarser fragments of rock, 2 cm. in diameter, which are stated in Renard and Murray's paper to occur in the near-shore muds. We must assume a considerable depth of water for the Lower Gault—what depth I would be well pleased to leave to Dr. Gwyn Jeffreys to say. Now, if there is one fact more apparent than another, it is that the Upper Gault represents a deeper sea than the Lower, and therefore that the depression was maintained. The *Blue Mud* is replaced in neighbouring areas by *Green Muds and Sands* with Glauconitic grains which apparently are deposited in similar depths or situations; but the limit of depth at which either the one or the other is dredged is not stated. A still continued depression takes us through *Green Mud* to the Chalk Marl, which apparently is a true Globigerina ooze[1]; and this passes into a true White Chalk. The White Chalk is the result of still farther depression, for it overlaps the other deposits, and as the great change in the character of the sediment cannot have been due to the shallowing of the sea, and yet must have had a cause, we must conclude it was due to deepening. Its enormous extent and thickness and great purity proclaim it in fact to be an oceanic deposit, and there does not appear to be anything with which it can be compared except Globigerina ooze.[2] The White Chalk of England could therefore only have been deposited under the conditions of depth, or remoteness from land under which the deposit of Globigerina ooze is possible. If Globigerina ooze is not a "terrigenous" deposit, Chalk is not, and it does seem singular that it should be classed as such by Renard and Murray, unless indeed they are prepared to point to an area in which a similar terrigenous formation is taking place at the present day. If genera now confined to shallow-water are present in it, it only proves that there must have been deep-water representatives of those genera in the Cretaceous ocean. This is in fact probable from at least two considerations, the one that the examination of the abyssal fauna is still relatively "extremely slight and cursory," as Dr. Gwyn Jeffreys has so amply admitted in the address he refers me to. It is probable that thousands of casts of the dredge have been made in the littoral zone for one in the abyssal zone, and we are, therefore, not in a position yet to say that any genus may not have representatives in the latter. The second consideration is far more important. Dr. Gwyn Jeffreys states that "all of them (the Cretaceous Mollusca) are evidently tropical forms." Now there is strong evidence from the present distribution, and the deposition of the Chalk, that the sea did not communicate with Arctic seas. Prof. Prestwich, in his anniversary address to the Geological Society, pointed this out in 1871. But even if it had, the Arctic climate during the Cretaceous period was a warm one, and for these two reasons, or either of them, the abyssal depths of the Chalk ocean were probably higher in temperature than they are now, while the temperature of the more littoral zones may have been almost tropical. Heat and cold seem greater factors in the distribution of Mollusca than depth of water. Relatively cold-loving genera or species of genera that could only have found the necessary temperatures then at great depths, may now find suitable habitats in shallow water. The "tropical Mollusca" of the Chalk might for this reason have been able to live at much greater depths when such were warmer, but are of necessity now restricted to those in which suitable temperatures are to be met with; and since these are now all relatively shallow, Dr. Gwyn Jeffreys may be quite right in saying that these extinct species have a shallow-water facies, without our being obliged to accept his inference that the Chalk sea was a shallow one. But if we accept the Mollusca pure and simple as a test of depth, their evidence as adduced is untrustworthy owing to the association together of those of the Gray Chalk and the Irish Chalk band of Kilcorrig. Eliminating these, we have no patelloid shell left but *Hipponyx*, and the Chalk species was completely different in habit to anything living. I do not know the Chalk *Chama* (if the Irish form, this is a limpet) or *Pinna*, and these must be very rare and even pos-ibly drifted shells. The unextinct characteristic genera are in fact reduced to *Terebratula*, *Lima*, *Pecten*, *Armussium*, and *Spondylus*, and of these all but the latter are stated, in the address I am referred to, to have been met with in water 1450 fathoms deep.[3]

It would be impossible to dispose of a question of such importance in a mere letter. My object in writing is to elicit, if possible, the exact grounds on which Messrs. Murray and Renard base their statement that the Chalk was a shore deposit; and it would also be exceedingly useful if Dr. Woodward, Dr. Duncan, Mr. Davidson, and Mr. Carpenter would give their opinions, and the grounds on which they are based, on the probable depth required by each of the Cretaceous groups, respecting which they are the chief authorities. Dr. Gwyn Jeffreys is the only one who has contributed anything definite towards a solution of this most important geological problem, and for this, while believing other conclusions may be deduced than those he has arrived at, I and many other geologists heartily thank him. J. STARKIE GARDNER

[1] Globigerina ooze is mainly composed, according to Murray, of 40 to 95 per cent. of carbonate of lime, oxides of iron and manganese, and argillaceous matter.

[2] Of reef-building corals there is not a trace either in it or in any contemporaneous formation, and nothing can be more opposed to all evidence than the supposition advocated, it will be remembered, by Wallace in "Island Life."

[3] There are few traces in the English Chalk of any Mollusca except those that possessed calcite shells, and what the rest were like as a group no one can say.

Animal Intelligence

THE following notes of facts observed in New Zealand may be thought of interest; in some way they may serve to illustrate Mr. Romanes' work on "Animal Intelligence": they are submitted without making an attempt to distinguish where they may overlap the fine line between instinct and intelligence. Cases which may show apparent intelligence or the reverse are recorded, that we may arrive at a clearer view of the truth in animal life.

The dog cannot be passed over without mention; he is always to the fore where intelligence is required. Here, where sheep occupy so large a share in the employment of country people, the colly may be seen daily exhibiting its wonderful talents in controlling the movements of its simple charge. Its achievements are too numerous for recital.

Amongst birds we found the quail-hawk (*F. novæ-zelandiæ*), quickly learnt to avail itself of the property of the new settlers; it attacked both poultry and pigeons with the greatest determination directly these foreign birds appeared at the stations and outlying farms. The harrier (*Circus approximans*), more stealthy than the falcon in its depredations on the poultry, perhaps not less destructive, is careful if possible to convey its prey to a quiet spot free from interruption, where its meal can be finished at leisure in security. It found out the use of cornricks and haystacks as mouse-preserves; in some places several harriers might be seen at one time perched on the thatch carefully watching for vermin. It killed the rabbit; the swift-footed hare it found out could be hunted to best advantage in company: several of them would join in the pursuit, wheeling softly with every double of the distressed animal, till, quite exhausted, it lay stretched out in death. The harrier, the gull, the tern, all used to put in their appearance after the large grass fires of former days had swept miles of country; lizards, as they crept from under the stones laid bare by fire, seemed the attraction for all these birds. One autumn, when laid up with rheumatism, lights were brought into the room rather early. I often heard the sound of scratching on the window-glass, and found it proceeded from the efforts of an owl (*Athene*, *N.Z.*) to secure moths from the lighted-up window-panes; this was repeated for many evenings during parts of the months of April and May, so that I always expected my evening visitor. As a mouser this same species learnt the value of stations near barns and stacks; frequently, many scores of times, have I seen it keeping its solemn watch on a post or rail of the barnyard (see *Zoologist*, 1873, p. 3621). The kakapo or night-parrot (*Stringops*) shows intelligence in its nesting arrangements: the chamber at the end of a long tunnel is covered at the bottom with a great accumulation of excreta; each of these is an inch or more in diameter —the bird is a vegetable feeder—the warmth derived from this mass is secured by the young, reversing the proverb, "It is an ill bird that befouls its own nest."

The kea (*Nestor notabilis*) (see NATURE, vol. iv. p. 489). Its rapid development of a change of habit that led it to destroy sheep has proved very disastrous to many mountain sheep-farmers. It is remarkable that the discovery of the excellence of kidney fat should become known almost simultaneously through a long tract of country; how were beginners instructed to dig their beaks into the wool just above the sheep's kidneys? Horses have been wounded by them in the same part; all this shows a ready means of spreading information. One of the writer's sons snared a few fine specimens, but they very soon became aware of the snare and promptly avoided it. When thrown at, they learn to dodge the stone, just ducking or moving aside. One, imprisoned under an inverted bucket, after a time thrust its strong beak between the rim of the bucket and the floor, turned over the bucket and escaped.

The two cuckoos *Eudanymis* and *Chrysococcyx* offer a problem of peculiar interest as regards migration. The journeys they undertake and accomplish across wide expanses of ocean are amongst the most courageous and trying physical feats in bird history: "as bold as a hawk," "as brave as a gamecock," are proverbs that are befitting; but these birds deserve as much recognition for their adventurous daring.

When either of these species is observed flying, it will be noticed that the wings are kept constantly in rapid motion; there is no sailing or soaring, gliding through the air on still expanded pinions, but the bird is sustained by determined work. Here in the South Island they are to be seen from the end of the first week in October (further to the north earlier) till March, or even April; the remainder of the year they are not seen in this country. Every part of the islands is known to the Maories; there is no district where they could winter without the fact becoming known. The whistler, or shining cuckoo (*Chrysococcyx*) has been observed in the month of October at Te Wakaru, Chatham Isles, on the beach and in trees exhausted, wet as though from spray; looking at the period of its arrival, and remembering that Te Wakaru is the north-east corner of the large island, it points to the probability that some birds arrive there direct from the warm and distant north, and not from New Zealand, from which the Chatham Island group lies easterly from Cook's Straits about 500 miles. From my own observation I am inclined to believe that with this species the first emigration sets in from the west coast of the South Island of New Zealand about the end of December, as I have observed adult birds in numbers on the sand dunes close to the sea, probably preparing for departure. They make use of the warmest domed nest of our native insect-eating birds, the very rare exceptions afford but two or at most three exceptions in thirty years' observation; in one of these cases an egg found in a nest of the blight-bird (*Zosterops*) approximated in colour those of the dupe; because it was dropped in an open nest? A good example of approximate coloration came under notice in the case of a wounded bittern which was secured and placed under a coop on a piece of grass; she laid an egg of a pale green colour; under ordinary conditions a buff egg would have very well matched the flags of dead sanpo (*Typha*) and faded water grasses, of which its nest is composed. The kingfisher (*Halcyon*) gives a good instance of cleanliness, most necessary in a close nest, containing from five to seven young birds, which remain at home until they can fly well; the entrance of the tunnel to the nest chamber is an upward slope, whilst the eggs are saved from rolling out by a ridge on the edge of the nesting place. In another species the flycatchers (*Rhipidura*) cleanliness is attended to thus: the young back themselves to the edge of the nest to void excrement, which is taken away by the old birds. In the slight nest of the wood-pigeon (*Carpophagus*) cleanliness is provided for by the open work of the structure, so that the dried excreta of the young pass through the spaces of the concave platform. The tui (*Prosthemadera*) enjoys the faculty possessed by the keas, gulls, terns, and many other species, of quickly making known events of interest, as, for instance, in a gorge of one of the great rivers, some cherry-trees rewarded the care of a settler with a fine crop of fruit; a wandering tui found this out, immediately the fruit was attacked by numbers of these beautiful birds, and the crop cleared off. The tuis had to travel some miles from a wood to the cherry-trees. Another instance of the possession of this quality could be witnessed here at the present moment (April 23); from the midst of the massive armed leaves of a variegated aloe has arisen a stately and erect column of blossom reaching upwards to a height of twenty-four feet; its bracts, between thirty and forty in number, laden with rich golden-coloured flowers spread out in formal array. A bell-bird (*Arthornis*) first discovered the richness of the nectaries of this foreign plant. Soon bell-birds and tuis assembled there, a most pleasing sight; their ever-varying motions and postures could be distinctly seen as they flitted about, darted between or hung suspended from the blossoms whilst probing for the honeyed sweets. It has become a floral play-place, a stage enlivened throughout the day with songs and aërial movements; even when the sun has retired behind the western hills, when bees have winged away to distant hives, a bell-bird or two will yet linger, as if to the last minute they would extract some luscious drops.

Since its arrival here in 1856 the blight-bird (*Zosterops*) has shown some notable changes in habit that are in accord with the different conditions under which it now lives in this country: for some years after its arrival it built a suspended, somewhat hammock-shaped nest, in which it laid three eggs; finding from experience that its nest was unmolested by snakes or other egg-robbers, it saved itself much pains and labour by commencing to fix its home in a spray. It, like the goldfinch (*F. carduelis*), freely availed itself of the sheep paddocks, and collected wool as an excellent fabric for nests very readily obtainable; I have seen nests of this species almost entirely constructed of it. One of its familiar names was conferred because it helped to clear fruit-trees of blight and other insect pests; it soon found out the excellent food that a variety of fruits afforded; when trees were netted to secure them from its attacks, it learnt to find out where the meshes of the nets were stretched to their full extent, and there made its ingress and egress to the fruit beneath. The robin (*P. albifrons*) visits conservatories for the sake of insects; we have known them make daily tours round a glass-house, waiting till flower-pots have been removed, when they have eagerly picked up the lurking insects that hid beneath, thus easily earning a hearty meal. The lark or pipit (*Anthus, N.Z.*) for a similar reason will leave uncultivated tussock land to follow the trench made by the gardener's spade, and thus get an abundant supply of the larvæ of the brown chaffer-beetle. I wish to say I do not think this a general habit of the pipit, but I have seen several of the species thus well employed.

The yellow-breasted robin (*P. macrocephala*) and the wren (*Acanthisitta*) will at times use man's buildings for their homes. Nesting material offered to this robin and to the flycatcher have been readily accepted; the latter species made use of some red cotton wool thus put in its way, but worked it up so that it was not seen from the outside. In some cases I have known the last-named neat architect to add a rim to the nest when the young required more room. The chaffinch (*F. cœlebs*[1]) here follows the traditions of its native land, tricks out the exterior of its beautiful nest with lichens, and in many cases supplements this material with fragments of newspaper, for lichen is scarce here; singularly enough this hereditary habit outweighs its sense of concealment, as it places its nest thus adorned on trees without lichen on their bark.

The sparrow (*F. domestica*) is remarkable for the ease and readiness with which it modifies its nesting habits to suit circumstances; in the very heavily topped ti palms (*Cordyline*), where the divergence of the branches is hidden by a massive thatch of long ensiform leaves, sometimes a common roof shelters many compartments; the gregarious instincts of the species are thus carried out at breeding-time; from one of these communities we have taken thirty-one eggs and fourteen young birds at once. On the shelterless "plains" it has been known to modify its old habits by building on the ground, or in heavy road cuttings its nest may be seen in a crevice of the bank, or it builds in some fissure in the cliffs over the sea, just below man's reach; it has taken possession of intricate passages in a heap of coils of fencing-wire; in this last-named instance poultry feathers for lining had to be brought from a mile distance; but then the situation promised security.

The weka (*Ocydromus*), as curious as a magpie, knows the value of a fruit-garden, and that a poultry-yard furnishes eggs. I have seen it pecking at the skin of a dead lamb with heavy blows, and the insects being driven out, it has tugged away at the decaying skin till it has been able to pick up the insects that lay underneath. The dotterel (*C. bicinctus*), red-bill (*Hæmatopus*), paradise duck (*Casarca*), all simulate lameness or distress to lead wayfarers from their young and afford them opportunity for escape or concealment.

The Australian magpie (*Gymnorhina*) has given us some noteworthy instances of its intelligence and resource under difficulties: a pair bred here for some years; one season the young were taken, the wings cut very close. Some impatient creature who could not endure constant and sudden attacks shot the male bird; the young were given away except one poor one, which turned out to be a male. In the following season the old hen was seen building very high, as usual, in a blue gum (*Eucalyptus*); there she was observed feeding young; at length a young one flew from the nest, and, when sufficiently strong on the wing, together with the old hen left the district. Now the poor male with the wings cut was never able to rise from the ground further than by jumping; he had never flown at all, as the stumps of the quills remained in the wings. This was the only male to which the hen could have had access. Whilst the hen was intent on new family cares, the crippled male died. Another pair on the plains, where sticks were scarce, availed themselves of a supply of binding-wire from a patent reaper and binder; the wire cut in lengths furnished an ample supply of lasting material for the nest.

The big gull (*Larus dominicanus*) instantly finds out a dead beast, and makes the fact known; it attacks sickly lambs or sheep that are cast by pecking out the eyes, thus securing its prey by rendering it helpless. I have seen it ascend with a shell-

[1] See *Mittheilungen des Ornithologischen Vereines in Wien*, No. 3, Marz 1884, p. 35.

fish to a considerable height, and drop it on to a shallow in one of the bays, recover its prize, and drop it again and again to obtain the fish within. Many weeks before nesting time these birds visit the old breeding-stations, as if to estimate the repairs that will be necessary to render the old nests available; this visit is carried on with great clamour. A cormorant (*Graculus*) was shot at and wounded at a tench-pond at Rockwood; it kept in the pond; it could not fly. A dog was sent in to fetch it out; it faced the dog resolutely, which turned tail; this part of the animal was immediately seized by the cormorant, who was in this singular manner towed ashore; but its odd feat did not serve to save its life. The fantail flycatcher (*Rhipidura*) enters houses in pursuit of flies glancing from room to room; it soon clears them of these insects. Dr. Otto Finsch in his "Ornithological Letters from the Pacific" mentions this habit as witnessed by him here.

Amongst hymenopterous insects the Sphegidæ offer instances of intelligence. A species of Sphex with orange-coloured body deposits the benumbed or torpid bodies of spiders in some crevice for future use. An individual of this species had its hole in a dry corner beneath the plate of a long veranda. One day I observed it dragging a victim along a gravelled walk that was parallel to the veranda; the small stones and grit made its progress very difficult. After very trying struggles with these impediments it displayed a remarkable degree of intelligence, by which it gained its ends. It altered its course and made for the veranda, ascending the smooth, painted board that adjoined the gravelled walk. After slowly traversing seven inches of perpendicular it came to a rounded beading which projected outwards. Now came its supreme moment of physical exertion. The body of the spider apparently was too heavy to render the aid of wings available. After several pauses in its progress it slowly yet surely surmounted the difficulty presented by the projecting beading, gained the level boards of the veranda, along which it travelled rapidly with its burden, which it sometimes dragged, sometimes pushed before it. By the expenditure of great exertion in surmounting the beading it gained a smooth and level run to its home of thirty-nine feet. A species of Mantis remains so still on a leaf of its own colour that it is difficult of detection; it takes its prey by surprise, darting forward its armed fore-limbs with a sudden spring.

I have had in the shrubbery a colony of Phasmæ for the last nine or ten years. In all that time they have remained almost entirely on one tree (*Oleurea Fosteri*). Yet, accustomed as I am to them, they place themselves so much in a line with the sprays of the tree that they are difficult to discern; in drizzling north-east weather some dark markings appear along their bodies, which match the wet sprays wonderfully. It should be noted that the Australian magpie, the halcyon, and many insect-eaters have for years bred and lived in the trees or banks near them; yet they still survive, notwithstanding the proximity of these enemies to insect life. T. H. POTTS

Ohinitahi

THE following extract from a letter which I have just received from Mr. J. H. Wheelwright appears to me of sufficient interest to publish in your columns, as it serves to give, among other things, a good deal of new and first-hand information on one of the most important branches of comparative psychology, viz. that relating to feral and partly wild domesticated animals.

GEORGE J. ROMANES

Cattle very easily relapse from domestication. They become distinctly nocturnal in their habits; their sense of smell is very strong. Wild cattle degenerate rapidly in size, owing, I think, to the persecution of the young heifers by the yearling bulls. In three or four generations in Queensland wild cattle revert to one uniform colour, a dun colour or dirty brown with a yellowish stripe along the spine, and a yellow nose. Wild cattle will remain all day long concealed in the depths of thick, inaccessible jungle—"bungalow scrub" or "mallee" we call it in Australia—issuing forth at night to graze and drink, and it requires much care and very hard riding to entangle a few of them among a lot of quiet cattle and secure them. Australian cattle have many habits their domesticated progenitors have lost. For instance, in summer-time grass becomes very scarce near the rivers, and the cattle walk in from their feeding-grounds as much as ten or fifteen miles to water, marching in long strings and feeding back again. Young calves of course could not do this. I have frequently noticed two or three cows far out on the plain, who, when they saw me, would lift their heads and watch me. Presently I would come across a kind of *crêche*, a mob of perhaps thirty little calves all lying snugly in some small, sheltered dip of the ground, left there in charge of the sentinel cows by their mothers who had gone in to water. Now as soon as these calves saw me they would try to hide—do it very well too, under any little bush there might be handy, and lie close until I got off my horse and touched one; then he would jump up, and, no matter how young, make a staggering charge at my legs. He would give a peculiar cry at the same time, which would bring the guardian cows in at a full gallop and give me reason to mount at once. Cattle have extraordinary *homing* power; so have horses. Cattle recognise *individuals* in a very extraordinary way. I have had considerable experience in droving large herds, say 1000 or 1200 head, long journeys extending over many months. I have been struck with the fact that, a week after that herd has been travelling, every beast in it seems personally acquainted with every other: that is, if a strange cow or bullock were to join the herd, that cow or bullock would be immediately expelled. When a herd is travelling thus, each beast in a very few days takes up his position in the mob, and may always be found in the van, the rear, or on the right or left wing, the strongest cattle leading. That cattle and horses can *smell* water is a delusion. Cattle and horses always have their particular friends; at night when cattle are *camping* on a journey, there is always much bellowing and fuss until certain *côteries* of friends get together and lie down comfortably. A beast blind of, say the *left* eye, always travels at the outside of the *right* wing of the drove. A beast that has been scratched sufficiently to draw blood will be hunted and pursued by all the rest. Cattle have a habit of appointing certain *camps* or *rendezvous*, where, on any alarm, they congregate. Half-wild cattle are sometimes very difficult to drive off these camps. Wild cattle are singularly clever in concealing themselves, as are all wild beasts, and will hide in half a dozen little bushes no one would suppose would hold a calf.

Wild "dingo" puppies, taken away from their mothers, are easily reared, but never lose their inborn savagery: they are not to be trusted near poultry, sheep, or cats. The chief difference between them and their civilised brethren is, if, say a collie pup misbehaves himself and is kicked, he yelps, sticks his tail between his legs, and runs away; whereas his wild brother, with his tail erect as that of a Dandie Dimont terrier, snaps viciously at the foot which kicks him. I have owned a pure-bred dingo ("warrigal" we call them) which ran with our kangaroo dogs, and the dog would worry one of his own kind with as savage a zest as would any of the great powerful hounds with which he had associated himself. As to feigning death, I think the Australian "dingo," or "warrigal," a good case in point. We once ran a wild dog with three powerful kangaroo dogs, noted for their killing powers; they caught him, worried him, and he lay for dead; at any rate the hounds thought he was done for; they lay down quite contentedly to regain their wind. We cut off the warrigal's brush, and he gave no sign. Just as I was getting on to my horse, I saw the supposed corpse open one eye. Of course we put the thing beyond a doubt. A kangaroo dog has been known to run down a dingo bitch at heat, line her, and then kill her. The worst and most dangerous wild dogs in the Australian pastoral districts are *half-bred ones*. Kangaroo dogs should be, I think, about three-quarters greyhound—the rest either mastiff or bull-dog; such a dog should be able to catch and kill almost anything.

A doe kangaroo, when hunted and hard pressed, will throw the young one out of the pouch into any handy clump of scrub or tussock of grass. The "Joey" accepts the situation, and makes himself as small as ever he can; in fact, in looking for him, all you ever can see are his bright eyes. Young kangaroos seem to possess exactly the same instinct as the calves of wild or semi-wild cattle, that of concealing themselves. Young kangaroos soon adapt themselves to circumstances, and make themselves comfortable at the bottom of the pocket of a jacket.

I remember that once upon a time, about 1856, we caught a brood of wild ducklings, which we took home and put under a hen. These ducklings, not one of them fledged, walked a mile and a half along a very dusty road to the place whence we had taken them, and rejoined, as I hope, their progenitors. Our black boys tracked them.

Diffusion of Scientific Memoirs

IN some of the numbers of NATURE which have recently reached me I find that Prof. Tait has broached a subject of extreme importance to societies interested in the distribution of their publications, and in the receipt of publications from kindred bodies. Although I am connected with a small society which has hitherto only enjoyed an ephemeral existence, I trust that a brief account of my experience in the distribution of publications may have a little value. Early in 1883 more than 250 copies of our publications were distributed amongst the leading societies and libraries throughout the world. On my exchange list I now have 271 names; I also send to about 100 members of the Society. A few copies of our publications have been sent to persons who have made seismology a specialty. With each volume there was inclosed a printed form to be filled up and returned, both as a receipt and as a statement of other volumes which might be required. The institutions to which volumes were sent were as far as possible selected as having an interest in scientific investigations. Especial care was taken to forward volumes to institutions established in earthquake-shaken or volcanic regions, as for instance to many parts of South America, New Zealand, Central Siberia, Iceland, &c.

In many cases our receipts were returned. In others societies returned their own special forms. A few societies sent us their publications in return. One society very kindly made a collection of earthquake literature for us. Several others made special applications for particular volumes to complete the series of our publications. *In about half the cases, however, I find that no notice whatever was taken of our gift.*

For example—

For 34 volumes sent to institutions in	England	17	receipts were received.
,, 40 ,, ,, ,,	Germany	22	,, ,,
,, 49 ,, ,,	the United States	31	,, ,,
	&c., &c., &c.		

One result has been that our distribution list has been reduced. In one or two instances, where I know that earthquake literature cannot fail to be acceptable, the omission to send acknowledgment has been overlooked, and I continue to post our publications.

No doubt many societies publish lists of presents. These may be useful to the members of such societies; but they are valueless to donors who are not favoured with such publications.

The fact which is most to be regretted is that these omissions have resulted in many libraries not being *au courant* with the latest information.

Now supposing that the publications in question have any value whatever, it is natural to seek a cause for this state of affairs.

In many instances the omissions may be due to negligence, whilst in others they may be due to institutions having failed to establish a system for their correspondence.

From my own communications with various societies it is evident that many of them neither possess forms for routine correspondence nor have they the means for facilitating reference to ordinary or extraordinary correspondence. Sheets of plain notepaper, envelopes, pens, ink, and a few postcards constitute the business equipment.

Those societies which possess forms for the acknowledgement of presents, &c., often sacrifice fivepence for the postage of an elaborate document where a wrapper or a three-halfpenny postcard, although wanting in formality, would attain the same result. Although learned societies are not institutions where business is a specialty, many of them might possibly derive benefit by the adoption of more business-like methods. At present it would appear that there are many institutions which are as equally indifferent to the circulation of their own publications as they are to receive those of others.

JOHN MILNE,
Hon. Sec. of the Seismological Society of Japan

Tokio, June 7

Suicide of Snakes

THE letter of Edward F. Hardman in NATURE (vol. xxix. p. 452), with reference to the suicide of black snakes, recalls an incident which I once witnessed; I was quite small, but my memory of the strange occurrence is very clear and distinct. It was in the State of Illinois, when at that early day a short, thick variety of rattlesnake was very numerous, so much so that the State acquired an unenviable reputation in the older parts of the Union. Farmers in "breaking prairie," as the first ploughing of the prairie sod was called, would kill them by dozens in the course of a single summer. They were very venomous, but owing to their sluggish nature and their rattle, which was always sounded before an attack, but few persons were bitten by them. Moreover, there was little danger of death if proper remedies were applied at once.

I was one day following one of the large breaking ploughs common at that time. It was drawn by five or six yoke of oxen, and there were two men to manage the plough and the team. As we were going along, one of the men discovered a rattlesnake, as I remember about twelve or fourteen inches in length. They rarely exceeded eighteen or twenty inches, so that this one was probably about two-thirds grown. The man who first saw it was about to kill it, when the other proposed to see if it could be made to bite itself, which it was commonly reported the rattlesnake would do if angered and prevented from escaping. Accordingly they poked the snake over into the ploughed ground, and then began teasing it with their long whips. Escape was impossible, and the snake soon became frantic at its ineffectual attempts either to injure its assailants or to get away from them. At last it turned upon itself and struck its fangs into its own body, about the middle.

The poison seemed to take effect instantly. The fangs were not withdrawn at all, and if not perfectly dead within less than five minutes, it at least showed no signs of life. That it should die so quickly will not seem strange if it is borne in mind that the same bite would have killed a full-grown man in a few hours' time.

The men watched it long enough to be sure that it would not be likely to move away, and then went on with their work. I trudged around with them for an hour or more, and every time we came where the snake was I stopped and looked at it, but it never moved again. In this case I do not remember that the snake had been injured at all. I have often heard of rattlesnakes biting themselves under such circumstances, but this was the only case that ever came under my observation.

Ongole, India, June 17 W. R. MANLEY

Sky Glows

AS we appear to be having a return of the gorgeous sunset phenomena with which we were favoured towards the end of last year, a brief mention of two of the most brilliant displays that I have recently had the good fortune to observe may be of sufficient interest to place upon record in the pages of NATURE, The "after-glow," though very brilliant, has not of course attracted the attention it would have done, on account of the twilight; if it had not been for that circumstance, I think the recent displays of the phenomena would have been quite as gorgeous as those of last year. It would certainly have been so in the case of the "after-glow" on June 22; the "glow" on that evening at nine o'clock reached an altitude of 45°, and extended from the north to the west-north-west point of the horizon. For an altitude of about 20° the glow was of a beautiful crimson tint; above that altitude it was of a pale pink fading away gradually towards the edge to a pale orange. On that evening the reddish glow was not confined to that part of the horizon where the sun had gone down, but extended over the entire sky from the west to the east, the whole celestial vault, which was quite free from clouds, appearing to be slightly tinged with red. So conspicuous was this redness of the sky that a lady friend remarked—before her attention had been called to it —"How red all the sky is."

On the 7th inst. the "glow" equalled in brilliancy, though not in extent, the display above described. At 8h. 45m. p.m. it reached an altitude of about 30°, and extended from the west-south-west to the north-north-west point of the horizon. The redness of the whole sky, which was so noticeable on the former occasion, was wanting on this. On both occasions the "glow" was not visible for more than an hour and a quarter after sunset.

Dalston, E., July 12 B. J. HOPKINS

MANY of your correspondents have referred to the "remarkable appearances of the sky" at sunrise and sunset last year, but I have not observed any reference to the following:—On the morning of November 30 I was on my way from Basle to Calais by the St. Gothard mail train, and observed the whole eastern sky become lit up as though there were a splendid sunrise; the larger print of a newspaper was easily readable at the carriage window. On referring to our watches we found it was 5 o'clock by Basle time. During the next half-hour every trace of the phenomenon gradually vanished. FRANK PETRIE

July 11

Fireball

READING W. G. Smith's remarks on lightning in last week's NATURE (p. 241), recalls to my mind a ball I saw during a storm in the autumn of 1881. The storm had lasted some time, and I sat reading a little back from an open window but facing it. Suddenly it became so dark that I could no longer see. I dropped my book and looked out. A ball of fire was passing through the window into the room. It moved very slowly onwards and downwards towards me, and became almost stationary over my book. At first I thought it rested upon it, but I soon saw it was moving slowly across. Having passed over the book, it turned in the direction of my hand, paused just beneath it, and then sank towards the carpet. At this instant a peal of thunder crashed over the house—it was the very loudest I have ever heard. ANNIE E. COCKING

The Elms, Bedford Park, Chiswick, W., July 14

Butterflies as Botanists

THERE can be no doubt, as pointed out by Fritz Müller in your last issue (p. 240), that the habits of insects often indicate affinities in plants. There is doubtless a strong affinity between the Solanaceæ and Scrophularineæ; the small oval pollen is almost identical in both. The habits of fungus parasites sometimes disclose similar relationships, often more real than is at first apparent; we have an example of this in the fungus of the potato disease, *Peronospora infestans*. This parasite is almost peculiar to the Solanaceæ, being especially destructive to Solanum, Lycopersicum, and Petunia, but at times it invades the Scrophularineæ and grows on Anthocersis and Schizanthus. It is not common to find one parasitic fungus attacking the members of two natural orders of plants, but other examples could be given.

W. G. S.

A Cannibal Snake

ABOUT eighteen months ago, just previous to my leaving India, at Devalah in the Wynaad, the horsekeepers chased and killed a large cobra, 5 feet 4 inches; previous to death it was thrown down in front of the door of our house, when, after a good deal of twisting and wavy contortion of the body, it disgorged a small rock snake over 4 feet in length. I had heard of the same thing before in India, so that I do not think cannibalism in snakes is so uncommon as Mr. Evans thinks.

JOHN FOTHERINGHAM

96, Netherwood Road, West Kensington Park, W., July 12

FOURTH NOTE ON THE ELECTRICAL RESISTANCE OF THE HUMAN BODY

IN my communication to NATURE (vol. xxix. p. 528) I described the use of alternating currents and the telephone for the above purpose, and promised to endeavour to obtain at least an approximate measurement of the E.M.F. developed in the secondary coil of an induction apparatus. This promise I now propose to fulfil. But before proceeding to the special subject of the present note, I should wish to draw attention to a paper which appeared on the 15th of the same month in the *Asclepiad*, by that able experimentalist Dr. B. W. Richardson. He therein describes not only experiments made with the large induction coil of the Polytechnic, but also others made as early as 1868 in conjunction with the late Mr. Becker, the object of which was to obtain a measure of the resistance of animal structures.

"The results," says Dr. Richardson, "were not fully satisfactory. They were variable even when the conditions under which the experiments were made were entirely the same. This variability we found to be due to decomposition of the animal substance, a decomposition which, however feeble the battery, was sufficient to destroy the precision we desired to obtain." Putting the more recently coined word "polarisation" for decomposition, this expresses exactly the difficulty described by me in my first note. "It was, however, possible," says the doctor, "to make out that blood conducted better than any other structure of the body, and better than water." I can now fully corroborate this excellent observation, and perhaps extend its application.

Physiological and even pathological fluids, such as the serum of dropsy, conduct far better than muscle, bone, and nerve. One instance out of many may serve. In the very first case recorded in my communication to NATURE (vol. xxviii. p. 151) the lowest resistance obtained from foot to foot was 2300 ohms. The patient was then very emaciated, but quite free from dropsy. Towards the end of the case, which after death proved to be one of ulcerative endocarditis, as I had considered it to be during life, slight but distinct dropsical effusion in the lower extremities set in; the resistance sank at once to 700 ohms, and I had to discontinue my observations from the evident change of electrical conditions. I have since verified the same fact many times, and on it I partly found the belief, already several times stated, that "the human body, in spite of its large amount of liquid constituents, follows a similar thermal law of resistance to that influencing solid conductors, though in a very much higher ratio" (NATURE, vol. xxviii. p. 152).

Dr. Richardson does not seem to have attempted to determine the resistance of the living body, which Du Moncel, in 1877, did, and with fairly accurate, if unpleasant, results (NATURE, vol. xxix. p. 528). On the discovery, however, in 1879, of Prof. Hughes's electric balance, he resumed his observations, this time with an alternating induction current, though he does not himself notice the important change. His results are unfortunately taken in arbitrary units on the graduated scale of 200 parts originally applied to Prof. Hughes's instrument. If there is any way of reducing these fictitious to absolute values, my work will be both lightened and assisted by a proved observer. Blood-clot and serum, white and gray nervous substance, muscle, bone, coagulated albumen, gelatine, and pus were all tested. Some of the results were excellent. For instance, fat, which by one experimenter has been stated to increase the conductivity of the body, is found by Dr. Richardson, as I also have found it, to be an absolute non-conductor. It is almost unnecessary to say that, with so skilled a chemist and physiologist, all proper temperature corrections and other similar precautions were most strictly observed.

I can now proceed to the main topic of my present note. On receipt of the Wurzburg dynamometer it was put in adjustment, and a strenuous effort made to compare the indications given with a constant and an alternating current, to both of which it is sensitive. But the movable suspended coil made of an ivory core, with a double weight of silk-covered copper wire, hung by a platinum hook, and dipping by its other termination into a vessel of strong sulphuric acid by means of a platinised platinum plate, is very heavy; takes a long time to get to its full deflection, thus allowing the battery to run down sensibly, and, what is worst of all, has a tendency to "integrate." By this I mean to sum up, by its mechanical inertia, a large number of small, intermittent pulls as given by the reversed current, into an almost identical deflection (less, of course, losses) with that given by the one steady pull of a continuous current. In spite of its beautiful workmanship, it had to be discarded for the present research. Somewhat in despair, I fell back on a similar instrument, shown by me at the Oxford meeting of the Physical Society in June 1882, and there heavily abused. The moving coil in this is made of silk-covered aluminium wire to insure lightness, and the bifilar suspension is made of the silver-gilt wire used for military epaulettes and facings. It is the work of my own poor hands.

Herr Obach then stated, and the statement was repeated in your columns, that this material had already been used by Messrs. Siemens for their "dust-recorders," but had failed by difficulty of making contact. On testing my little toy, I found its resistance had not altered in twenty-five months one fraction of an ohm, and that it moved briskly up to its maximum, standing there quite long enough for a good observation. Indeed, in spite of its condemnation by a jury of experts over two years ago, it was still so lively that I thought it better to check extra swing by a small platinum paddle 1 cm. square moving in sulphuric acid.

On a metre scale, at one metre distance, the reflected image in a telescope gave ·365 mm. deflection[1] with the whole induction current from Prof. Kohlrausch's metre-bridge, as described in my last note.

The object now was obviously to obtain an independent measure of the actual E.M.F. to which this deflection was due. The quadrant electrometer, or some other delicate potential measurer, of course suggested itself. A trapdoor portable, kindly lent me by Prof. McLeod, refused to take notice of my wretched little currents, limited as they are by human susceptibility. I do not possess a quadrant, nor will the Royal Society, though twice asked, lend me one. Here again my friends at Cooper's Hill came to my rescue, and I have to express my thanks, not only to Prof. McLeod, but also to Prof. Stocker and his excellent demonstrator Mr. Gregory, for their assistance. With my Kohlrausch induction bridge in a big bag I journeyed to Egham, and thence on foot to Cooper's Hill.

The formula to be made use of was obvious. It is given in Prof. Adams's Cantor lectures, and has been kindly verified for me by Prof. Hopkinson. In it the needle is connected with one pair of quadrants, so that $V_3 = V_1$. In this case—

$$\text{Deflexion} = \frac{k}{2}(V_1 - V_2)^2.$$

Prof. Adams has since shown me a different, and perhaps better, way of working, which I intend to make use of in the future. It was found that the two fine quadrant electrometers at Cooper's Hill College were unavailable; the one given by Lord Salisbury not admitting of the needle being placed in connection with either pair of quadrants, the other being disabled by some casual contact. We therefore with heavy hearts made a last struggle with the old Elliott pattern and single quadrants. This succeeded admirably, and on a mean of the four best out of six observations, we obtained a deflection of 107 with the intermittent current. "In order to be quite sure," Mr. Gregory wrote to me next day, "of the true value of the mean deflection we obtained, I have executed measurements with different numbers of cells. In these, the negative pole was to earth, the positive being connected at will to either pair of quadrants, and the needle also at will to either pair, giving four readings for each observation. I give only means, which agree well.

E.M.F.	Defl.	k
21 volts ...	32·75 ...	·149
29 " ...	63·75 ...	·151
47 " ...	161·75 ...	·146
	Mean	·1486

k was calculated from the formula

$$\delta = \frac{k}{2}(V_1 - V_2)^2.$$

By calculation, using the mean value of k, the E.M.F. to give a deflection of 107 came out 38. By observation, using an E.M.F. of 38 volts, the deflection was 107·25. This agrees so well with the calculated value that it will be easy to evaluate the E.M.F. corresponding to any deflection by the above formula."

The effect of rapid alternations seems to be to lessen the deflection, though Mr. Glazebrook stated, in a paper read before the Physical Society, that with between 10 and 120 contacts per second the result, in charging a condenser, was not perceptible.

[1] The bridge arrangement being entirely disconnected.

On the whole therefore, though I agree with Mr. Gregory that we have not obtained a measure of the maximum E.M.F., but only an integration, disregarding sign, the approximation is, I hope, superior to any made before, and affords a good general basis for farther work.

W. H. STONE

GAS-BURNERS[1]

THE economist who wished to point the moral of a healthy competition in industrial commerce could scarcely find a better instance to his hand than the progress made by gas illumination under the impetus given in the last few years by the rise of electric lighting. It is not overstating the case to say that greater improvement in the use of gas has been made since Jablochkoff introduced his electric candle than in the previous sixty years' history of gas lighting. Compared with the recent development of invention, the long period of non-competition appears almost stagnant. With the introduction of electricity arose a popular demand for "more light." With a new illuminant competing for favour, consumers growled more openly at "bad gas" and high gas bills. Each advance of the electric light was greeted with acclamations by the popular voice, shareholders began to tremble, and gas shares came down with a rush. It was time for gas managers and manufacturers to bestir themselves. The happy days of a monopoly in light seemed over. The consumers have reaped the benefit. Under the stimulus of competition the price of gas has been lowered, impurities have been cut down. Some half a dozen years ago the great London Companies were endeavouring to prove before a Parliamentary Committee that coal-gas could not be purified from bisulphide of carbon without creating such a nuisance as to be intolerable. Their object was to do away with the lime purifiers, made necessary by the regulations of the Gas Referees, and to use only oxide of iron. Since the advent of the electric light not a word has been heard about the impossibility of purifying coal-gas by lime. On the contrary, every effort is now made to supply gas as free from sulphur as possible. But while the gas has thus been improved in quality and lowered in price, a still greater improvement has been effected in the methods of burning it. By the application of the regenerative principle to gas-burners, the illuminative value of coal-gas has been doubled.

But in spite of the great advances made in gas-burners, the public have by no means yet reaped the full benefit. Owing to the carelessness of gas-fitters and the ignorance of consumers, the great majority of those who light their houses by gas waste at least 20 per cent. of their gas as an illuminating agent. If the flame smokes, or flickers, or gives a poor light, most people put it down to "bad gas," when in reality the burner is unsuitable, or worn out, or the supply pipes (nearly always too small) are choked. To all who burn coal-gas in their houses, and are troubled with "bad gas," we can heartily recommend "Gas-Burners, Old and New," by Owen Merriman.

This little book, published at a price which places it within the reach of a large public, describes very plainly in popular language the evolution of the best modern burners of Sugg, of Bray, and of Siemens, from the original "cock-spur burner" of Murdock, and Accum's "tube with a simple orifice, at which the gas issues in a stream, and if once lighted will continue to burn with the most steady and regular light imaginable, as long as the gas is supplied." The illustrations are all that can be desired.

Owen Merriman has taken pains to insist on the two great *desiderata* of gas-burners—high temperature and low temperature, but we think he has gone too far in attempting to give a popular "theory of luminous combus-

[1] "Gas-Burners, Old and New." By Owen Merriman. (London: Walter King, 1884.)

tion." We are told that "the various gases which constitute ordinary coal-gas do not all burn together in the flame; . . . thus hydrogen is the first to burn, taking fire readily as soon as it issues from the burner, while the combustion of heavy hydrocarbons does not commence until they enter the hotter portions of the flame." Again Owen Merriman says: "the amount of light developed by any coal-gas flame is *directly proportional* to the degree of intensity to which the temperature of the carbon particles is raised." The italics are ours. In a note on page 23 there is some confusion as to the effect of the admission of air into a Bunsen burner. "A continuous wind blowing upon the flame destroys its luminosity altogether, because the heat intensity of the flame is *lowered below the temperature necessary to decompose the hydrocarbons;* consequently these latter burn without the preliminary separation of carbon, and a non-luminous flame is produced—exactly as in the Bunsen burner." The reader would gather from this that the flame of a Bunsen was colder than an ordinary flame, and by the same argument the blast of a blow-pipe would render a gas-flame colder instead of hotter. And again on page 43 a similar mistake is made when we are told that a too long flame is bad because the gas is "brought too early into intimate contact with air, and so oxidised, or fully consumed, *before its carbon has been raised to the temperature necessary to enable it to give out light.*" We point out these few blemishes in the hope that the author may correct them in a future edition of the work, to which we wish a hearty success.

BIRDS'-NEST SOUP

IT is scarcely probable that the famous birds'-nest soup which Chinese cooks at the Health Exhibition offer to favoured visitors will ever become a popular dish in England. The tasteless, gelatinous compound is not suited to our palates. Perhaps this is not to be regretted, as the supply of material for this mysterious compound is far from being inexhaustible. There appears to be only one place in the world where it can be obtained in any quantity, and this has recently been visited by Mr. Pryer, a naturalist of Yokohama, who communicates his observations to the *Japan Gazette*, an English journal published in that settlement. Leaving Elopura, the infant capital of the infant colony of British North Borneo, in March last, Mr. Pryer ascended for some thirty miles the Sapugaya River, which flows into Sandakan Bay, on which the town is built. Passing through the mangrove and nipa swamps which line the banks, he arrived at noon on the second day at his destination—the celebrated birds'-nest caves of Gomanton. These caves, which are two in number, called by the natives the Black and the White Caves, are situated in a limestone cliff 900 feet in height, which the traveller came on quite suddenly in the centre of the forest. The porch, Mr. Pryer writes, is rather over 100 feet wide by 250 high, and the roof slopes up for 110 feet more, so that the height of this magnificent natural cathedral is 360 feet. The interior of the Black Cave is well lighted, as there is a large circular hole in the roof on the right, and a smaller one on the left, forming two aisles. The walls and roof are rugged, and beautifully coloured, shading from black to brown, gray, dark yellow, red, and green. The nests of the bats and swifts were seen hanging in clusters from the sides and roof, and here and there in seemingly the most inaccessible places were the rattan stages, ladders, and ropes of the nest-gatherers. These latter reached their perilous heights by means of many smaller caves in the cliff above. The White Cave is 400 feet higher up than the Black Cave, and at the entrance to this the nest-gatherers live under a guard of the North Borneo Company's soldiers. After some examination Mr. Pryer was able to discover the material which forms these mysterious nests, and from which they derive the qualities which render them so highly prized in China. They are made from a soft fungoid growth that incrusts the limestone in all damp situations; it grows about an inch thick, outside dark brown, but inside white. The birds make the black nests from the outside layer, and the best quality of white nests are, of course, from the inside. It is taken by the bird in its mouth, and drawn out in a filament backwards and forwards like a caterpillar weaving its cocoon. At nightfall takes place what the natives style with much justice the most wonderful sight in all Borneo, and it might be added, one of the most wonderful sights in the world—viz. the return of the swifts to their nests, and the departure of the bats for the night. About that time a rushing sound was heard, and peering over the abyss into the Black Cave Mr. Pryer saw columns of bats wheeling round and round the sides in regular order; soon they began to circle up, rising into the air in a corkscrew flight. Having reached a certain height, a detachment would break off and fly away rapidly. He counted nineteen flocks go off like this, each flock consisting of many thousands, and then they commenced to pour away in a continuous stream until it was too dark to see them any longer. Soon after the bats emerged from their cave, the swifts began to return to theirs, first in tens, then in hundreds, and at last they too streamed in continuously, and when the traveller went to sleep at midnight they were still flying in in undiminished numbers. Rising before daylight the following morning, Mr. Pryer witnessed a reversal of the proceedings of the previous night, the swifts going out and the bats coming home. The latter, he says, literally rained into their chasm for two hours after sunrise; looking up to the bright sky, numbers of small specks appear, flash down perpendicularly with great rapidity, and disappear into the darkness. From specimens of the bat which were secured, they were found to be all of one species, the caudal membrane extending only half down the tail, which is free for an inch and a half, giving the animal, when the wings are folded up, very much the appearance of a mouse. The wings are very long and narrow, and it flies with great speed. Two species of birds of prey—one a kite, the other a hawk—the *Haliaster indus* and the *Machœramphus alcinus*, prey on the bats and swifts when swarming into and out of the caves. A detailed examination of the latter was rendered disagreeable by enormous quantities of guano, the deposit of centuries. Its depth is not known, but a long spear does not touch the bottom when thrust in to the hilt. All the roof of the dark parts of the cave was occupied by birds who keep up an intermittent twittering, sounding, from the immense number of them, like the surf beating on a rocky shore. Near the centre of the largest cave the explorer was shown a small beam of light from a funnel at the top of the rock, exactly 696 feet above his head. The nests are gathered from these enormous elevations by means of flexible rattan ladders and stages. On these two men take their station; one carries a light four-pronged spear about 15 feet long, and just below the prongs a lighted candle is fixed. Holding on to the ladder with one hand, the spear is managed with the other, and the nest transfixed, a slight push detaching it from the rock. The spear is then withdrawn until the head is within reach of the second man, who takes the nest off the prongs and puts it in a pouch carried at the waist. According to statements made by the headman of the place, the annual value of the nests taken varies from five to six thousand pounds sterling. This, it is to be presumed, means the value on the spot; their value on reaching China must be far higher. The caves have been worked for seven generations without any apparent diminution, although three crops are gathered in the year. Notwithstanding the dangerous nature of their occupation—for even samphire-gatherers work in the open—accidents are very rare amongst the natives employed in collecting the nests. There is an almost inexhaustible supply of guano in the caves, and the number of bats and swifts in them is so enormous that if they are undisturbed a regular quantity may be taken out yearly. Should the visitor to the Health Exhibition who obtains some of this far-famed and mysterious soup have little relish for it, as is not unlikely, he will at any rate have the satisfaction of knowing that he has before him a dish the principal ingredient of which was formed by the little swifts and bats which inhabit the Gomanton Caves in the centre of the magnificent tropical forests of North Borneo. There is probably no other article of food in the Health Exhibition, or in all Europe, more extraordinary in the mode of production, or in the method and circumstances under which it is obtained.

ON THE EVOLUTION OF FORMS OF ORNAMENT[1]

II.

THE leaf in Dracunculus has a very peculiar shape: it consists of a number of lobes which are disposed upon a stalk which is more or less forked (tends more or less to dichotomise). If you call to your minds some of the Pompeian wall decorations, you will perceive that similar forms occur there in all possible variations. Stems

FIG. 12.

are regularly seen in decorations that run perpendicularly, surrounded by leaves of this description. Before this, these suggested the idea of a misunderstood (or very conventional) perspective representation of a circular flower. Now the form also occurs in this fashion, and thus negatives the idea of a perspective representation of a closed flower. It is out of this form in combination with the flower-form that the series of patterns was developed which we have become acquainted with in Roman art, especially in the ornament of Titus's Thermæ and in the Renaissance period in Raphael's work. [The lecturer here explained a series of illustrations of the ornaments referred to (Figs. 12, 13, 14).]

[1] From a paper by Prof. Jacobsthal in the *Transactions* of the Archæological Society of Berlin. Continued from p. 251.

The attempt to determine the course of the first group of forms has been to a certain extent successful, but we meet greater difficulties in the study of the second.

It is difficult to obtain a firm basis on which to conduct our investigations from the historical or geographical point of view into this form of art, which was introduced into the West by Arabico-Moorish culture, and which has since been further developed here. There is only one method open to us in the determination of the form, which is to pass gradually from the richly developed and strongly differentiated forms to the smaller and simpler

FIG. 13.

ones, even if these latter should have appeared contemporaneously or even later than the former. Here we have again to refer to the fact that has already been mentioned, to wit, that Oriental art remained stationary throughout long periods of time. In point of fact, the simpler forms are invariably characterised by a nearer and nearer approach to the more ancient patterns and also to the natural flower-forms of the Araceæ. We find the spathe, again, sometimes drawn like an Acanthus leaf, more often, however, bulged out, coming to be more and more of a mere outline figure, and becoming converted into a sort of background; then the spadix, generally conical in

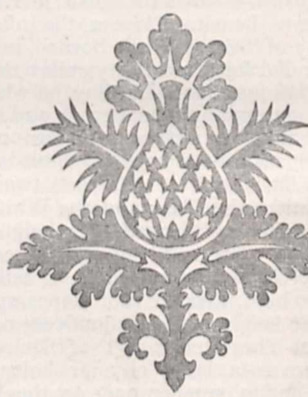

FIG. 14.

shape, sometimes, however, altogether replaced by a perfect thistle, at other times again by a pomegranate. Anberville in his magnificent work "L'Ornement des Tissus," is astonished to find the term pomegranate-pattern almost confined to these forms, since their central part is generally formed of a thistle-form. As far as I can discover in the literature that is at my disposal, this question has not had any particular attention devoted to it except in the large work upon Ottoman architecture, published in Constantinople under the patronage of Edhem Pasha. The pomegranate that has served as the original of the pattern in question is in this work surrounded with leaves till it gives some sort of an approach to the pattern. (There are important suggestions in the book as to the employment of melon-forms.) Whoever has picked the fruit from the tender twigs of the pomegranate-tree, which are close set with small altered leaves, will never dream of attributing the derivation of the thorny leaves that

Fig. 15.

appear in the pattern to pomegranate-leaves at any stage of their development.

It does not require much penetration to see that the outline of the whole form corresponds to the spathe of the Araceæ, even although in later times the jagged contour is all that has remained of it, and it appears to have been provided with ornamental forms quite independently of

Fig. 16.

the rest of the pattern. The inner thistle-form cannot be derived from the common thistle, because the surrounding leaves negative any such idea. The artichoke theory also has not enough in its favour, although the artichoke, as well as the thistle, was probably at a later time directly pressed into service. Prof. Ascherson first called my attention to the extremely anciently cultivated plant, the

Fig. 17.

Fig. 18.

Fig. 19.

Safflor (*Carthamus tinctorius*, Fig. 15), a thistle plant whose flowers were employed by the ancients as a dye. Some drawings and dried specimens, as well as the literature of the subject, first gave me a hope to find that this plant was the archetype of this ornament, a hope that was borne out by the study of the actual plant, although I was unable to grow it to any great perfection.

In the days of the Egyptian King Sargo (according to Ascherson and Schweinfurth) this plant was already well known as a plant of cultivation; in a wild state it is not known (De Candolle, "Originel des Plantes cultivées"). In Asia its cultivation stretches to Japan. Semper cites a passage from an Indian drama to the effect that over the doorway there was stretched an arch of ivory, and about it were bannerets on which wild safran (*Saflor*) was painted.

The importance of the plant as a dye began steadily to decrease, and it has now ceased to have any value as such in the face of the introduction of newer colouring matters (a question that was treated of in a paper read a short time ago by Dr. Reimann before this Society). Perhaps its only use nowadays is in the preparation of rouge (*rouge végétale*).

But at a time when dyeing, spinning, and weaving

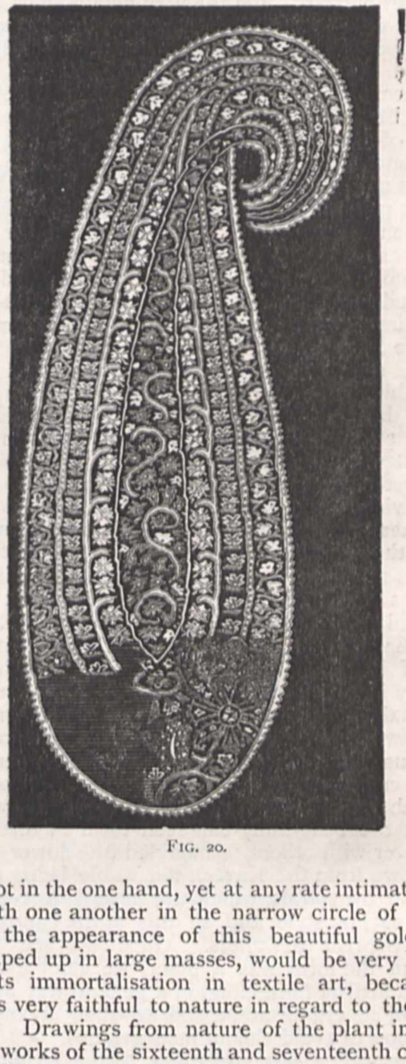

Fig. 20.

were, if not in the one hand, yet at any rate intimately connected with one another in the narrow circle of a home industry, the appearance of this beautiful gold-yellow plant, heaped up in large masses, would be very likely to suggest its immortalisation in textile art, because the drawing is very faithful to nature in regard to the thorny involucre. Drawings from nature of the plant in the old botanical works of the sixteenth and seventeenth centuries look very like ornamental patterns. Now after the general form had been introduced, pomegranates or other fruits—for instance, pine-apples—were introduced within the nest of leaves.

Into the detailed study of the intricacies of this subject I cannot here enter; the East-Asian influences are not to be neglected, which had probably even in early times an effect upon the form that was assumed, and have fused the correct style of compound flowers for flat ornament with the above-mentioned forms, so as to produce peculiar patterns; we meet them often in the so-called Persian textures and flat ornaments (Fig. 16).

We now come to the third group of forms—the so-called Cashmere pattern, or Indian palmetta. The developed forms which, when they h ive attained their highest development, often show us outlines that are merely fanciful, and represent quite a bouquet of flowers leaning over to one side, and springing from a vessel (the whole corresponding to the Roman form with the vessel), must be thrown to one side, while we follow up the simpler forms, because in this case also we have no information as to either the where or the when the forms originated. (Figs. 17, 18, 19.)

Here again we are struck by resemblances to the forms that were the subjects of our previous study, we even come across direct transitional forms, which differ from the others only by the lateral curve of the apex of the leaf; sometimes it is the central part, the spadix, that is bent outwards, and the very details show a striking agreement with the structure of the Aroid inflorescence, so much so that one might regard them as actually copied from them.

This form of ornament has been introduced into Europe since the French expedition to Egypt, owing to the importation of genuine Cashmere shawls. (When it cropped up in isolated forms, as in Venice in the fifteenth century, it appears not to have exerted any influence; its introduction is perhaps rather to be attributed to calico-printing.) Soon afterwards the European shawl-manufacture, which is still in a flourishing state, was introduced. Falcot informs us that designs of a celebrated French artist, Couder, for shawl-patterns, a subject that he studied in India itself, were exported back to that country and used there (Fig. 20).

In these shawl-patterns the original simple form meets us in a highly developed, magnificent, and splendidly coloured differentiation and elaboration. This we can have no scruples in ranking along with the mediæval plane-patterns, which we have referred to above, among the highest achievements of decorative art.

It is evident that it, at any rate in this high stage of development, resisted fusion with Western forms of art. It is all the more incumbent upon us to investigate the laws of its existence, in order to make it less alien to us, or perhaps to assimilate it to ourselves by attaining to an understanding of those laws. A great step has been made when criticism has, by a more painstaking study, put itself into a position to characterise as worthless, ignorantly imitated, or even original, miscreations such as are eternally cropping up. If we look at our modern manufactures immediately after studying patterns which enchant us with their classical repose, or after it such others as captivate the eye by their beautiful colouring, or the elaborative working out of their details, we recognise that the beautifully-balanced form is often cut up, choked over with others, or mangled (the flower springing upside down from the leaves), the whole being traversed at random by spirals, which are utterly foreign to the spirit of such a style, and all this at the caprice of uncultured boorish designers. Once we see that the original of the form was a plant, we shall ever in the developed artistic form cling, in a general way at least, to the laws of its organisation, and we shall at any rate be in a position to avoid violent incongruities.

I had resort, a few years ago, to the young botanist Ruhmer, assistant at the Botanical Museum at Schöneberg, who has unfortunately since died of some chest-disease, in order to get some sort of a groundwork for direct investigations. I asked him to look up the literature of the subject, with respect to the employment of the Indian Araceæ for domestic uses or in medicine. A detailed work on the subject was produced, and establishes that, quite irrespective of species of Alocasia and Colocasia that have been referred to, a large number of Araceæ were employed for all sorts of domestic purposes. Scindapsus, which was used as a medicine, has actually retained a Sanscrit name, "vustiva." I cannot here go further into the details of this investigation, but must remark that even the incomplete and imperfect drawings of these plants, which, owing to the difficulty of preserving them, are so difficult to collect through travellers, exhibit such a wealth of shape, that it is quite natural that Indian and Persian flower-loving artists should be quite taken with them and employ them enthusiastically in decorative art. Let me also mention that Haeckel, in his "Letters of an Indian Traveller," very often bears witness to the effect of the Araceæ upon the general appearance of the vegetation, both in the full and enormous development of species of Caladia and in the species of Pothos which form such impenetrable mazes of interlooping stems.

In conclusion, allow me to remark that the results of my investigation, of which but a succinct account has been given here, negative certain derivations, which have been believed in, though they have never been proved; such as that of the form I have last discussed from the Assyrian palmetta, or from a cypress bent down by the wind. To say the least the laws of formation here laid down have a more intimate connection with the forms, as they have come down to us, and give us a better handle for future use and development. The object of the investigation was, in general words, to prepare for an explanation of the questions raised, and even if the results had turned out other than they have, it would have sufficed me to have given an impulse to labours which will testify to the truth of the dead master's words:—

"Was Du ererbt von deinen Vätern hast,
Erwirb es, um es zu besitzen."

NOTES

THE death is announced, at the age of seventy-four years, of Prof. Lepsius, the celebrated Egyptologist.

THE conference and jury work at the Health Exhibition is now in full swing, and we are glad to note that, with regard to the Conference, all the societies and organisations that have to deal with subjects akin to health or education are taking up the matter very warmly, so that the executive of the Exhibition has the advice and opinion of many experts. The recent opening of the Educational Section by the Prince of Wales, to which we have already referred, has recently drawn more attention to the *mens sana*, the *corpus sanum* having up to the present moment been alone regarded. From the first we consider that the matter of education has been placed altogether in far too secondary a position, and if a little more trouble had been taken by those who are responsible for the Exhibition, the educational exhibits might have been as extensive and as important as those regarding health. That is the more to be regretted because so much is being said about education nowadays, especially technical education, by those who know very often very little of what is really wanted, and of what true technical education really means. The members of the various juries are working with a will, and from what we learn we do not think it probable that the objections made to some of the awards last year will be renewed this. The opportunity which has been afforded to the exhibitors of practically nominating a considerable number of jurymen is a measure well adapted to allow the thing to work smoothly. One of the great difficulties encountered by the jurymen has been the hurried way in which the Exhibition itself has been put together and catalogued. We have for too great an extent a succession of shops containing various articles, instead of a complete separation of the various articles among their several classes. This of course gives great trouble to all concerned, and is an administrative blunder which should not be allowed to be repeated.

We are glad to be able to announce that H.R.H. the Prince of Wales has become Patron of the Marine Biological Association, and has contributed a handsome donation to its funds. The following is now the full list of Officers and Council as definitely elected by the Association, at its meeting on June 17:—Patron, H.R.H. the Prince of Wales, K.G. President, Prof. Huxley (President of the Royal Society). Vice-Presidents: The Duke of Argyll, K.G., the Duke of Sutherland, K.G., the Marquis of Hamilton, the Earl of Dalhousie, K.T., Lord Walsingham (Trustee of the British Museum of Natural History), Edward Birkbeck, M.P. (Chairman of the Executive Committee of the International Fisheries Exhibition), George Busk, F.R.S., W. B. Carpenter, C.B., M.D., F.R.S., W. H. Flower (Director of the British Museum of Natural History), J. Gwyn Jeffreys, F.R.S., Sir John Lubbock, Bart., M.P. (President of the Linnean Society). Council: Prof. Moseley, F.R.S. (Oxford), Chairman, Prof. Jeffrey Bell, F.Z.S. (British Museum), W. S. Caine, M.P., W. T. Thiselton Dyer, C.M.G. F.R.S. (Royal Gardens, Kew), John Evans, D.C.L. (Treasurer Royal Society), A. C. L. G. Günther, F.R.S. (British Museum), Prof. Herdman (Liverpool), E. W. H. Holdsworth, Prof. McIntosh (St. Andrew's), Prof. Milnes Marshall (Manchester), Sir Philip Cunliffe Owen, K.C.M.G., C.B., G. J. Romanes, F.R.S. (Sec. Linn. Soc.), P. L. Sclater, F.R.S. (Sec. Zool. Soc.), Adam Sedgwick (Cambridge). Hon. Treasurer, Frank Crisp, (V.P. and Treas. Linn. Soc.), 6, Old Jewry, E.C. Hon. Secretary, Prof. E. Ray Lankester, F.R.S., 11, Wellington Mansions, North Bank, N.W.

We regret to learn of the death of the venerable Abbé Moigno at the age of eighty-one years. The name of the Abbé has been long known in connection with French science, and more especially as the founder, and till quite recently the editor, of *Les Mondes*.

It is proposed to hold a special American Exhibition in London in May 1886, at which the products, manufactures, and varied phases of life in the United States will be represented.

By a decree dated Ems, July 4, the Emperor conveys his thanks to Dr. Auwers, the celebrated astronomer who so successfully superintended the German preparations for observing the transits of Venus in 1874 and 1882. The Emperor further expresses his thanks for the assistance so hospitably rendered to the German scientific expeditions, not only by Germans living abroad, but also by many persons belonging to other nationalities.

Biologists attending the Montreal meeting of the British Association will be pleased to hear that Prof. Asa Gray has promised to be present and to read a paper in Section D "On some characteristic features of the Botany of North America," with the special view of aiding botanists and members of the Section generally in their appreciation of the flora.

It is announced from Montreal that a large number of the members of the British Association visiting Canada next month have already been "placed" in private houses in the city. The matter continues to be very heartily taken up in the towns of the Dominion, and there is every probability of a warm welcome being extended to the members. Considerable amusement has been caused in Montreal by some of the letters received by the Montreal Committee of the British Association from those members on this side of the Atlantic desiring information regarding Canada. The climate of the country is evidently a subject upon which there is much misconception among members. The queries on this point are most exacting, while a quite unwarranted dread of mosquitoes is held by not a few members. It is satisfactory to learn that a circular is now in course of preparation that will answer most of the queries received by letter, and that on the completion of the labours of the Private Hospitality Committee a directory of the visitors will be published.

Prof. R. S. Ball has accepted an invitation from the Lowell Institute, Boston, United States, to give a course of six lectures on "Chapters in Modern Astronomy" next October.

The Society of Chemical Industry held their annual meeting in Newcastle last week. Dr. Perkin, F.R.S., was elected President for the next year.

Under the auspices of the Royal Geographical Society, Mr. E. C. Rye has done a most useful service to students by compiling a New Guinea Bibliography. It will be appended to Mr. C. R. Markham's paper on New Guinea, to be issued in No. 2, vol. i. of the Society's Supplementary Papers. Mr. Rye's Bibliography covers over fifty pages, and contains considerably over a thousand entries. It is evidently the result of immense labour and research, and is practically exhaustive. It includes not merely geography proper, but every aspect of the country; the references to natural history are specially copious, and include not only books but papers and notes in periodical publications of all kinds. The references are of the most precise character, and the whole is worthy of the editor of the *Zoological Record*.

It may interest many of our readers—especially those who would like to add to the pleasure of a tour by a little photography—to know that the London Stereoscopic Company give gratuitous private lessons to amateurs who purchase their apparatus from the Company. We have no doubt this will solve a prime difficulty in the case of many who are ambitious to be able to photograph on their own account, but who do not know how to take the first step.

Notwithstanding the troubles that have surrounded Madagascar for the last year or two, the scientific activity of the missionaries of the London Missionary Society has not abated, and the native printing press has not been idle. We have just received from Antananarivo two numbers of the *Antananarivo Annual and Madagascar Magazine*, edited by the Rev. R. Baron, containing numerous papers of varied scientific interest. The following are those of most importance:—The Sakalava (No. 2), by the Rev. A. Walin; Notes on four species of Lemur and on the Aye-Aye, by Mr. G. A. Shaw; Customs connected with death and burial in Malagasy, by the Rev. S. E. Jorgensen; Resemblance between Malagasy words and customs and those of Western Polynesia, by the Rev. R. S. Codrington. In No. 7 for 1883 we have—The race elements of the Malagasy, by the Rev. L. Dahle; the Sakalava (No. 3), by the Rev. A. Walin; Volcanoes in Eastern Imerina, by the Editor; Malagasy "Fady" (Tabu), by Mr. H. E. Standing; Genera of Malagasy plants, by the Editor; Relics of sign and gesture language among the Malagasy, by the Rev. J. Sibree; and various natural history and meteorological notes.

Dr. Regel, the Russian traveller, who recently arrived in Merv, intends proceeding along the northern mountain slopes of Afghanistan and the Amu Daria to Pamir. This journey will conclude the doctor's explorations in Central Asia.

The Milan Society for the Commercial Exploration of Africa has organised a circumnavigation of Africa, with a view of affording the pupils of the High School of Commerce, and others, an opportunity of becoming acquainted with likely markets for Italian products. The steamer will leave Genoa on September 1, and the whole voyage will occupy four months. A professor is to lecture during the voyage on the commercial geography of Africa.

The first mail from Kadiak Island received this season, *Science* states, has arrived at San Francisco, bringing dates to May 2. According to the correspondent of the *Bulletin*, the account of the eruption of the volcano on Augustine Island, Cook's Inlet, sent by the last advices of 1883, was much exaggerated. The island "was not split in two, and no new island was formed but the west side of the summit has fallen in, forming a new crater, while the whole island has risen to such an extent as to fill up the only bay or boat harbour, and to extend the reefs, or sea-otter rocks, running out from the island in various directions." The hunting-party feared to be lost has arrived safely in Kadiak. No tidal waves were observed on the west shore of Cook's Inlet or on Kadiak Island. The winter had been very mild, the mercury not having fallen below 10° F.; and spring began in March, wild flowers being in bloom in the latter part of April.

THE educational statistics of Japan for the past year show that the number of common schools throughout the country is 29,081, being an increase of 339 as compared with the preceding year, while the number of scholars is 3,004,137, an increase of 396,960, and the number of teachers is 84,765, being an increase of 8147.

THE Swedish Government have granted a sum of 850*l*. for the establishment of five additional so-called "chemical" stations, in order to benefit the agriculturist with scientific advice as to the crops, their diseases, &c.

THE sixth Davis Lecture of the Zoological Society of London was given in the Lecture-Room in their Gardens in Regent's Park, on Thursday, the 10th inst., by Mr. Henry Seebohm. The subject was that of "Birds' Nests," and consisted of an account of the breeding of birds on the Fern Islands off the coast of Northumberland, on the Derbyshire Moors, and in the valley of the Lower Danube. The lecturer pointed out that, so far as regards the means which birds take for the protection of their eggs, they may be classified in five groups—(1) those which rely upon the concealed position of the nest, such as the kingfishers, bee-eaters, pigeons, &c.; (2) those which rely upon the inaccessible position of the nest, such as guillemots, herons, &c.; (3) those which rely upon the protective colour of the eggs, such as sandpipers, terns, &c.; (4) those which rely upon the protective colour of the sitting hen, such as the blackbird, game birds, &c.; (5) those which rely upon their own ability, either singly, in pairs, or in colonies, to defend their eggs, such as cormorants, birds of prey, &c. Mr. Seebohm laid great stress upon the much greater interest to be found in the study of the life and habits of birds than in the investigation of the form and colour of their feathers or the peculiarities of their anatomy.

UNDER the auspices of the East India Association, a meeting of naturalists, planters, sportsmen, and others interested in the affairs of India, was held on Friday, July 11, at the rooms of the Zoological Society, under the presidency of Prof. Flower, LL.D., F.R.S. (Director of the British Museum Natural History Department, and President of the Zoological Society), for the purpose of urging the necessity of Government measures for the preservation of wild birds in India. The principal address was delivered by Mr. Robt. H. Elliot, sometime planter of Mysore, and a well-known writer upon Indian topics. He pointed out that every civilised Government with the exception of that of India has recognised the value of birds as insect-eaters, and has adopted measures for their preservation; and that the absence of legislation forebodes, where it has not yet presented, serious results to planters and agriculturists. As the most convenient season for the destruction of birds is during the fine weather that succeeds the heavy rains of the monsoons, and as this season is also the breeding time, the destruction of insect-eating birds proceeds at such a rate as must soon lead to almost absolute extermination unless preservative measures are speedily adopted. There was a general agreement at the meeting that legislation on the subject is imperatively required; and it was resolved that a representation to that effect should be addressed to the Government of India.

MR. J. H. ANGAS, who has already founded an engineering scholarship of the annual value of 200*l*. in the Adelaide University, has signified his intention of endowing a Chair of Chemistry. For this purpose Mr. Angas is prepared to give the sum of 6000*l*., and to pay an annual sum of 350*l*. until he pays over the capital sum. A letter to this effect from Mr. J. H. Angas was recently read by the Chancellor at a special meeting of the Council. The Council resolved to accept the gift, and authorised the Chancellor to write and thank Mr. Angas for his continued munificence to the University.

PROF. MILNE of Tokio, Japan, writes to us:—"A short time ago I described a pair of conical pendulum seismographs. Each seismograph consisted of a heavy mass suspended by a string, &c. A local paper describing this innocently gave to the world an account of a pair of 'comical pendulums.' Each 'comical' pendulum consisted of a heavy 'man' suspended at the end of a string, &c. These errors, which were repeated throughout the article, did so much to popularise the instrument that their correction was neglected."

A RECENT number of *L'Exploration* contains an article by the Chancelier of the French Consulate at Hanoi, M. Aumoitte, which possesses special interest at the present time. It is the record of a journey from Hanoi through Bacninh to Langson on the Chinese frontier, thus following the same route as the French troops have done in their recent operations in Tonquin. From Hanoi to the meeting of the provinces of Bacninh and Langson, the country is described as populous and fertile, but on entering the latter district it becomes mountainous, with bad roads, and almost depopulated by the brigand hordes which have infested this borderland. Almost everywhere the water is bad, and fever rife even amongst the natives. Nowhere is this the case more than at Bacle, where the French forces are now hurrying up to retrieve the recent reverse at Langson. The country around Langson is described as healthy, it is rich in rice and cotton, but the trade here, as all along the valley of the Red River, is in the hands of the Chinese. When the French occupy this region, and when the recent treaty is carried out, we may expect a vast addition to our scanty knowledge of the geography of this little visited region.

FROM a paper contributed by the veteran scholar, Dr. Edkins, to a recent number of the *Chinese Recorder*, it appears that about B.C. 2200 the Chinese possessed a knowledge of the art of writing, a year of 366 days with an intercalary month, the astrolabe, the zodiac, the cycle of sixty, of twelve musical reeds forming a gamut, which also constituted the basis of a denary metrology for measures of length, weight, and capacity, divination, and a feudal system. In B.C. 1130 they were acquainted with the clepsydra and with the gamut of five musical notes. "Human knowledge was systematised in a scheme of numerical categories in which the five elements played a special part." About B.C. 550 the silk trade sprang into existence, the stars were classified, foreign names of unknown origin were introduced for the purpose of applying the cycle of sixty-two years. About B.C. 140 Chinese travellers visited Afghanistan, India, Bactria, &c. The cycle of Calippus was introduced into Chinese astronomy, and geographical knowledge concerning western nations was acquired. In the first two centuries of our era trade became more extended by sea; paper-making and the manufacture of ink were introduced from Europe, the Babylonian cosmogony became the main element of the Taoist cosmogony, and the gamut of five was increased to seven notes.

THE last volume (ii.) of the *Revue d'Ethnographie* contains, among others, articles by M. Bertrand on the Troglodytes, the introduction of metals into the West, and the great routes of migration and commerce in the earliest historical period; by M. Charnay, on the ruins in Yucatan; by M. Deniker, on the Giliaks; by Dr. Martin, on the cranial malformation of the

Chinese; by M. Ujfalvy, on the Aryans north and south of the Hindoo Koosh; and by Baron Vaux, on the Kanakas of New Caledonia.

THE additions to the Zoological Society's Gardens during the past week include two Quebec Marmots (*Arctomys monax* ♂ ♀) from North America, presented by Mr. N. Stainfield; a Prairie Wolf (*Canis latrans* ♂) from North America, presented by Mr. R. Payze; three Suricates (*Suricata tetradactyla*) from South Africa, presented by Mr. W. R. Dobbin; two Red-beaked Weaver Birds (*Quelea sanguinirostris* ♂ ♀) from West Africa, presented by Mrs. Nettleship; two Swift Parrakeets (*Lathamus discolor* ♂ ♀) from Tasmania, presented by Mr. J. Abrahams; four Common Vipers (*Vipera berus*), British, presented by Mr. Walter E. Blaker; two Smooth Snakes (*Coronella lævis*), British, presented by the Rev. Charles Harris; two Red Kangaroos (*Macropus rufus* ♂ ♀), a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, deposited; four Beautiful Finches (*Estrelda bella* ♂ ♂ ♀ ♀) from Australia, a Lanner Falcon (*Falco lanarius*), captured at sea, purchased; a Hybrid Luhdorf's Deer (between *Cervus luehdorfi* ♂ and *C. canadensis* ♀), a Hybrid Mesopotamian Deer (between *Dama mesopotamica* ♂ and *D. vulgaris* ♀), a Red Deer (*Cervus elaphus* ♀), four Australian Wild Ducks (*Anas superciliosa*), a Mandarin Duck (*Æx galericulata*), bred in the Gardens.

ZOOLOGICAL NOMENCLATURE [1]

II.

DR. COUES said that he was much gratified at the interest shown in the subject of zoological nomenclature, and indorsed the words of the Chairman that names were of the greatest possible consequence. Nomenclature was a necessary evil, and the point was always to employ that method of naming objects which should most clearly reflect not only the characters of the objects themselves, but our ideas respecting those characters and the view we take of them. As to what constitutes a species, there had been an absolute revolution in the definition of a species since the time of Linnæus, the opinion having been long held that every species was a distinct and individual creation. But that idea had passed out of existence in the minds of most natural historians of the present day, who accepted a general theory of the evolution of species by a gradual modification. That being the case, it was idle to inquire "What is a species?" no such thing existing any more than a genus; and so intimately related were all forms of animal and vegetable life that, if they were all before us, no naming would be possible, for each would be found to be connected completely with another; therefore the possibility of naming any species was, as it were, the gauge and test of our ignorance. Having thus touched very briefly upon the subject of missing links, which alone enable us to name objects which still exist, Dr. Coues proceeded to inquire, "What of so-called species the connecting links between which are still before our eyes?" In illustration of this he would cite some instances of connecting links which exist between certain forms. He then referred to the case of one of the best-known Woodpeckers in North America (*Picus villosus*), and discussed its climatal and geographical variation. He was of opinion that all these geographical races were indistinctly separable forms, and he would indicate them by trinomial names, proceeding upon the definite principle of geographical variation according to conditions of environment, meaning by this all the external influences which modify the plastic organism. Moisture, the humidity of the atmosphere, appeared to have the greatest effect, particularly in regard to colour. Latitude, with its varying degrees of heat, determined size more than any other influence. As a matter of fact this condition of things was found to occur, and the question was, How should we recognise it in our language? Specification had ceased to be of use, and the question was whether the system in favour in America was sufficient or insufficient to meet the case. On these points he would be glad to hear opinions; and in concluding he would read a paragraph from the new edition of the "Key to North American Birds," giving formally the rule for the employment of trinomials as now in use by American ornithologists and many other zoologists of the United States. This rule is as follows:—

"No infallible rule can be laid down for determining what shall be held to be a species, what a con-species, sub-species, or variety. It is a matter of tact and experience, like the appreciation of the value of any other group in zoology. There is, however, a convention upon the subject, which the present workers in ornithology in this country (America) find available—at any rate, we have no better rule to go by. We treat as 'specific' any form, however little different from the next, that we do not know or believe to intergrade with that next one;—between which and the next one no intermediate equivocal specimens are forthcoming, and none, consequently, are supposed to exist. This is to imply that the differentiation is accomplished, the links are lost, and the characters actually become 'specific.' We treat as varietal of each other any forms, however different in their extreme manifestation, which we know to intergrade, having the intermediate specimens before us, or which we believe with any good reason do intergrade. If the links still exist, the differentiation is still incomplete, and the characters are not specific, but only varietal, in the literal sense of these terms. In the latter case, the oldest name is retained as the specific one, and to it is appended the varietal designation." [1]

Dr. Günther, F.R.S., said that during the whole of this discussion it appeared to him that this new movement was in fact a reaction. It had always appeared to him that ornithologists went too far in attaching to the slightest modification of colour the rank of a species; and when he looked over the list of species of a genus well known to him, he found there a number of different forms distinguished for very different reasons, and could not help being struck by the great diversity of value which was attached to the distinctive characters of these various forms. There was nothing to show that there was any system in distinguishing and naming the species of birds. He looked with favour on the method proposed by Dr. Coues and his compatriots. It was a system he had himself employed occasionally in his systematic writings since 1866, and Dr. Coues would find that in some cases he had adopted it pure and simple. He (Dr. Günther) had been disappointed in looking over the new edition of Dr. Coues' "Key to North American Birds," for he found there that Dr. Coues adopted trinomials in some cases and binomials in others. He maintained that logically one ought to adopt the trinomial nomenclature for all other forms, and keep the binomial only for that category in which these varieties may be contained. If Dr. Coues and those who were with him adopted that system, he for one would gladly employ it in all those cases in which the geographical range of certain forms is clearly ascertained.

Dr. Sclater, F.R.S., would remind Dr. Coues that this mode of designating the forms of animal life was by no means a new one, as might be seen on reference to Schlegel's "Revue Critique," published in 1844. He thought the only difficulty lay in the extent to which it was likely to be carried out. Dr. Coues, in his preface to the new "Key to the Birds of America" had hinted at this difficulty. If too much stress were laid upon the value of trinomialism we should open the flood-gates to an avalanche of new names by naturalists who have not taken enough trouble to investigate the matter under consideration. The time had now come when it would be advisable to a certain extent to use trinomials. He could not at all agree with Dr. Coues when he said that no such thing as species exists, for forms were known which had all the characters of well-marked species. It was only in cases where faunæ had been fully worked out that trinomial names would come into use, and for such forms he was quite prepared to adopt the system.

Mr. Blanford, F.R.S., said that he would add one word to the discussion, as nobody else had taken up the one or two points which might be advanced in opposition to the proposed system. He thought the movement an unfortunate one, for the reason that it would certainly have the effect of rendering nomenclature in general less certain than it was before. An equation containing three variables was much more complicated than one in which there were only two, and when one had three names any one of which was liable to be changed to suit personal views the fixture of nomenclature would be even further off than it is now. Then the case of ornithology was not nearly, in point of fact, so complicated as some other classes, as, for instance, in the Mollusca. Trinomial nomenclature had been proposed to,

[1] Continued from p. 259.

[1] A more formal and elaborate presentation of Dr. Coues' views may be found in the *Zoologist* for July, 1884, p. 241, being the verbatim report of the address delivered before the National Academy of Sciences at Washington U.S.A., in April last.—ED.

but almost universally rejected by, a meeting of geologists. He did not consider that the time had arrived for any innovation, and thought it desirable first to agree upon strict rules of zoological nomenclature.

Prof. Bell agreed with Mr. Blanford that the method would not be universally applicable. How could it be applied for instance to cases where varieties were found living with one another, as was often the case with littoral forms with free-swimming embryos?

Mr. W. F. Kirby said that it was necessary to distinguish sub-species and varieties at times, and there seemed to be only two courses open to us, either to retain the binomial nomenclature, and to treat sub species, so far as nomenclature was concerned, as equivalent to species, or to retain varietal names. Still it was difficult to lay down hard and fast rules, applicable to all cases; and he feared that the system of naming varieties was liable to great abuse, especially in entomology, where the number of species is already so great. Thus we have 100,000 species of *Coleoptera* on our lists; and one of the most variable families is that of the *Coccinellidæ*, in which some entomologists have lately begun to name mere colour varieties of single species by twenty and thirty at a time. Mr. Kirby thought, too, that, whenever a named form previously regarded as a variety was held by a later author to be worthy of specific rank, the varietal name should, wherever practicable, always be retained for the species, instead of a new one being imposed. He knew that this was not always adhered to, but in his own work he made it an invariable rule.

Lord Walsingham said that, as Dr. Coues suggested that the trinomial system should be used only in distinguishing gradations, he would instance two species very common in England, small species of the genus *Teras* (*Teras hastiana* and *Teras cristana*). These exhibited a very extensive series of individual variations, but some varieties, although perhaps reared from the same brood of larvæ, showed marked differences not distinctly connected by intermediate gradations with other forms of colouring. He asked how the method was to be applied to these if indeed it was intended to be applied at all to such cases. He himself knew several cases in North America in which variation, according to latitude, is very marked. You get a form in Vancouver Island gradually merging into the form in California, and in Mexico something apparently distinct if taken by itself, and probably only an extreme variety in colour and markings, but you have no form for the South of California. Would it be proposed to treat this Mexican form as a proper subject for the trinomial system or to give it only two names as at present? The principle appears to be right provided it facilitates the recognition of the forms we are naming. He hoped there was no danger of drifting into the inconvenient multiplication of names too commonly known in the catalogues of professional horticulturists.

Dr. Sharp said that whatever names we gave to morphological forms of less than specific value, whether we called them varieties, or sub-species, or morphological forms, we could not define or limit them; and if we attempted to name them, as no line could be drawn, we must go on till we gave a separate name to every individual that had passed through the hands of zoologists. He considered Dr. Coues' system of a third name unnecessary, because all the purposes it sought to attain could be accomplished without it by the old-fashioned system of "var. *a*, var. *b*," and so on. The adoption of a system of names for forms lower than species he thought would lead to complete chaos.

Dr. Woodward, F.R.S., said he might mention two cases which occurred to him in which perhaps the system would be convenient. It was considered desirable by many palæontologists that the group of Ammonites should be broken up into a number of genera, and he thought the present plan of erecting specific names into generic ones was inconvenient. The student was already hampered with too many names, and we ought to remember that students were harder worked now than they were twenty-five years ago. The system of cramming he considered deteriorating to the stamina of the future naturalist. Every time a group was broken up into genera, sub-genera, species, and sub-species, the labour of the student was increased. Therefore it appeared to him that the use of a third term following the generic and specific one, as proposed, was very convenient if not insisted on as a matter of instruction.

Mr. H. T. Wharton would prefer not to see other names troduced unless they were absolutely necessary. But when ell-marked intermediate forms had to be dealt with he admitted the value of the trinomial system, but of course Dr. Coues knew that the method advocated by him was not new to naturalists, for trinomial names were to be found in botanical catalogues. He should be glad to know how it was proposed to deal with such a form as *Corvus cornix*, for example, which in the new edition of Yarrell's "British Birds" had been united with *Corvus corone*.

Mr. H. Saunders said he would like to direct attention to a practical point of the question. Most of those present were aware that there was an unpretending annual called the *Zoological Record*, which consisted now of about 800 pages, and that if trinomialism were adopted, it would make the volume of too great a size. He would also remind those present that Mr. Sharpe was the recorder of Aves, and he did not know how that gentleman would relish the additional labour which would be thrown upon him if this system were generally adopted.

Dr. Traquair said:—I think I quite understand the scope and limits of the system so ably advocated by Dr. Coues, but I feel convinced that were any such system to receive the authoritative sanction of naturalists, these limits would not be observed by the ordinary crowd of name manufacturers. My own studies in recent zoology have been more especially of an anatomical and morphological character, but in the subject of fossil ichthyology I have been brought face to face with the question of the definition and naming of species. Here I conceive that the "species" must include all those forms which can be indubitably shown to graduate into each other. For such species, the only idea of a species which seems to me practicable, one generic and one specific name are quite enough, and I would leave each author to deal with "sub-species" and varieties as he pleased, but without permitting him to apply any *authoritative* name to such. So great, in many cases, is the amount of variation observed in fossil fishes and fish remains, and so difficult is it also to arrive at safe conclusions as to specific identity or distinction with the material before us, that, were the proposed system of trinomial nomenclature to receive the authoritative sanction of naturalists, I am convinced that in this department the flood-gates would simply be opened for a deluge of new names, from people whose sole function in life seems to be to invent such on the most trifling pretext. If the binomial system is at present often abused by such people for the creation of "species" which have no existence, save in their own imaginations, what might we not expect them to do if the adoption of a trinomial system afforded them further scope for their faculties!

Mr. J. E. Harting said if he could be satisfied that the introduction of a system of trinomial nomenclature, as proposed, would be of any real benefit to science, he should have no hesitation in adopting it. But, so far from any advantage resulting from it, he feared that a positive disadvantage would accrue from its adoption in a way which had not been sufficiently considered. The tendency to describe as new species mere individual variations had already (with certain specialists at least) become very prevalent, and had led to an expression of regret and dissatisfaction amongst those who were content to take a broader view of things, and who regarded such a process of refining as tending to perplex, while in no way advancing science. All workers in zoology found themselves sooner or later in one of two classes, which had been named, expressively, if not elegantly, "lumpers" and "splitters." Now, if the proposed system of trinomial nomenclature were to be adopted, the former class would have either to surrender at discretion to the latter, or a wider gap than ever would be created between them, a result which would surely lead to great inconvenience; while the latter, who had already gone to great lengths in what he had termed the process of refining, would receive fresh encouragement to go to still greater lengths in that direction, to the disadvantage, as he conceived, of those who were to come after them. We had been told by the advocates of the trinomial system that it was impossible not to recognise climatic variations in any given species when they were found to be constant and well marked. In this he agreed: he only dissented from them in regard to the mode by which such recognition was to be effected. To say that the only mode of recognising such variations was to add a third name to the generic and specific names was begging the question. If any such variation as that alluded to was sufficiently well marked to distinguish it at once from the species of which it was said to be a variety, he would prefer to regard it as an allied species, and bestow on it a specific name, retaining a binomial nomenclature. The binomial system had been found to work well enough in practice, from its simplicity; and it was surely simpler to write *Turdus propinquus* than *Turdus migratorius propinquus*. After all, nomenclature was not science, and even if we had the most perfect system of nomenclature which could be devised, he did not see how science would be thereby advanced. It is true we could not get on without nomenclature, but the simpler it was the better; and the less time we spent in discussing it the more we should have to devote to real study.

Dr Coues, replying to previous speakers, said that the system of trinomial nomenclature had nothing whatever to do with individual variations of specimens from one locality. It was not a question of naming varieties or hybrids, but there was a definite principle to proceed upon, namely, that of geographical and climatal variation. He was well aware that the use of three names to designate objects in zoology was no new thing; but he believed that the restricted application of trinomialism to the particular class of cases he had discussed was virtually novel, and that his system would prove to be one of great practical utility. He thought that the application of the principle was a question which, after this discussion, and after further private discussions, might well be left to the discretion of authors.

The Chairman concluded the meeting by saying:—I hope that Dr. Elliott Coues is satisfied with the manner with which his views have been received. Although there are some uncompromising binomialists present, many have pronounced themselves as what may be termed limited trinomialists, and some appear to go as far as Dr. Coues himself. Distinctly defined species undoubtedly exist in great numbers, owing to extinction of intermediate forms; for these the binomial system offers all that is needed in defining them. But on the other hand there are numbers of cases in the actual state of the earth, and far more are being constantly revealed by the discoveries of palæontology, and nowhere so rapidly as in Dr. Coues' own country, where the infinite gradations defy the discrimination either of a binomial or a trinomial system. Zoologists engaged in the question of nomenclature are being gradually brought face to face with an enormous difficulty in consequence of the discovery of these intermediate forms, and some far more radical change than that now proposed will have to be considered. In conclusion I must express the thanks of the meeting to Dr. Coues for having brought his views and those of his countrymen, of whom he is such a worthy representative, before us, and also to Mr. Bowdler Sharpe, to whose zeal and energy the organisation of the meeting is entirely due.

A unanimous vote of thanks was given to Prof. Flower for presiding.

KRAKATOA

AT the meeting of the Meteorological Society of Mauritius on May 22 some interesting communications were made relating to the Krakatoa eruption. Among others was a letter from M. Lecomte regarding detonations heard at Diego Garcia on August 27. In his letter, which was written at Diego Garcia on April 24, M. Lecomte says:—"Le lundi 27 août entre 10 et 11 heures du matin, pendant le déjeûner, nous avons entendu des détonations sourdes mais violentes. Nous avons cru tellement à l'appel d'un navire en détresse que nous avons couru et que j'ai envoyé plusieurs hommes vers le rivage extérieur de l'île sur plusieurs points différents, en observation. Le Capitaine Florentin de l'*Eva Joshua* et son second, M. Daniel Sauvage, venaient de quitter Pointe de l'Est pour aller mouiller à Pointe Marianne, lorsqu'ils ont entendu les mêmes détonations. Ils ont aussitôt envoyé des hommes en observation à l'extrémité des mâts. Mais comme les miens ils n'ont rien vu.

"Ce jour là et les jours suivants le soleil était comme obscurci, probablement par la formidable quantité de vapeurs et de cendres qui ont du s'élever dans l'atmosphère."

The information obligingly furnished by M. Lecomte was valuable, inasmuch as, take n in conjunction with the reports which had been received from Rodrigues, it confirmed verbal information which had been previously obtained. There could now be no doubt that the explosions which took place at Krakatoa were distinctly heard both at Diego Garcia and Rodrigues, and there was probably no other recorded instance of sound having travelled over so great a distance. The fact, also, that at Diego Garcia the sun was partially obscured on August 27 and on several subsequent days, as well as at the Seychelles and Rodrigues, was an additional proof of the great quantity of matter which must have been ejected from Krakatoa, and of the rapidity with which it was conveyed from its source. There could be no reasonable doubt that the presence of that matter in the atmosphere was the cause of at least the lurid sunsets and sunrises which were observed over the Indian Ocean on the last days of August and in the first week of September.

The Secretary, Dr. Meldrum, stated that the Royal Society of London had appointed a Committee to collect information regarding the phenomena which had been observed during and after the volcanic eruptions that took place at Krakatoa in August, and requests had been received from that and other quarters for information from Mauritius. To these requests the Secretary had replied that he was preparing for his Excellency the Governor a detailed account of what had been observed at Mauritius and several of its dependencies, but that owing to the almost daily reception of additional details his report was not yet ready. All he did, therefore, was to give the general results as far as they had been determined.

Several remarkable phenomena had to be described. In the first place, there were disturbances of the sea water, or, as they had been called by some, tidal disturbances, and these had been observed all over the Indian Ocean.

There were also barometric disturbances, to which attention had first of all been called in Mauritius early in September, and which at the time were ascribed to the explosions at Krakatoa. Some time afterwards it was ascertained in England that these disturbances had extended over the whole globe and that they were recorded by all self-registering barometers in both hemispheres. At Mauritius there were at least seven well-marked disturbances of which the epochs of *maximum intensity* were as follows:—

			h. m.		
(1)	August	27,	0.6	p.m.	local time.
(2)	,,	28,	2.20	p.m.	,,
(3)	,,	28,	10.40	p.m.	,,
(4)	,,	30,	1.35	a.m.	,,
(5)	,,	30,	9.17	a.m.	,,
(6)	,,	31,	1.48	p.m.	,,
(7)	,,	31,	8.00	p.m.	,,

At first these disturbances were supposed in Mauritius to have been due to successive eruptions, but General Strachey, who examined a number of barographs received from different parts of the world, had recently adduced evidence to show that they were produced by an air-wave proceeding outwards from Krakatoa in all directions round the earth, expanding till it was half round, then contracting till it reached the antipodes of its origin, and afterwards returning, the wave thus travelling round the globe two or three times. Assuming that view, the first disturbance at Mauritius (which was at its maximum at oh. 6m. p.m. on the 27th) would be caused by the passage over the Observatory of the wave travelling from east to west; and the third, fifth, and seventh disturbances would be returns of the wave to Mauritius after having gone round the earth. Similarly the second disturbance would be the first passage of the wave travelling from Krakatoa eastward, and the fourth and sixth would be its returns to Mauritius. Now, the mean interval in time between the returns of the wave to Mauritius, in its passage from east to west, was 24h. 38m., and in its passage from west to east 35h. 44m. It would thus appear that the rate of progression had been greater from east to west than from west to east, which may have been partly or wholly due to the great circle passing through Krakatoa and Mauritius being within the tropics, where the prevailing wind was from the eastward. The rate of progression from east to west was very nearly 709 miles an hour, and from west to east 697 miles. By taking as nearly as possible the times half way between the commencements and endings of the disturbances similar results were obtained. There was also an *eighth* (but small) disturbance between 7 and 9 a.m. on September 2, which may have been the fourth return of the wave from east to west, the interval in time between that disturbance and the seventh having been nearly thirty-six hours. The sixth disturbance was the last indication of the wave in its passage from west to east.

Another effect of the Krakatoa eruptions was the spread of ashes and pumice over considerable portions of the Indian Ocean, and a good deal of information on that point also had been collected in Mauritius. The first intimation of the probability of volcanic action in the direction of the Straits of Sunda was contained in a letter published by Capt. Walker, of the *Actæa* in the *Mercantile Record* of June 16, 1883. At noon on

May 20 the *Actæa* was in 6° 50′ S. and 104° 2′ E., and on the morning of that day a "peculiar light green colour" was observed in the sky to the east-south-east, while "from east to east-north-east there was a dark blue cloud, which reached from the horizon to the zenith." "About 2 p.m. it was quite dark. What appeared to be a rain squall rose up from the east, but, instead of rain, a kind of very fine dust commenced to fall, and very soon everything was covered; ships, sails, rigging and men were all dust colour; nothing could be seen 100 yards off. The fall continued steadily all night, and stopped about 9 a.m. on Monday the 21st. When we saw the sun it looked like dull silver. At noon we were in lat. 8° 15′ S. and long. 102° 28′ E., distant from Java Heads about 170 miles. The sky all round remained a dusty hue, and small quantities of dust again fell during the night. The sky did not assume a natural appearance till the 23rd." At a meeting of this Society held on July 12, the Secretary called attention to Capt. Walker's letter, and said there was little doubt that the dust in question had come from Krakatoa, as, according to a note in NATURE of June 7, a volcano in that island was in full eruption. From that time accounts of pumice and ashes observed in the Indian Ocean had been extracted from log-books, and they showed that on several occasions vessels had passed through fields of pumice long before the great eruptions of August 26 and 27. After that month the reports became more frequent, and they still continued, the latest being from the vicinity and shores of Mauritius, where, since the middle of February, large quantities of pumice had been seen. It would appear, however, that fields of pumice had passed Mauritius long before February, for "a large quantity of pumice-stone and lava was washed up on the beach at Durban (Natal) on October 23." According to the reports received, fields or lanes of pumice had been observed in different parts of the Ocean from 105° to 48° E. and 6° to 12° S. Farther south the extent in longitude had been apparently less.

That the remarkable sunrises and sunsets which had been observed over a great part of the world after August 27 were due to matter ejected from Krakatoa seemed to be generally admitted. The few who objected to the volcanic dust theory had not proposed any other theory that so completely accounted for the facts. The presence of vapours and finely-divided dust at certain elevations would, as a consequence of known physical laws, produce all the chromatic effects that had been seen and described, and it was known that immense quantities of matter had been shot up from Krakatoa. Similar phenomena had been witnessed by observers between whom and the sun volcanic dust passed, as on the occasion of an eruption of Cotopaxi a few years ago. But it was not necessary to go so far back. From May 20 to 22 last, after an eruption of Krakatoa, Captain Walker, as already stated, observed that to the east-south-east the sky was of a light green colour, that on the 21st the sun looked like dull silver, that the sky all round was of a dusty hue, and that it did not assume its natural appearance till the 23rd. That was perhaps the earliest instance of the chromatic effects of the Krakatoa dust and vapours. Immediately after the eruptions of August 27 they were more intense and on a greater scale. At the Seychelles on the 27th the sky, according to Mr. Estridge, was hazy all day. The sunset on that day was gorgeous; the sky was lurid all over, and beams of red light stretched from over St. Anne's to nearly the horizon. At sunset on the 28th the sun looked as it did through a fog on a frosty day in England. On the morning of the 29th the sun at 7 a.m. was more like a full moon than anything else. According to other letters from the Seychelles the sun for a whole week appeared dim. At Rodrigues, according to Mr. Wallis, whose report was written on August 31, the sky at north-west on every evening since the 27th had a very threatening and strange appearance of a deep purplish red colour, which lasted till 7.15 p.m., and which, with the disturbances of the sea water, caused much fear and excitement. Similar phenomena were observed on the same evenings at Diego Garcia and St. Brandon, and for several days the sun looked as if partially obscured. At Mauritius the sky was overcast throughout the whole of the 27th, and it was observed and noted at the time that there was an unusual dimness. On the evening of the 28th there was a gorgeous sunset, the first of a long series of remarkable colorations and glows, which had already been described. Observations of these optical phenomena had been taken daily during nearly the last nine months whenever the weather permitted. Knowing what had been observed on board of the *Actæa*, and that Krakatoa had been in eruption, these extraordinary sunsets and sunrises were attributed to the presence in the upper strata of the air of finely-divided matter, and probably gases and vapours, from Krakatoa, and subsequent events confirmed that opinion. It was difficult to explain phenomena which had been identical under all conditions of weather, and in many distant places, by any purely meteorological causes. To the meteoric dust theory it might be objected that it was purely an hypothesis almost, if not wholly, unsupported by facts. No unusual number of meteors had been seen. No extraordinary glows had been observed at or near the times of the great meteoric showers of November 1866, and November 1872. Moreover one would suppose that if the earth had for months been passing through volumes of meteoric dust the chromatic effects would have appeared simultaneously wherever the sun rose and set. But such had not been the case. Upon the whole there seemed to be a preponderance of evidence in favour of the volcanic dust theory. The objection that the quantity of matter was insufficient was not a formidable one, for the effects did not depend merely upon the quantity of matter that had reached the higher regions, but also upon its form and degree of tenuity. A few pounds of matter might be spread over thousands of square miles. As to the objection that it was difficult to conceive how even finely-divided matter could remain so long in suspension, it might be remarked that, independently of the possibility of the particles being electrified, the lower strata of the atmosphere might be denser than the foreign matter in the upper strata. The extraordinary sunsets and sunrises which were observed in 1783–84, and which Arago and others ascribed to volcanic dust, were said to have lasted eleven months. Those of 1883–84 would probably last fully as long. Within the last few weeks there had been at Mauritius a considerable increase in the intensity and duration of the glows.

EVIDENCES OF THE EXISTENCE OF LIGHT AT GREAT DEPTHS IN THE SEA [1]

THE evidences of the presence of light and its quality and source at great depths are of much interest. At present very little experimental knowledge in regard to these questions is available. That light of some kind, and in considerable amount, actually exists at depths below 2000 fathoms, may be regarded as certain. This is shown by the presence of well-developed eyes in most of the fishes, all of the cephalopods, most of the decapod Crustacea, and in some species of other groups. In many of these animals, living in 2000 to 3000 fathoms, and even deeper than that, the eyes are relatively larger than in the allied shallow-water species; in others the eyes differ little, if any, in size and appearance, from the eyes of corresponding shallow-water forms; in certain other cases, especially among the lower tribes, the eyes are either rudimentary or wanting in groups of which the shallow-water representatives have eyes of some sort. This last condition is notable among the deep-water gastropods, which are mostly blind; but many of these are probably burrowing species; and it may be that the prevalent extreme softness of the ooze of the bottom, and the general burrowing habits, are connected directly with the habits or rudimentary condition of the eyes in many species belonging to different classes, including Crustacea and fishes. Such blind species usually have highly developed tactile organs to compensate for lack of vision.

Other important facts bearing directly, not only on the *existence*, but on the *quality*, of the light, are those connected with the coloration of the deep-sea species. In general, it may be said that a large proportion of the deep-sea animals are highly *coloured*, and that their colours are certainly *protective*. Certain species, belonging to different groups, have pale colours, or are translucent, while many agree in colour with the mud and ooze of the bottom; but some, especially among the fishes, are very dark, or even almost black; most of these are probably instances of adaptations for protection from enemies, or concealment from prey. But more striking instances are to be found among the numerous brightly-coloured species belonging to the echinoderms, decapod Crustacea, cephalopods, annelids, and Anthozoa. In all these groups, species occur which are as highly coloured as their shallow-water allies, or even more so. But it is remarkable that in the deep-sea animals the bright colours are almost always shades of orange and orange-red, occasionally brownish red,

[1] From a paper in *Science*, July 4, on "Results of Dredgings in the Gulf Stream Region by the U. S Fish Commission."

purple, and purplish red. Clear yellow, and all shades of green and blue colours, are rarely, if ever, met with. These facts indicate that the deep sea is illuminated only by the sea-green sunlight that has passed through a vast stratum of water, and therefore lost all the red and orange rays by absorption. The transmitted rays of light could not be reflected by the animals referred to, and therefore they would be rendered invisible. Their bright colours can only become visible when they are brought up into the white sunlight. These bright colours are therefore just as much protective as the dull and black colours of other species.

The deep-sea star-fishes are nearly all orange, orange-red, or scarlet, even down to three thousand fathoms. The larger ophiurans are generally orange, orange-yellow, or yellowish white, the burrowing forms being usually whitish or mud-coloured, while the numerous species that live clinging to the branches of gorgonians and to the stems of Pennatulacea are generally orange, scarlet, or red, like the corals to which they cling. Among such species are *Astrochele lymani*, abundant on the bushy orange gorgonian coral, *Acanella normani*, often in company with several other orange ophiurans belonging to Ophiacantha, &c. *Astronyx loveni* and other species are common on Pennatulacea, and agree very perfectly in colour with them. These, and numerous others that might be named, are instances of the special adaptations of colours and habits of commensals for the benefit of one or both. Many of the large and very abundant Actiniæ, or sea-anemones, are bright orange, red, scarlet, or rosy in their colours, and are often elegantly varied and striped, quite as brilliantly as the shallow-water forms; and the same is true of the large and elegant cup-corals, *Flabellum goodei*, *F. angulare*, and *Caryophyllia communis*,—all of which are strictly deep-sea species, and have bright orange and red animals when living. The gorgonian corals of many species, and the numerous sea-pens and sea-feathers (Pennatulacea), which are large and abundant in the deep sea, are nearly all bright coloured when living, and either orange or red. All these Anthozoa are furnished with powerful stinging organs for offence and defence; so that their colours cannot well be for mere protection against enemies, for even the most ravenous fishes seldom disturb them. It is probable, therefore, that their invisible colours may be of use by concealing them from their prey, which must actually come in contact with these nearly stationary animals in order to be caught. But there is a large species of scale-covered annelid (*Polynoë aurantiaca*, Verr.) which lives habitually as a commensal on *Bolocera tuediæ*, a very large orange or red actinian, with unusually powerful stinging organs. Doubtless the worm finds, on this account, perfect protection against fishes and other enemies. This annelid is of the same intense orange colour as its actinian host. Such a colour is very unusual among annelids of this group, and in this case we must regard it as evidently protective and adaptive in a very complex manner.

It has been urged by several writers, that the light in the deep sea is derived from the phosphorescence of the animals themselves. It is true that many of the deep-sea Anthozoa, hydroids, ophiurans, and fishes are phosphorescent; and very likely this property is possessed by members of other groups in which it has not been observed. But, so far as known, phosphorescence is chiefly developed in consequence of nervous excitement or irritation, and is evidently chiefly of use as a means of defence against enemies. It is possessed by so many Anthozoa and acalephs which have, at the same time, stinging organs, that it would seem as if fishes had learned to instinctively avoid all phosphorescent animals. Consequently it has become possible for animals otherwise defenceless to obtain protection by acquiring this property. It is well known to fishermen that fishes avoid nets, and cannot be caught in them, if phosphorescent jelly-fishes become entangled in the meshes; therefore it can hardly be possible that there can be an amount of phosphorescent light, regularly and constantly evolved by the few deep-sea animals having this power, sufficient to cause any general illumination, or powerful enough to have influenced, over the whole ocean, the evolution of complex eyes, brilliant and complex protective colours, and complex commensal adaptations.

It seems to me probable that more or less sunlight does actually penetrate to the greatest depths of the ocean in the form of a soft sea-green light, perhaps at two thousand to three thousand fathoms equal in intensity to our partially moonlight nights, and possibly at the greatest depths equal only to starlight. It must be remembered that in the deep sea, far from land, the water is far more transparent than near the coast. A. E. VERRILL

ARTIFICIAL LIGHTING[1]

In early times but a small fraction of our forefathers' lives was spent under artificial light. They rose with the sun and lay down to rest shortly after sunset. During the long winter evenings they sat round the fire telling stories and singing songs of love and war; the fire-light was sufficient for them, except occasionally during grand feasts and carousals, when their halls were lighted by pine-wood torches or blazing cressets. But, as a rule, after sunset they lived in semi-darkness.

From that early period, as man has advanced in civilisation, in the thirst for knowledge derived from books, and in following the gentler pursuits which demand an indoor life, there has been a steady increase in that fraction of our lives which is spent under light other than that of the sun. But the improvement in the quality of the artificial light has been very slow. The ruddy lights and picturesque shadows so faithfully handed on to us by Rembrandt's pictures show us very graphically what our poets have called "the dim glimmer of the taper" of those days. A few years before the introduction of gas, Argand, by his improvements in the burners of oil lamps, enabled our fathers to see for the first time a comparatively white light, but as far as the matter we to-day propose to discuss is concerned, viz. the effect of artificial lighting, and more particularly electric lighting, on our health, we need only consider the reign of artificial light as it commenced with the general use of gas and petroleum, for then and only then could it be said to affect our health.

Prior to the introduction of the electric light we have been accustomed to consider every hour spent under artificial light as an hour during which all conditions are less favourable to perfect health than they would be during daylight. Can we now hope to ameliorate this condition of things through the agency of electricity? Before we can discuss this question I must point out to you the chief differences which exist between hours of work or recreation spent in daylight and under artificial light. In the former case we live in abundance of light. The sunlight itself exercises a subtle influence on our bodies; that mixture of heating and chemical rays which when analysed form the solar spectrum, and combined form the pure white light of daylight, is needed to enable all animal and vegetable organisms to flourish in the fullest conditions of healthful life.

In nearly all cases when the sun is up, the functions of life are in the state of fullest activity, and when it sets they sink into comparative repose. In daylight life wakes, in darkness life sleeps. In addition to the abundance of pure white light, the heat attending is only that necessary for health. The air remains unvitiated, except by our own breathing. On the other hand, when working under artificial light, we have these conditions all altered in degree:

1. We have an insufficient light; a scale of lighting by gas or by electricity which would be pronounced excessive at night-time is still far inferior to average daylight.

2. All artificial lights, whether produced by combustion, as in the case of candles, oil, gas, and petroleum, or by the incandescence of a conductor by the means of electricity, produce heat; this heat, in proportion with the light afforded, is enormously in excess of the heat given by sunlight. Electricity, as you will see hereafter, is far the best in this respect, but even it is inferior to sunlight.

3. All these same illuminants, excepting electricity, contaminate the air and load it with carbonic acid, sulphur, and other compounds—all injurious to the health and to the general comfort of the body. It will be convenient to consider the effects—first, on our health generally; second, on our eyesight in particular. I have already called your attention to the fact that that proportion of coloured rays which, when combined, form white sunlight, is that best suited to healthy life. It is necessary too to that sufficient and proper stimulus to the organic changes which go on in our bodies, and which we call a state of good health. The various artificial lights differ very widely from sunlight in this respect, that they are all more or less deficient in the rays at the violet end of the spectrum, commonly called the actinic rays, and which most probably exercise a very powerful effect on the system. It is the want of a due portion of these violet rays which makes all artificial light so yellow. Even the light of the electric arc, which is richer in these rays than any other, is still on the yellow side of sunlight. The incandescent electric light is next best in this respect; next in order come gas, petroleum, and the various oil lamps. No doubt some of you will

[1] Lecture delivered at the Health Exhibition by Mr. R. E. B. Crompton

challenge my statement that the electric arc is yellow. It has always been called a cold blue light. It is not so; it is only by comparison with the yellower light of gas or with the incandescent lamps that it appears blue; when compared with the sunlight reflected from a white cloud it will be seen to be distinctly yellow in tinge; but still both classes of electric light are far superior to all others in nearest approaching the white light of daylight, and thus satisfying the actinic action which our bodies demand.

Turning now to the comparative heating and air-vitiating properties of artificial lights which we shall find it convenient to take together, I have here a table (Table A) prepared by Dr.

TABLE A.—*Showing the Oxygen consumed, the Carbonic Acid produced, and the Air vitiated, by the Combustion of certain Bodies burnt so as to give the Light of 12 Standard Sperm Candles, each Candle burning at the rate of 120 grains per hour*

Burnt to give light of 12 candles, equal to 120 grains per hour	Cubic feet of oxygen consumed	Cubic feet of air consumed	Cubic feet of carbonic acid produced	Cubic feet of air vitiated	Heat produced in lbs. of water raised 10° F.
Cannel Gas	3·30	16·50	2·01	217·50	195·0
Common Gas..	5·45	17·25	3·21	348·25	278·6
Sperm Oil......	4·75	23·75	3·33	356·75	233·5
Benzole	4·46	22·30	3·54	376·30	232·6
Paraffin	6·81	34·05	4·50	484·05	361·9
Camphine	6·65	33·25	4·77	510·25	325·1
Sperm Candles	7·57	37·85	5·77	614·85	351·7
Wax	8·41	42·05	5·90	632·25	383·1
Stearic...........	8·82	44·10	6·25	669·10	374·7
Tallow...........	12·00	60·00	8·73	933·00	505·4
Electric Light.	none	none	none	none	13·8

Meymott Tidy, which shows the oxygen consumed, the carbonic acid produced, the air vitiated, and the heat produced by the combustion of certain bodies burned so as to give the light of twelve standard candles, to which Mr. R. Hammond has added the heat produced by a 12-candle incandescent electric lamp. From these figures you will see that the air of a room lighted by gas is heated twenty times as much as if it were lighted to an equal extent by incandescent electric lamps. When arc lamps are used, the comparison is still more in favour of electricity. You will be surprised to see from the table that our old friend the tallow candle, and even the wax candle, is far worse than gas in the proportion of air vitiated and heat produced, and you will be disposed to disbelieve it; but the fact is, that so long as candles were used light was so expensive that we were obliged to be content with little of it; in fact we lived in a state of semi-darkness, and in this way we evaded the trouble. It is only since the general introduction of gas and petroleum that we have found what an evil it is.

It is not unusual, in fact it is almost invariable, for us to find the upper stratum of air of the rooms in which we live heated to 120° after the gas has been lighted for a few hours. We have grown accustomed to this state of things, and are not surprised that when we take the library ladder to get a book from the upper shelf we find our head and shoulders plunged into a temperature like that of a furnace, producing giddiness and general malaise. If you look again at the table you will see that each gas burner that we use consumes more oxygen and gives off more carbonic acid, and otherwise unfits more air for breathing, than one human being, and it is this excessive heating and air vitiation combined which are the main causes of the injury to the health from working long hours in artificial light. I could go on for a long time giving instances of the fearful state of the atmosphere of our large public buildings as well as of our private homes after the gas has been lighted for a few hours, but this paper is not intended as an onslaught on gas; moreover these ills are so well known to nearly all of you that I need not bring them more prominently before you. I will only take one instance, viz. that of the Birmingham Town Hall, which has been lighted alternately by gas and electricity.

During the grand Birmingham Musical Festival, which was held in that hall two years ago, some careful experiments were made to show how the orchestra and audience in the hall were affected by the two kinds of lighting. The gas lighting was in the form of several huge pendants suspended down the centre of the hall. The electric lighting was in the form of clusters of lights placed on large brackets projecting from the side walls with two central pendants placed between the gas pendants. The candle-power given by the electric light was about 50 per cent. in excess of that given by the gas light; the degree of illumination by electricity was consequently very brilliant.

It was found that when the gas was used the temperature near the ceiling rose from 60° to 100° after three hours' lighting. The heating effect of the gas was, therefore, the same as if 4230 persons had been added to the full audience and orchestra of 3100. Similarly the vitiation of the air by carbonic acid was equal to that given off by the breathing of 3600 additional persons added to the above audience of 3100. But on evenings when the electric light was used the temperature only rose 1½° during a seven hours' trial, and the air, of course, was only vitiated by the breathing of the audience. The further experiment was tried of giving to every member composing the large orchestra a printed paper of questions asking how the new mode of lighting affected him or her personally, and I have here 265 replies to those questions. They are very interesting. I will read a very few of them out to you. From this you will learn that without exception the comfort and general well-being of this large orchestra was increased enormously by the use of the new illuminant, yet it is reasonable to suppose that the comfort of the audience was increased in an equal degree. Now we all of us know that the times when we suffer most from the effect of artificial light is in crowded places of public amusement, which are at the same time brilliantly lighted. Many of us are unable to go to the theatre or to attend evening performances of any kind, as the intense headache which invariably attends through staying a single hour in such places entirely prevents them. This headache we commonly say is inseparable from the heat and glare of the gas. Now this phrase is not strictly correct. It is no doubt due to the heat of the gas and its air-vitiating properties, but when we use the word glare I believe we refer to the effect the gaslight has upon our heads, and which effect is not due to excess of light. On the contrary, I believe if a far greater amount of light be given by the electric light without the heating and air vitiation being present such headache is never produced, although some of the more tender-headed amongst us will at first complain of the glare because they are habituated to associate plenty of light with great heat, great air vitiation, and other evils.

Indeed, so long have we been accustomed to closely associate brilliant artificial light with headache and glare, that we who are introducing electric light are most cautious not to give the full quantity of light which we could afford to give, and which would afford the greatest rest to the eye and greatest bodily comfort. I now come to the effect that light has upon the temperament. If we try the experiment in an assemblage of people of gently decreasing the lighting of the room, it will be found that the spirits of every one will be depressed just as the light is depressed, and, *vice versâ*, their spirits will be raised just as the light is raised. I have many times, when conducting experiments of electric lighting on a large scale, noticed this fact, and I have been led to the conclusion *that during hours of waking every person is benefited by increase of light up to the extent of full sunlight*, providing that this high degree of lighting is not attended by heat and by air vitiation; and I must add that the source of light must not be from one or two brilliant points only, but it must be well regulated and not such as to cause dark, deep shadows.

This leads me on to the subject of the effects on the eyesight of the electric light as compared with other lights. *Healthy eyesight demands a plentiful supply of light. It is the greatest mistake to suppose that a state of semi-darkness is good* for our eyes, unless they are defective, or recovering from the effects of past injury or disease. Whoever saw a painter, engraver, printer, watchmaker, or indeed any one the quality of whose work depends on the excellence of his eyesight, who did not desire a flood of pure white light thrown on to his work. I think I have the authority of oculists when I say that 19-20ths of the diseases of the eyes arise from working the eyesight long hours with insufficient light. Again, another great cause of injury to eyesight is the unsteadiness of most artificial lights. Much improvement has been made in the light of gas during the last few years by the introduction of argand burners, and globes for the flat gas burners having much larger lower openings, so that the dancing and flickering batswing burner of five years ago is not so common in a good house. Even the steadiest of the modern gas burners is extremely unsteady as compared with the light of the incandescent electric lamp. Those of you who have been to the Savoy Theatre will have noticed the effects of the lights behind the scenes on the scenery itself. The light is so absolutely steady that it is comparable to sunlight. Hitherto I have said nothing as to the comparative excellence of the two forms of electric light, viz. the electric arc and the incandescent lamp. Both have their proper places. The arc light, which is the whitest in colour and most economical to produce, is not so steady as the incandescent lamp. It is therefore unsuitable for indoor use or for reading by, or for such occupations as require the maximum of steadiness. But it is well suited for the lighting of large buildings and public places. I am unaware if any experiments have been made as to the effects of brilliant arc lighting on the eyesight of men who have to work night shifts, as although the opinion of the workmen who have to work under it is unanimous in its favour, yet that opinion is more based on their personal comfort, due to their being able to carry on their work with facility almost equal to that given by daylight. The large sorting rooms at the General Post Office at Glasgow have been for a long time lighted by the arc light, and with a most beneficial result to the health and eyesight of the letter-sorters and telegraph clerks. The former occupation is one which tries the eyesight very severely. The public generally does not know how the habit of writing the addresses on envelopes with pale ink and blotting it off rapidly before it has time to darken tries the eyesight of the Post Office letter-sorters. So long as gas is used, a powerful burner has to be brought very close to the head of the sorter, and under such conditions the eyesight fails at an early age. At Glasgow Post Office I am able to boast that by the introduction of the electric light I enabled many of the more aged sorters who were commencing to use spectacles to do without them—and even I put back the clock of time in enabling several who had used them for some years to disclaim them. I am aware that it has been alleged by the opponents of the electric light, whether interested or otherwise, that in many cases the intensity of the light has injured eyesight. I do not think any such cases can be substantiated. Many of us who are in the habit of experimenting with powerful arc lamps have had our eyelids temporarily affected by incautious exposure at too short a distance. Again, over and over I meet with the complaint that if I stare at an arc lamp for a long time it will make my eyes ache; the obvious retort being, Why should you stare at the light? If you do the same with the sun, you will be equally inconvenienced. Before such an audience as this, which is of course familiar with the beautiful electric lighting in the Health Exhibition itself, it is useless for me to enlarge on the many conditions of the electric light as it indirectly affects health. I may only name the many additional pleasures of the eye we get from its use. Our flowers in our rooms do not fade away, and are seen in their true colours. Our pictures or all coloured objects are seen to better advantage. I may mention one thing which would not generally occur to you, that in London certainly an electric-lighted house can be cleaned properly in winter. You may smile at this, but I assure you that the advantage of being able to turn a flood of light into your drawing-rooms and dining-rooms at six o'clock on a winter's morning, so that the whole of the cleaning can be finished as thoroughly as if done by daylight, before the family comes down to breakfast, is one that must be experienced before it can be thoroughly appreciated. Again, the advantage to the health of our children is simply inestimable. No night-lights, matches left about, or gas turned down low are required. A child six years old can be trusted to press a button and so turn the light off or on; the lamps being high and out of reach are not easily broken or over-turned, and the air of the children's nursery, even if the light be kept burning the night through, remains pure throughout. Another indirect advantage due to the absence of heat is that it is comparatively easy to thoroughly ventilate and cool during the hot weather a room lighted by the electric light. The heat of gas placed high in the room causes such intense draughts when the windows are open that the discomforts and dangers of the draughts are almost worse than the discomfiture from the heat and vitiated air, whereas in an electric-lighted room there is no difficulty in opening wide all the windows, the draughts produced being so gentle as to be hardly felt.

SOCIETIES AND ACADEMIES

LONDON

Physical Society, June 28.—Prof. Guthrie, President, in the chair.—New Member, Mr. W. H. Hensley.—Lord Rayleigh made a communication on the practical use of the silver voltameter for the measurement of an electric current. On a former meeting of the Society the method was explained by the author, but on the present occasion the apparatus was exhibited. The author considers this the best method of determining the strength of current in absolute measure. One ampere deposits 4 grammes of silver in an hour; therefore a quarter to half an hour is sufficient to give 1 or 2 grammes, quantities which can be measured with accuracy. Any current from 1/10 to 4 or 5 amperes can be measured successfully in this way. With very weak currents there is a difficulty in weighing the deposits; with very dense currents the deposit is apt to be irregular. The author deprecates the use of acetate of silver, pure nitrate or pure chlorate of silver giving the best results. The cathode of his apparatus is a platinum bowl, the anode a silver sheet wrapped with clean filter paper sealed over it to keep any loose silver from dropping on the cathode. The anode is immersed in the solution of silver salt; and at the end of several hours (if great accuracy is required) a measurement of the weight of silver deposited is made by weighing the bowl cathode in a chemical balance. Dr. Fleming inquired whether it was not better to weigh the loss of weight suffered by the anode, as is sometimes done. Lord Rayleigh had not found this plan so good, the anode being apt to disintegrate and lose weight, not by true electrolytic action. Prof. Guthrie remarked that with small electrodes peroxide of silver is formed, and that the inferiority of acetate of silver might be due to formation of subacetate.—Lord Rayleigh then made a communication on a colour-mixing apparatus founded on refraction. This apparatus had been described at a former meeting of the British Association, and consists of a double-refracting prism, a lens, dispersing prism, and screen, by which an overlapping of spectra can be obtained, and thus a mixture of colours. In comparing different eyesights with it Lord Rayleigh finds that the majority of persons are more sensitive to red than he himself. In answer to Mr. W. Baily he had not observed any difference between the two eyes of the same person, except what might be due to fatigue and freshness. Dr. Guthrie inquired if the author had discovered any racial characteristics of colour-blindness. Lord Rayleigh had not observed any so far. Dr. Guthrie stated that, though colour-blind to red, he believed he was more than usually sensitive to blue. Dr. Stone and Mr. Stanley referred to known cases of blindness to green, as well as red. Dr. Lodge asked if persons abnormally sensitive to red could see further down the spectrum. Lord Rayleigh believed they could see the spectrum brighter near its limits at all events. Mr. Glazebrook briefly described a modification of Lord Rayleigh's apparatus by which the distance on the spectrum which any one can see could be measured.—Mr. C. V. Boys read a paper on a phenomenon of electromagnetic induction. Between the poles of an electro-magnet a small disk of copper is hung by a bifilar suspension. If the magnetic field is uniform, and the disk at an angle to the lines of force, then on making the magnet it is jerked parallel with the lines of force. If it is a changing field, and the disk perpendicular to the lines of force, it is repelled on making the magnet and attracted on breaking by the nearest pole. This phenomenon, which was observed by Faraday, was shown by Mr. Boys to be useful for determing the intensity of a magnetic field by measuring the throw of the disk on magnetising and demagnetising. It might also be employed to measure the resistance of bodies in the form of plates, from their diameter, moment of inertia, and observed throw. Any structural difference of resistance in different directions in the body might be determined by its means. Mr. Boys illustrated his remarks with curves of results obtained by experiment. Lord Rayleigh considered that the effect of self-induction on the results was not likely to be serious.—Mr. J. Hopps read a paper on the alteration of electrical resistance in metal wires produced by coiling and uncoiling. His experiments were made with an inclined plane, the angle of which could be varied, and a car, carrying bobbins, which was drawn up or let down the plane by the wires experimented on. It appeared that coiling and uncoiling tends to produce hardness in a wire. Coiling produces an increase of resistance, and uncoiling a decrease in the resistance of a wire.—Mr. R. T. Glazebrook, M.A., F.R.S., read a paper on the determination in absolute measure of the electrical capacity of a condenser, and on a method of finding by electrical observations the period of a tuning-fork. The paper described experiments conducted according to a method given in "Maxwell," vol. ii. § 776, for measuring the capacity of a condenser. Mr. J. J. Thomson showed (*Philosophical Transactions*, 1883, part iii.) that Maxwell's formula is only approximate, and gave the correct formula, which was used in the author's experiments. In these tuning-forks were used of frequencies approximately 16, 32, 64, and 128 to a second, the frequencies being determined by careful comparison with the clock by Lord Rayleigh's method, and the corresponding values found for the capacity were ·3336 m.f., ·3340 m.f., ·3335 m.f., ·3337 m.f. The mean is ·3337 m.f., and the experiments do not show any variation in the capacity, as the time of changing varies from 1/16 to 1/128 of a second. The condenser was furnished by Messrs. Latimer Clark, Muirhead, and Co. The method also gives a ready and accurate means of determining the pitch of a tuning-fork, for if the capacity of the condenser used is known the frequency (n) can be determined. The author has used the method successfully for this purpose. Lord Rayleigh objected to mercury contacts in such experiments; and Dr. Stone said he had found iron and mercury contacts good.—Prof. Herbert Macleod exhibited a sunshine recorder made by placing a water lens in front of a camera box and lens. Sensitised paper is placed in the bottom of the box so that the focused ray strikes on it, and as the sun moves traces a curved line or band on the paper. Several of these records were shown to the meeting.

Royal Microscopical Society, June 11.—Rev. W. H. Dallinger, F.R.S., President, in the chair.—Prof. Zenger's method of constructing endomersion objectives by using a mixture of ethereal and fatty oils for correcting chromatic aberration was explained and an objective exhibited.—Dr. Wallich exhibited a new condenser which he had devised.—Mr. J. Mayall, jun., exhibited and described his method of applying amplifiers to a microscope, by which a considerable range of magnifying power and working distance was obtained.—Mr. C. Beck exhibited and described a new form of microscope lamp for use in various pathological and physiological investigations.—Notes were read on human spermatozoa with two tails (Mr. Hazlewood), on the potato-blight insect (Mr. Brennan), and on a *Spirochæte* of unusual form (Mr. Cheshire).—Dr. Anthony read a paper on drawing prisms, on which a long and interesting discussion took place.—Mr. Dowdeswell read a paper on some appearances in the blood of vertebrate animals with reference to the occurrence of bacteria therein; Mr. Oxley, on *Protospongia pedicellata*, a new compound infusorian; and Mr. C. D. Ahrens on some new polarising prisms which he had devised.

PARIS

Academy of Sciences, July 7.—M. Rolland, President, in the chair.—Remarks in connection with a note of M. Berthot on the mutual attraction and repulsion of the molecules of bodies, by M. de Saint-Venant.—Note on the absorption of chlorine by carbon, and on its combination with hydrogen, by MM. Berthelot and Guntz.—Remarks on the projected inland sea in North Africa, by M. de Lesseps. The author, who supports the scheme, replies to the objections raised by M. Cosson, and denies that its execution would involve the ruin of the Belad-al-Jerid and Sûf districts.—On the cholera epidemic, by M. E. Cosson. While admitting that the present epidemic in the south of France is of the Asiatic type, the author points out that it is of a much less virulent character than previous visitations. The germs of the malady seem to lose their intensity and power of transmission in proportion to the distance of the places whence they have been imported. They may thus be compared to the attenuated virus artificially cultivated by M. Pasteur. The efficacy of military cordons and measures of isolation and disinfection is insisted upon, and illustrated by reference to the results obtained by these precautions during the prevalence of cholera in Algeria in the year 1867.—On the so-called algebraic monothetic equations, in which all the coefficients are functions of a single matrix m, by Prof. Sylvester.—Memoir on the chemical composition and alimentary value of the various constituents of wheat, by M. Aimé. Contrary to the generally received opinion, the author concludes, from experiments made on himself, that whole meal or household bread, containing all the ingredients of the grain, is less wholesome and more indigestible than pure white bread made of the flour alone.—New researches on the structure of the brain and on the functions of the white fibres of the cerebral substance, by M. J. Luys.—On the developments bearing on the distance of two points, and on some properties of the spherical functions, by M. O. Callandreau.—Note on the holomorphous functions of any genus (mathematical analysis), by M. E. Cesaro.—On the determination of longitudes in the region of the Caucasus, letter addressed to M. Faye by General Stebnitski.—Note on the electric conductivity of highly diluted aqueous solutions of organic substances, such as ethylic alcohol, glycerine, phenol, glucose, urea, acetone, albumen, ordinary ether, and ethylic aldehyde, by M. E. Bouty. Researches on anhydrous phosphoric acid, by MM. P. Hautefeuille and A. Perrey.—On some new borotungstates, by M. D. Klein.—On the dishydrating action of salts, by M. D. Tommasi. According to the researches of M. Grimaux, salts would appear to favour the coagulation of colloidal substances by acting as dishydratants. But the author shows that in some cases certain salts produce the opposite result.—On perseite, a saccharine substance extracted from the berry of *Laurus persea*, and analogous to mannite, by MM. A. Muntz and V. Marcano.—On the bibromide of metaxylene, metaxylenic glycol, and other derivatives of metaxylene, by M. A. Colson.—Polarimetric researches on the regenerated cellulose of pyroxylenes and on the cellulose subjected to the action of sulphuric acid, by M. A. Levallois.—Experiments on the artificial fabrication of farmyard manure, by M. P. P. Dehérain.—Contribution to the comparative anatomy of the races of mankind: dissection of a Bosjesman, by M. L. Testut. These studies, made on a subject from twelve to fourteen years old, have revealed a muscular system in a more or less rudimentary state, which exists in a normal condition in various anthropoid and other apes, and in some instances even in mammals of other orders. In his remarks on the paper M. de Quatrefages points out that it supplies no fresh argument in favour of man's descent from a simian prototype.—On the submaxillary of *Oligotoma saundersii*, *Œdipoda cinerascens*, *Gryllus domesticus*, and some other members of the locust family, by M. J. Chatin.—Researches on the transpiration of vegetables under the tropics, by M. V. Marcano.—On a new genus of fossil grain (*Gnetopsis elliptica*, *G. trigona*, and *G. hexagona*) from the Upper Carboniferous Measures, by MM. B. Renault and R. Zeiller.

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