THURSDAY, AUGUST 17, 1893.

OLD AND NEW ASTRONOMY.

Old and New Astronomy. By Richard A. Proctor, completed by A. Cowper Ranyard. (London: Longmans, Green and Co., 1892.)

AS originally designed by Mr. Proctor, this work was to contain a complete account of the Old and New Astronomy, particular attention being paid to the latter portion, which he wished to make a special feature of the book. It was to be issued in twelve monthly parts, the first of which duly appeared in March 1888; but unfortunately Mr. Proctor did not live to complete what he intended to be his *magnum opus*, and at the time of his death in September 1888 only seven parts were in type and the manuscript of the chapters on the planets well advanced. Although a considerable proportion of the materials for the chapters on the new astronomy had been collected, nothing had been written for that portion of the book. Mr. Ranyard undertook to complete the work, and as we now have it five-sixths of the book deals with the ancient and old astronomy, and is due to Mr. Proctor; while the remaining portion by Mr. Ranyard deals with some of the work and problems of the new astronomy. Unfortunately the book had grown to such an enormous size that promised chapters on meteors and comets were omitted and we have the strange anomaly of a work on astronomy in which neither of these important phenomena are dealt with.

It is to be regretted that Mr. Ranyard, in finishing the book, did not attempt to draw up a list of the errata which "must inevitably occur in a work of this kind," and that he did not see fit to add in some places details of important points, the original omission of which was probably accidental. For instance, in the first chapter, which gives an account of ancient and modern methods of observing the heavenly bodies, we find it indicated that the reflecting telescope suffers from chromatic aberration (page 45); and although stress is laid on the "amazingly exact system of modern measurement" the two essential instruments, the filar micrometer and the chronograph, are not even mentioned. Later on, in dealing with spectroscopy, the diffraction grating is not dealt with, and there is no mention of the grating spectroscope. We venture to think that no work on astronomy can be considered complete in which such essentials to the proper comprehension of the subject are omitted.

In the chapters on the shape of the earth, the apparent motions of the sun, moon, and planets, the true mechanism of the solar system, and the measuring and weighing of the solar system, we find Mr. Proctor probably at his best, the subjects being treated in considerable detail, although there is very little that has not already appeared in his earlier works.

Two chapters are devoted to the sun and its surroundings, and here Mr. Proctor differs from most of the authorities on the subject. He assumes that the formation of a spot is usually preceded by the formation of a facula, although the subject is still under discussion and the weight of evidence at present is distinctly in favour of the opposite conclusion. Now that Prof. Hale has enabled us to supplement our photographic study of sun-spots by photographs of all the faculæ on the solar disc, we may shortly hope to be able to fully trace the life history of solar disturbances in particular regions of the sun, and so to obtain a firm basis for a definite conclusion in the matter. We are also told in this chapter that we are forced to the conclusion that sun-spots are produced in the main by uprushes of intensely heated vapours from below the photosphere, but the generally accepted view is that spots are due to downrushes of comparatively cool matter from the regions above the photosphere. The main objection to the view that sun-spots are due to uprushes of intensely heated matter is that the bright lines in sun-spot spectra are few, and are not those usually associated with extremely high temperature.

The interesting chapter on the sun's surroundings is marred by personalities which render it practically impossible to consider this portion of the book as dispassionate scientific work. We may mention, however one error of fact which Mr. Ranyard should have corrected. On page 408 we are told that the eclipse of 1860 is remarkable as the first in which photography was employed to secure views of the corona, whereas Majocchi, at Milan, in 1842, had unsuccessfully tried this method of observation; and Berkowski, July 28, 1851, had obtained a perfectly successful picture showing the prominences and corona.

The chapters on the planets are, as might have been expected, very full and complete, but contain little that is new or calls for special comment. The method of illustrating the seasons on the earth by a series of diagrams, showing our planet as seen from the sun at 6 a.m., midday, 6 p.m., and midnight at Greenwich, on one day in each month, may, however, be noticed as certainly an advance on the very unsatisfactory method usually found in works on astronomy. It is also interesting to note that the moon is properly considered as a planet.

In the discussion of the temperature of the lunar surface, we are told that merely theoretical considerations could be thoroughly relied upon as proving that the temperature during the lunar day exceeds that of boiling water; and Lord Rosse's measurements which indicate a temperature of fully 500° Fahrenheit, are accepted, while those of Prof. Langley, which assign a temperature below freezing point, are rejected as being affected by some unknown cause of error. Later researches by Mr. Boys have, however, confirmed Prof. Langley's results, and it would have been an advantage had Mr. Ranyard noted this in the completed volume.

In the chapters on stars and the new astronomy Mr. Ranyard gives in the beginning a full account of parallax, and presents an interesting diagram showing the distances of all stars whose parallaxes have been determined during the present century. The theories of the earlier astronomers with regard to the construction of the stellar universe are passed in rapid review, due credit being awarded to the work of Thomas Wright, of Durham, who really anticipated many of the speculations of Sir William Herschel. The various later disc, ring, and spiral theories of the Milky Way are carefully discussed and compared with the latest researches of Prof. E. C. Pickering and Dr. Gould, the whole object of the work being obviously to give a full and fair statement of fact,

without regard to any preconceived ideas or theories. The distribution of nebulæ is then considered, and a careful analysis made of the many wonderful structures shown in Dr. Robert's photograph of the great nebula in Orion. The great similarity of these forms to those traceable in the Solar Corona is clearly demonstrated, and it is suggested that just as the coronal forms probably have their origin in enormous streams of gaseous matter ejected into a resisting medium, so these similar structures may be due to a similar cause. Mr. Ranyard then shows how these forms are reproduced in the arrangement of clouds of stars in the Milky Way, as shown by the marvellous photographs taken by Prof. E. E. Barnard of the Lick Observatory, excellent reproductions of which illustrate this portion of the book. A detailed examination of Prof. Barnard's plates seems to Mr. Ranyard to indicate the existence of dark-absorbing matter, "either like cold gas or fog of opaque particles in space, cutting out or dimming down the light of the region beyond." These dark patches assume the curved forms and tree-like structure already referred to, and thus seem to further confirm the idea of a resisting medium, which, as Mr. Ranyard is careful to point out, need not necessarily be a gas; dust moving in space, or meteors, or large masses would equally offer resistance.

By deliberately overprinting photographs of the Milky Way, long chains of stars and curving dark lanes have been brought into great prominence, and have materially assisted in the investigation. There is certainly much to recommend this startling suggestion of dark absorbing matter in space, and the wonderful details of Prof. Barnard's photographs, and the similarity to coronal and nebular forms, can scarcely be explained as due to accidental groupings of stars and dark spaces in the Milky Way. Although most authorities, including Prof. Barnard himself, prefer to suspend their judgment in the matter until still more photographic results are available, there can be little doubt that no more satisfactory hypothesis has as yet been advanced.

The connection between nebulæ and bright stars, and the connection of bright stars with faint ones by means of thin wisps of nebulous matter, undoubtedly indicate that differences in magnitude of stars are due to differences of physical condition and not to distance. As illustrating the far reaching results of this conclusion Mr. Ranyard says:—

"If we assume a distance fifteen times as great as the distance of α Centauri, for a part of the Milky Way in which a first magnitude star is found to be associated with stars of the 17½ magnitude, we must be prepared to assume a diameter for the large star twenty times as great as the solar diameter, unless its photosphere is brighter than the solar photosphere; while the smaller stars if their photospheres were as brilliant as the solar photosphere, would have diameters equal to about one-hundredth part of the solar diameter—that is, they would not much exceed the earth in magnitude."

The question of proper motion next receives attention, and this is followed by an account of binary and triple stars, most of the recent work being fully dealt with. In the discussion of stellar spectroscopy we find Secchi's classification of star spectra given to the exclusion of all others. Details of other systems might well have been introduced here, but it is evident that Mr. Ranyard considers the subject one in which very little advance towards a proper classification has been made. He is of opinion (p. 795) that the Sirian type are less condensed and are in an earlier stage than the solar type, and indicates that bright line stars must come somewhere between nebulæ and the Sirian type; but there is nothing to indicate whether he thinks this is the stage of rising or falling temperature, and the whole question is left in a vague and somewhat unsatisfactory manner.

In considering the supposed physical connection between the stars in the great nebula in Orion and the nebula itself, Mr. Ranyard relies on the fact that similar bright lines are found in the spectra of each, and quotes the photographs of Dr. Huggins as proving that these stars are physically bound up with the gaseous matter of the nebula. He himself seems inclined to the opinion that they are not condensations of the nebulæ, but are the centres from which the matter now forming the nebula was ejected; but whether condensations of the nebula or points of origin the spectra are supposed to be similar. It is important to remember, however, that it is nearly impossible to get a photograph of the spectrum of a star involved in a nebula without also obtaining a superposed spectrum of the nebula itself. Every tremor of the telescope sufficient to carry the star image off the slit will allow the nebula to imprint its spectrum on the plate, and if the star is allowed to trail along the slit it is clear that the nebula gets more exposure than the star at any particular point in the resulting photograph. There is at present no absolute photographic proof that the stars in the nebula in Orion contain the nebular lines as bright lines in their spectra, and consequently conclusions based on this assumption are untrustworthy.

Mr. Ranyard classes the nebulæ which give faint continuous spectra and do not show the characteristic green line, as white nebulæ, and places in this class the Andromeda nebula, the spiral in Canes Venatici, and the nebulous background of the Milky Way, but makes no suggestion as to the stage of development of these bodies.

On the question of "What is a nebula?" it is extremely difficult to understand the exact position assumed by Mr. Ranyard. He evidently considers nebulæ as containing solid or liquid matter and as increasing in temperature, but "the very great transparency" renders it probable that they either contain very little solid or liquid matter, or that the solid or liquid matter is aggregated into discrete masses with an average diameter of more than an inch; if the density of a nebula, leaving out of account its gaseous constituents, is as much as one one-thousand millionth of the density of atmospheric air at the sea level. These conclusions are practically an acceptance of the main idea of Lockyer's meteoritic hypothesis so far as it deals with nebulæ, although Mr. Ranyard rejects the spectroscopic evidence bearing on the point. The speculations as to the probable density of the Great Orion nebula, although extremely interesting, are vitiated by the fact that it is impossible to estimate the gravitational effect of the dark matter in interstellar space.

The book is well and copiously illustrated throughout, the plates and the photographic reproductions being of a very high-class character. Mr. Proctor's portion is

written in his usual clear and popular manner, but the prevailing impression is decidedly that of disproportion. Too much space is occupied by the personalities which were unfortunately too frequently shown in his controversial methods, and by details of a comparatively unimportant character; while essentials are, as we have pointed out, frequently incompletely dealt with or even entirely omitted.

Mr. Ranyard's portion is admirably written, is very thoughtful and suggestive, and is a valuable contribution to our knowledge of the stellar universe and the condition and distribution of matter in external space. Indeed, the comparative brevity of this portion of the book is its chief fault, and a condensation of the earlier portion to allow of the expansion of this would greatly increase its value to the student, and would certainly not lessen its interest to the general reader. A. T.

EARTHQUAKES.

Erdbebenkunde. Die Erscheinungen und Ursachen der Erdbeben, die Methoden ihrer Beobachtungen. Von Dr. Rudolf Hoernes, o.ö. Professor der Geologie und Palæontologie an der Universität Graz. (Leipzig: Veit and Co., 1893.)

Étude sur les Tremblements de Terre. Par Léon Vinot. (Paris and Nancy: Berger-Levrault et Cie, 1893.)

IN a recent article in this journal, entitled "Seismology in Japan" (see NATURE, June 8, p. 136) attention was directed to the long series of memoirs which deal with the methods and results of earthquake-observation, and have appeared in the Transactions of the Seismological Society of Japan, or its successor, the *Seismological Journal of Japan*. Any advance in our knowledge of the phenomena or causes of earthquakes resulting from the study of the frequently occurring shocks in Japan is largely due to the untiring efforts of the editor of those journals, Prof. John Milne, and to those of the school of active seismologists, whom he has educated and inspired with some of his own enthusiasm. We can best judge, perhaps, how far advances of our knowledge on this difficult and obscure department of physics and geology have been real and of permanent value from the examination of text-books and general treatises, in which summaries are given of the latest and most important researches upon the subject.

The two works whose titles appear at the head of this article, and which have recently made their appearance in Germany and France respectively, may well serve the purpose of illustrating what is the high-water mark of our knowledge at the present time concerning these remarkable but little understood phenomena.

If we compare these two books with their numerous predecessors, the first peculiarity which strikes us is the classification of earthquake-phenomena, based on the supposed causes of the disturbance of equilibrium in the earth's solid crust, which have been adopted by the recent authors. While the older writers took for granted the close connexion between seismological and vulcanological phenomena, so that "earthquakes and volcanoes" were almost always discussed in the same treatise, the two works before us afford distinct evidence that this conviction has now been very seriously shaken. It is true that nearly all great volcanic outbursts have been attended by earth-tremblings; but it is equally true that some of the grandest displays of seismic energy have occurred in areas that have not at any recent period been the scenes of volcanic activity; and both the German and the French author admit the existence of great classes of seismic disturbances, which have no necessary connexion with any manifestations of volcanic energy.

Dr. Hoernes classifies earthquakes under the four headings: "Vulkanische Beben," "Einsturzbeben," "Dislocationsbeben," and "Relaisbeben." M. Vinot treats of them under the following heads:—"Tremblements de terre suivis, d'éruptions où liés directement à l'action volcanique;" "Tremblements de terre dus encore à l'action direct du feu central, mais sans manifestation consécutive du volcanisme;" and, lastly, "Tremblements de terre indépendent de l'action volcanique."

But with this recognition of the class of non-volcanic earthquakes the resemblance between these two books ceases. Dr. Hoernes commences his work with an admirable account of the speculations on the nature and causes of earthquake-phenomena which have appeared from the earliest times. His comprehensive sketch begins with extracts from the writings of Hebrew prophets and Greek philosophers, and ends with references to the Seismological Society of Japan. The two chapters which follow on earthquake-phenomena and earthquake-observation are clear and useful summaries of the most recent researches on the subject, and are well brought up to date. Supplied as they are with drawings and descriptions of seismographic apparatus, they afford one of the best guides with which we are acquainted to a general knowledge of the principles and methods of seismological investigation.

M. Vinot commences his work with a chapter on the nebular hypothesis and the proofs of the existence of central heat within the earth. He insists that, to explain the phenomena of earthquakes, it is necessary to assume the existence at a depth which certainly does not exceed "quelques centaines des kilomètres," of a mass of incandescent liquid materials, which he argues must consist of molten metals in which are dissolved certain gases. The subsequent chapters of his book are a series of deductions from these premises. It will thus be seen that the methods and plan of the German and French authors are about as diverse as can well be conceived. The German work abounds with references by means of which the student who is not satisfied with the summary statements in the text is enabled to put himself into communication with the memoirs of the original investigator whose views have been cited. The French work is simply a readable essay, in which we have none of these valuable aids to study. The illustrations of M. Vinot's book consist of several page plates, reproduced from photographs, and representing the now-destroyed terraces of Rotomahana in New Zealand and the country affected by the eruption of Tarawera but the connexion of these illustrations with the text is by no means obvious.

In two works so diverse in their plan and execution as are those before us, it is interesting to note yet another and somewhat unexpected feature which they present in

common. That M. Vinot should commence his book with references to the Deluge, the destruction of Sodom and Gomorrah, and the giving of the law on Sinai, seems perfectly natural. But most readers will note with some surprise that the last chapter of Dr. Hoernes's book is one entitled "Die Sintfluth." We cannot but regard it as a remarkable testimony to the profound influence of that striking and suggestive book of Dr. Suess, "Das Antlitz der Erde," that this chapter should have been added by Dr. Hoernes to his systematic treatise on Earthquakes. It is scarcely necessary to point out that the flood to which the Austrian geologist devotes the final chapter of his treatise is the deluge, not of Sir Henry Howorth, but of Noah and Hasîs-Adra, and that the connexion between this final chapter and the preceding ones is of the very slenderest character. But the legends of our own childhood and of the childhood of our race have a fascination for us, which neither the brilliant French essayist nor the painstaking German professor seem to have been able to resist.

OUR BOOK SHELF.

The Points of the Horse. By M. Horace Hayes, F.R.C.V.S. (London: W. Thacker and Co., 1893.)

IT is certainly curious that although the English nation justly prides itself on its knowledge of horse flesh, and its success in producing the various equine breeds, it should possess no work dealing in an exact and scientific manner with the conformation of the animal that it has done so much to improve. That certain shapes are indicative of great speed, whilst others point to strength rather than speed, has, of course, always been insisted upon in a general way, but it has been left to Captain Hayes to imitate the example of several French authors, and deal with the subject in a scientific spirit. A soldier, a certificated veterinarian, a traveller, and a successful rider, the author is well qualified to treat of all that pertains to the subject before us. The work represents a painstaking endeavour to discover and explain the various principles which govern the make and shape of the horse.

Starting with a study of animals like the Indian black buck and cheetah, which possess terrific speed, he compares them with others such as the buffalo and rhinoceros, which are examples of great strength, a comparison which leads to the conclusion that animals of great strength are distinguished by a long body and short legs; those of great speed by a short body and long legs. This is an exemplification of Marey's law that muscles of speed are long and slender, and those of strength short and thick. Whether it was necessary to stray so far from home to find examples of this fact may be doubted. The thoroughbred racehorse on the one hand, and the massive carthorse on the other, are surely sufficiently contrasted types of speed and strength, whilst between the two extremes are numerous examples exhibiting the union of these two attributes in various degrees, the hunter, for example, uniting considerable strength with moderate speed.

The defects as well as many of the beauties of conformation are admirably depicted in a series of photographs, such defects as turned-in and turned-out toes, sickle-shaped hocks, and upright pasterns, being particularly good. The photographic plates, of which there are over seventy, certainly constitute an important feature in the work, embracing, in addition to the above, portraits of many celebrated racers, notably "Ormonde" and "St. Simon," as well as horses and ponies of various breeds found in different parts of the globe. A chapter is devoted to an examination of these photographs, the leading features and points of the animals represented being analysed and commented upon. It would be unfair in this connection to omit favourable mention of the 200 excellent drawings by the late J. H. Oswald Brown, which serve throughout the work to illustrate the letterpress.

Author, artist, and publisher have successfully united in producing a first-rate work, which may be cordially recommended to all lovers—and their name is legion—of the horse. W. F. G.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Quaternions and Vector Analysis.

IN a paper by Prof. C. G. Knott on "Recent Innovations in Vector Theory," of which an abstract has been given in NATURE (vol. xlvii. pp. 590–593; see also a minor abstract on p. 287), the doctrine that the quaternion affords the only sufficient and proper basis for vector analysis is maintained by arguments based so largely on the faults and deficiencies which the author has found in my pamphlet, "Elements of Vector Analysis," as to give to such faults an importance which they would not otherwise possess, and to make some reply from me necessary, if I would not discredit the cause of non-quaternionic vector analysis. Especially is this true in view of the warm commendation and endorsement of the paper, by Prof. Tait, which appeared in NATURE somewhat earlier (p. 225).

The charge which most requires a reply is expressed most distinctly in the minor abstract, viz. "that in the development of his dyadic notation, Prof. Gibbs, being forced to bring the quaternion in, logically condemned his own position." This was incomprehensible to me until I received the original paper, where I found the charge specified as follows: "Although Gibbs gets over a good deal of ground without the explicit recognition of the complete product, which is the difference of his 'skew' and 'direct' products, yet even he recognises in plain language the versorial character of a vector, brings in the quaternion whose vector is the difference of a linear vector function and its conjugate, and does not hesitate to use the accursed thing itself in certain line, surface, and volume integrals" (Proc. R.S.E., Session 1892-3, p. 236). These three specifications I shall consider in their inverse order, premising, however, that the *epitheta ornantia* are entirely my critic's.

The last charge is due entirely to an inadvertence. The integrals referred to are those given at the close of the major abstract in NATURE (p. 593). My critic, in his original paper, states quite correctly that, according to my definitions and notations, they should represent dyadics. He multiplies them into a vector, introducing the vector under the integral sign, as is perfectly proper, provided, of course, that the vector is constant. But failing to observe this restriction, evidently through inadvertence, and finding that the resulting equations (thus interpreted) would not be true, he concludes that I must have meant something else by the original equations. Now, these equations will hold if interpreted in the quaternionic sense, as is, indeed, a necessary consequence of their holding in the dyadic sense, although the converse would not be true. My critic was thus led, in consequence of the inadvertence mentioned, to suppose that I had departed from my ordinary usage and my express definitions, and had intended the products in these integrals to be taken in the quaternionic sense. This is the sole ground for the last charge.

The second charge evidently relates to the notations Φ_s and Φ_x (see NATURE, vol. xlvii. p. 592). It is perfectly true that I have used a scalar and a vector connected with the linear vector operator, which, if combined, would form a quaternion. I have not thus combined them. Perhaps Prof. Knott will say that since I use both of them it matters little whether I combine them or not. If so I heartily agree with him.

The first charge is a little vague. I certainly admit that

vectors may be used in connection with and to represent rotations. I have no objection to calling them in such cases *versorial*. In that sense Lagrange and Poinsot, for example, used versorial vectors. But what has this to do with quaternions? Certainly Lagrange and Poinsot were not quaternionists.

The passage in the major abstract in NATURE which most distinctly charges me with the use of the quaternion is that in which a certain expression which I use is said to represent the quaternion operator $q(\ )q^{-1}$ (vol. xlvii. p. 592). It would be more accurate to say that my expression and the quaternionic expression represent the same operator. Does it follow that I have used a quaternion? Not at all. A quaternionic expression may represent a number. Does everyone who uses any expression for that number use quaternions? A quaternionic expression may represent a vector. Does everyone who uses any expression for that vector use quaternions? A quaternionic expression may represent a linear vector operator. If I use an expression for that linear vector operator do I therefore use quaternions? My critic is so anxious to prove that I use quaternions that he uses arguments which would prove that quaternions were in common use before Hamilton was born.

So much for the alleged use of the quaternion in my pamphlet. Let us now consider the faults and deficiencies which have been found therein and attributed to the want of the quaternion. The most serious criticism in this respect relates to certain integrating operators, which Prof. Tait unites with Prof. Knott in ridiculing. As definitions are wearisome, I will illustrate the use of the terms and notations which I have used by quoting a sentence addressed to the British Association a few years ago. The speaker was Lord Kelvin.

"Helmholtz first solved the problem—Given the spin in any case of liquid motion, to find the motion. His solution consists in finding the potentials of three ideal distributions of gravitational matter having densities respectively equal to $1/\pi$ of the rectangular components of the given spin; and, regarding for a moment these potentials as rectangular components of velocity in a case of liquid motion, taking the spin in this motion as the velocity in the required motion" (NATURE, vol. xxxviii. p. 569).

In the terms and notations of my pamphlet the problem and solution may be thus expressed:

Given the curl in any case of liquid motion—to find the motion.

The required velocity is $1/4\pi$ of the curl of the potential of the given curl.

Or, more briefly—The required velocity is $\frac{1}{4\pi}$ of the Laplacian of the given curl.

Or in purely analytical form—Required ω in terms of $\nabla \times \omega$, when $\nabla.\omega = 0$.

Solution—

$$\omega = 1/4\pi\nabla \times \text{Pot}\,\nabla \times \omega = 1/4\pi\,\text{Lap}\,\nabla \times \omega.$$

(The Laplacian expresses the result of an operation like that by which magnetic force is calculated from electric currents distributed in space. This corresponds to the second form in which Helmholtz expressed his result.)

To show the incredible rashness of my critics, I will remark that these equations are among those of which it is said in the original paper (Proc. R.S.E., Session 1892–93, p. 225), "Gibbs gives a good many equations—theorems I suppose they ape at being." I may add that others of the equations thus characterised are associated with names not less distinguished than that of Helmholtz. But that to which I wish especially to call attention is that the terms and notations in question express exactly the notions which physicists want to use.

But we are told (NATURE, vol. xlvii. p. 287) that these integrating operators (Pot, Lap) are best expressed as inverse functions of ∇. To see how utterly inadequate the Nabla would have been to express the idea, we have only to imagine the exclamation points which the members of the British Association would have looked at each other if the distinguished speaker had said:

Helmholtz first solved the problem—Given the Nabla of the velocity in any case of liquid motion, to find the velocity. His solution was that the velocity was the Nabla of the inverse square of Nabla of the Nabla of the velocity. Or, that the velocity was the inverse Nabla of the Nabla of the velocity.

Or, if the problem and solution had been written thus: Required ω in terms of $\nabla\omega$ when $S\nabla\omega = 0$.

Solution: $\omega = \nabla\nabla^{-2}\nabla\omega = \nabla^{-1}\nabla\omega.$

My critic has himself given more than one example of unfitness of the inverse Nabla for the exact expression of thought. For example, when he says that I have taken "eight distinct steps to prove two equations, which are special cases of

$$\nabla^{-2}\nabla^2 u = u,"$$

I do not quite know what he means. If he means that I have taken eight steps to prove Poisson's Equation (which certainly is not expressed by the equation cited, although it may perhaps be associated with it in some minds), I will only say that my proof is not very long, especially as I have aimed at greater rigour than is usually thought necessary. I cannot, however, compare my demonstration with that of quaternionic writers, as I have not been able (doubtless on account of insufficient search) to find any such.

To show how little foundation there is for the charge that the deficiencies of my system require to be pieced out by these integral operators, I need only say that if I wished to economise operators I might give up New, Lap, and Max, writing for them ∇ Pot, $\nabla \times$ Pot, and $\nabla.$ Pot, and if I wished further to economise in what costs so little, I could give up the potential also by using the notation $(\nabla.\nabla)^{-1}$ or ∇^{-2}. That is, I could have used this notation without greater sacrifice of precision than quaternionic writers seem to be willing to make. I much prefer, however, to avoid these inverse operators as essentially indefinite.

Nevertheless—although my critic has greatly obscured the subject by ridiculing operators, which I beg leave to maintain are not worthy of ridicule, and by thoughtlessly asserting that it was necessary for me to use them, whereas they are only necessary for me in the sense in which something of the kind is necessary for the quaternionist also, if he would use a notation irreproachable on the score of exactness—I desire to be perfectly candid. I do not wish to deny that the relations connected with these notations appear a little more simple in the quaternionic form. I had, indeed, this subject principally in mind when I said two years ago in NATURE (vol. xliii. p. 512): "There are a few formulæ in which there is a trifling gain in compactness in the use of the quaternion." Let us see exactly how much this advantage amounts to.

There is nothing which the most rigid quaternionist need object to in the notation for the potential, or indeed for the Newtonian. These represent respectively the operations by which the potential or the force of gravitation is calculated from the density of matter. A quaternionist would, however, apply the operator *New* not only to a scalar, as I have done, but to a vector also. The vector part of New ω (construed in the quaternionic sense) would be exactly what I have represented by Lap ω, and the scalar part, taken negatively, would be exactly what I have represented by Max ω. The quaternionist has here a slight economy in notations, which is of less importance, since all the operators—New, Lap, Max—may be expressed without ambiguity in terms of the potential, which is therefore the only one necessary for the exact expression of thought.

But what are the formulæ which it is necessary for one to remember who uses my notations? Evidently only those which contain the operator *Pot*. For all the others are derived from these by the simple substitutions

$$\begin{aligned} \text{New} &= \nabla\,\text{Pot}, \\ \text{Lap} &= \nabla\times\,\text{Pot}, \\ \text{Max} &= \nabla.\,\text{Pot}. \end{aligned}$$

Whether one is quaternionist or not, one must remember Poisson's Equation, which I write

$$\nabla.\nabla\,\text{Pot}\,\omega = -4\pi\omega,$$

and in quaternionic might be written

$$\nabla^2\,\text{Pot}\,\omega = 4\pi\omega.$$

If ω is a vector, in using my equations one has also to remember the general formulæ,

$$\nabla.\nabla\omega = \nabla\nabla.\omega - \nabla\times\nabla\times\omega$$

which as applied to the present case may be united with the preceding in the three-membered equation,

$$\nabla.\nabla\,\text{Pot}\,\omega = \nabla\nabla.\,\text{Pot}\,\omega - \nabla\times\nabla\times\,\text{Pot}\,\omega = -4\pi\omega.$$

This single equation is absolutely all that there is to burden the memory of the student, except that the symbols of differentiation $(\nabla, \nabla\times, \nabla.)$ may be placed indifferently before or after the symbol for the potential, and that if we choose we may substitute as above *New* for ∇ Pot, &c. Of course this gives a good many equations, which on account of the importance of

the subject (as they might almost be said to give the mathematics of the electro-magnetic field) I have written out more in detail than might seem necessary. I have also called the attention of the student to many things, which perhaps he might be left to himself to see. Prof. Knott says that the quaternionist obtains similar equations by the simplest transformations. He has failed to observe that the same is true in my *Vector Analysis*, when once I have proved Poisson's Equation. Perhaps he takes his model of brevity from Prof. Tait, who simplifies the subject, I believe, in his treatise on Quaternions, by taking this theorem for granted.

Nevertheless, since I am forced so often to disagree with Prof. Knott, I am glad to agree with him when I can. He says in his original paper (p. 226), "No finer argument in favour of the real quaternion vector analysis can be found than in the tangle and the jangle of sections 91 to 104 in the 'Elements of Vector Analysis.'" Now I am quite ready to plead guilty to the tangle. The sections mentioned, as is sufficiently evident to the reader, were written at two different times, sections 102–104 being an addition after a couple of years. The matter of these latter sections is not found in its natural place, and the result is well enough characterised as a *tangle*. It certainly does credit to the conscientious study which Prof. Knott has given to my pamphlet, that he has discovered that there is a violent dislocation of ideas just at this point. For such a fault of composition I have no sufficient excuse to offer, but I must protest against its being made the ground of any broad conclusions in regard to the fundamental importance of the quaternion.

Prof. Knott next proceeds to criticise—or, at least, to ridicule—my treatment of the linear vector function, with respect to which we read in the abstract:—"As developed in the pamphlet, the theory of the dyadic goes over much the same ground as is traversed in the last chapter of Kelland and Tait's 'Introduction to Quaternions.' With the exception of a few of those lexicon products, for which Prof. Gibbs has such an affection, there is nothing of real value added to our knowledge of the linear vector function." It would not, I think, be difficult to show some inaccuracy in my critic's characterisation of the real content of this part of my pamphlet. But as algebra is a formal science, and as the whole discussion is concerning the best form of representing certain kinds of relations, the important question would seem to be whether there is anything of *formal* value in my treatment of the linear vector function.

Now, Prof. Knott distinctly characterises in half a dozen words the difference in the spirit and method of my treatment of this subject from that which is traditional among quaternionists, when he says of what I have called dyadics—"these are not quantities, but operators" (NATURE, vol. xlvii. p. 592) I do not think that I applied the word quantity to the dyadics, but Prof. Knott recognised that I treated them as quantities—not, of course, as the quantities of arithmetic, or of ordinary algebra, but as quantities in the broader sense, in which, for example, quaternions are called quantities. The fact that they may be operators does not prevent this. Just as in grammar verbs may be taken as substantives, viz. in the infinitive mood, so in algebra operators—especially such as are capable of quantitative variation—may be regarded as quantities when they are made the subject of algebraic comparison or operation. Now I would not say that it is necessary to treat every kind of operator as quantity, but I certainly think that one so important as the linear vector operator, and one which lends itself so well to such broader treatment, is worthy of it. Of course, when vectors are treated by the methods of ordinary algebra, linear vector operators will naturally be treated by the same methods, but in an algebra formed for the sake of expressing the relations between vectors, and in which vectors are treated as multiple quantities, it would seem an incongruity not to apply the methods of multiple algebra also to the linear vector operator.

The dyadic is practically the linear vector operator regarded as quantity. More exactly it is the multiple quantity of the ninth order which affords various operators according to the way in which it is applied. I will not venture to say what ought to be included in a treatise on quaternions, in which, of course, a good many subjects would have claims prior to the linear vector operator; but for the purposes of my pamphlet, in which the linear vector operator is one of the most important topics, I cannot but regard a treatment like that in Hamilton's "Lectures," or "Elements," as wholly inadequate *on the formal side*. To show what I mean, I have only to compare Hamilton's treatment of the quaternion and of the linear vector operator with respect to notations. Since quaternions have been identified with matrices, while the linear vector operator evidently belongs to that class of multiple quantities, it seems unreasonable to refuse to the one those notations which we grant to the other. Thus, if the quaternionist has e_q, $\log q$, $\sin q$, $\cos q$, why should not the vector analyst have $e\Phi$, $\log\Phi$, $\sin\Phi$, $\cos\Phi$, where Φ represents a linear vector operator? I suppose the latter are at least as useful to the physicist. I mention these notations first, because here the analogy is most evident. But there are other cases far more important, because more elementary, in which the analogy is not so near the surface, and therefore the difference in Hamilton's treatment of the two kinds of multiple quantity not so evident. We have, for example, the tensor of the quaternion, which has the important property represented by the equation $\mathrm{T}(qr) = \mathrm{T}q\mathrm{T}r$.

There is a scalar quantity related to the linear vector operator, which I have represented by the notation $|\Phi|$ and called the *determinant* of Φ. It is in fact the determinant of the matrix by which Φ may be represented, just as the square of the tensor of q (sometimes called the *norm* of q) is the determinant of the matrix by which q may be represented. It may also be defined as the product of the latent roots of Φ, just as the square of the tensor of q might be defined as the product of the latent roots of q. Again, it has the property represented by the equation

$$|\Phi\cdot\Psi| = |\Phi||\Psi|$$

which corresponds exactly with the preceding equation with both sides squared.

There is another scalar quantity connected with the quaternion and represented by the notation $\mathrm{S}q$. It has the important property expressed by the equation,

$$\mathrm{S}(qrs) = \mathrm{S}(rsq) = \mathrm{S}(sqr),$$

and so for products of any number of quaternions, in which the cyclic order remains unchanged. In the theory of the linear vector operator there is an important quantity which I have represented by the notation Φ_s, and which has the property represented by the equation

$$(\Phi\cdot\Psi\cdot\Omega)_s = (\Psi\ \Omega\cdot\Phi)_s = (\Omega\cdot\Phi\cdot\Psi)_s$$

where the number of the factors is as before immaterial. Φ_s may be defined as the sum of the latent roots of Φ, just as $2\mathrm{S}q$ may be defined as the sum of the latent roots of q.

The analogy of these notations may be further illustrated by comparing the equations

$$\mathrm{T}(e_q) = e^{\mathrm{S}q}$$

and

$$|e^{\Phi}| = e^{\Phi_s}.$$

I do not see why it is not as reasonable for the vector analyst to have notations like $|\Phi|$ and Φ_s as for the quaternionist to have the notations $\mathrm{T}q$ and $\mathrm{S}q$.

This is of course an *argumentum ad quaternionisten*. I do not pretend that it gives the reason why I used these notations, for the identification of the quaternion with a matrix was, I think, unknown to me when I wrote my pamphlet. The real justification of the notations $|\Phi|$ and Φ_s is that they express functions of the linear vector operator *quâ* quantity, which physicists and others have continually occasion to use. And this justification applies to other notations which may not have their analogues in quaternions. Thus I have used $\Phi_\times$ to express a vector so important in the theory of the linear vector operator, that it can hardly be neglected in any treatment of the subject. It is described, for example, in treatises as different as Thomson and Tait's *Natural Philosophy* and Kelland and Tait's *Quaternions*. In the former treatise the components of the vector are, of course, given in terms of the elements of the linear vector operator, which is in accordance with the method of the treatise. In the latter treatise the vector is expressed by

$$\mathrm{V}\alpha\alpha' + \mathrm{V}\beta\beta' + \mathrm{V}\gamma\gamma'.$$

As this supposes the linear vector operator to be given not by a single letter, but by several vectors, it must be regarded as entirely inadequate by any one who wishes to treat the subject in the spirit of multiple algebra, *i.e.* to use a single letter to represent the linear vector operator.

But my critic does not like the notations $|\Phi|$, Φ_s, $\Phi_\times$. His ridicule, indeed, reaches high-water mark in the paragraphs in which he mentions them. Concerning another notation, $\Phi^{\times}_{\times}\Phi$ (defined in NATURE, vol. xliii. p. 513), he exclaims, "Thus

burden after burden, in the form of new notation, is added apparently for the sole purpose of exercising the faculty of memory." He would vastly prefer, it would appear, to write with Hamilton $m\phi' - 1$, "when m represents what the unit volume becomes under the influence of the linear operator." But this notation is only apparently compact, since the m requires explanation. Moreover, if a strain were given in what Hamilton calls the standard trinomial form, to write out the formula for the operator on surfaces in that standard form by the use of the expression $m\phi' - 1$ would require, it seems to me, ten (if not fifty) times the effort of memory and of ingenuity, which would be required for the same purpose with the use of $\frac{1}{2}\Phi \times \Phi$.

I may here remark that Prof. Tait's letter of endorsement of Prof. Knott's paper affords a striking illustration of the convenience and flexibility of a notation entirely analogous to $\Phi \times \Phi$, viz. $\Phi : \Phi$. He gives the form $S\nabla\nabla_1 \, S\sigma\sigma_1$ to illustrate the advantage of quaternionic notations in point of brevity. If I understand his notation, this is what I should write $\nabla\sigma : \nabla\sigma$. (I take for granted that the suffixes indicate that ∇ applies as differential operator to σ, and ∇_1 to σ_1, σ and σ_1 being really identical in meaning, as also ∇ and ∇_1.) It will be observed that in my notation one dot unites in multiplication the two ∇'s, and the other the two σ's, and that I am able to leave each ∇ where it naturally belongs as differential operator. The quaternionist cannot do this, because the ∇ and σ cannot be left together without uniting to form a quaternion, which is not at all wanted. Moreover, I can write Φ for $\nabla\sigma$, and $\Phi : \Phi$ for $\nabla\sigma : \nabla\sigma$. The quaternionist also uses a ϕ, which is practically identical with my Φ (viz. the operator which expresses the relation between $d\sigma$ and $d\rho$), but I do not see how Prof. Knott, who I suppose dislikes $\Phi : \Phi$ as much as $\Phi \times \Phi$, would express $S\nabla\nabla_1 \, S\sigma\sigma_1$ in terms of this ϕ.

It is characteristic of Prof. Knott's view of the subject, that in translating into quaternionic from a dyadic, or operator, as he calls it, he adds in each case an operand. In many cases it would be difficult to make the translation without this. But it is often a distinct advantage to be able to give the operator without the operand. For example, in translating into quaternionic my dyadic or operator $\Phi \times \rho$, he adds an operand, and exclaims, "The old thing!" Certainly, when this expression is applied to an operand, there is no advantage (and no disadvantage) in my notation as compared with the quaternionic. But if the quaternionist wished to express what I would write in the form $(\Phi \times \rho)^{-1}$, or $|\Phi \times \rho|$, or $(\Phi \times \rho)_s$, or $(\Phi \times \rho)_\times$, he would, I think, find the operand very much in the way.

J. WILLARD GIBBS.

On Secular Variations of our Rainfall.

IN studying the rainfall of this country, it is instructive, I think, to compare a number of curves for different places, and a long series of years, all smoothed by means of five year averages. In the case of places not too far apart, one may then recognise a common type amid some diversity of detail. But it is not easy to trace such "family likeness" between *e.g.*, curves for the west of Scotland and the east of England.

The east of England curves seem to conform to the general law affirmed by Brückner for the greater part of the globe, viz. cold and wet periods alternating with warm and dry ones at intervals of about 35 years; so that, taking recent years, there was, in most places, a rainy period between 1841 and 1855, and again between 1871 and 1885, while a dry period occurred between 1856 and 1870.

In the accompanying diagram are shown two east of England curves, one for East Anglia, giving mainly the rainfall for Dickleburgh, in Norfolk, continued for about 17 years by that of Norwich (according to *British Rainfall*), the other for Boston (from the same work). These curves, it will be noted, dip down from a relative maximum in the early years, 1843 and 1847, and rise again to maxima in 1877 and 1881.

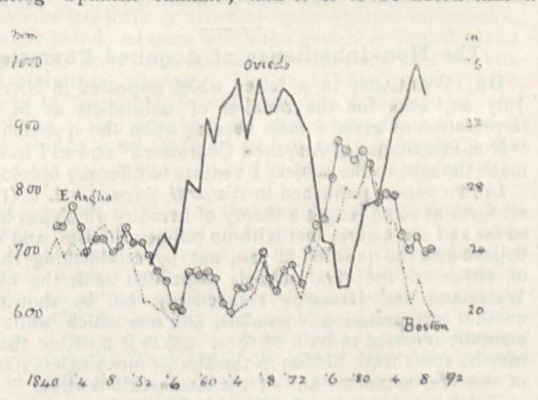
Some rainfall statistics for Oviedo were recently given in the *Meteorologische Zeitschrift* (Feb., 1892, p. 71). This is, it may be well to state, a university town in the north of Spain, capital of the province of Asturias, and about 20 miles from the coast of the Bay of Biscay. Now, the smoothed curve of this place, from 1853, has a form distinctly opposite to those just considered (as the diagram shows[1]). It rises to a maximum in 1864, goes down to a minimum in 1877, after which it rises again, reaching, perhaps, another maximum in 1887.

This oppositeness in the variation of rainfall appears to merit attention. How is it to be explained?

One of the most interesting meteorological facts brought to light in recent years is, that the depressions which come over from the west do not take, as it were, a random course, but tend to follow, with more or less frequency, certain well-defined paths. The course of several of these paths has been indicated by Van Bebber, who has made a special study of the subject. Some of the paths are known to shift in the course of the year, having a different direction in midsummer from what they have in midwinter. And there can be little doubt, though the matter is still obscure, that the paths shift in successive years. The paths numbered IV and V by Van Bebber, are said to have shifted in the years 1879 to 1884-5 from a more maritime to a more Continental position, and Lang connects with this an observed variation in the rate of travel of thunderstorms in South Germany (see *Met. Zeits.*, Nov., 1891, p. [68], of *Literaturber*). Such shifting is very probably accompanied with variations of rainfall. Hellmann supposes this to be the reason why in Spain a year that is wet in the north-west is generally dry in the south-east, and *vice versâ*. We might, perhaps, roughly compare such variations to the case of a man watering a lawn with a garden hose, and directing the jet of spray now on one side, now on the other.

I do not know whether any suggestion of this nature is applicable to the case before us, or whether some other and better explanation may be forthcoming.

Oviedo is not, apparently, included in Brückner's data for estimating Spanish rainfall; and it is to be noted that he



regards the north of Spain as conforming to his thirty-five years law, while southern Spain is reckoned exceptional.

Brückner has two classes of exceptions: the "permanent," in which the curves are opposite to the normal (Ireland and the Atlantic islands being examples), and the "temporary," in which there is conformity to the rule, for a time; then, during some lustra, there come irregular variations. To this latter class are relegated south and middle Spain, Mediterranean France, West England, and Scotland. If Brückner's view regarding the north of Spain is correct, how comes it that the Oviedo curve has the character indicated, which is apparently that of the permanent exceptions?

In discussions on the subject of sunspot influence on weather one sometimes hears the opposite character of weather in different regions urged as a difficulty in the way of accepting such influence. Thus, in connection with a paper read by Mr. Scott to the Royal United Service Institution last year (*Journal*, May, p. 510) I find him remarking: "It is not possible to say whether or not the mere fact of our having very wet or dry weather is due to the sunspots, when our neighbours not very far off are having exactly the contrary. . . . Last summer everybody was abusing the weather because of its wetness. I myself was then living in the Black Forest, and we had four days' rain in eight weeks. Which of these conditions depended on the sunspots? Was it my fine weather or was it the rain here?"

With all deference to an excellent authority, and without offering an opinion upon the particular cases cited, it seems to me not impossible that the influence of the solar cycle might be manifested in an opposite succession of effects in different

[1] The vertical scales, right and left, are not to be taken as equivalent.

regions. Suppose, *e.g.* that in some region the rainfall in a long series of years varied, not as in the cases above considered, but in a certain regular correspondence with the sunspot curve; and in another region (perhaps further south) in opposite correspondence; also that these variations were traced to the shifting of a depression path. The opposite correspondence would obviously not be a good reason for denying sunspot influence, but rather corroborative evidence of such influence. Again, it will be admitted, I think, as conceivable that we might find certain great anticyclonic systems to vary in position or extent with the sunspot variations. Suppose, then, an anticyclone which lay over a region (*a*) at the time of minimum sunspots, were moved in a given direction, say northwards, so that it came to cover a region (*b*) at the maximum of sunspots and that it returned to *a* by the next minimum. In that case a place, *e.g.*, in the south part of region *a*, would have high barometer at minimum sunspots, while a place in the north part of region *b* would have low barometer. And at the maximum of sunspots, on the other hand, the two places would again have opposite conditions of pressure (to each other and to the first). These are some out of many aspects of the matter which seem to me to render doubtful the affirmation that if the solar cycle influences weather, it cannot produce an opposite succession of effects in different (even neighbouring) regions.

To revert, for a moment, to the shifting of depression-paths, might it not, in some cases, account for certain changes observed in the relative proportion of different wind directions? Suppose *e.g.* that, by the shifting of a path a little southwards, a place which has been for some years in its southern border comes to lie in the northern border, might it not thus come to have more easterly wind and less westerly? A. B. M.

The Non-Inheritance of Acquired Characters.

DR. WALLACE, in a letter which appeared in NATURE on July 20, asks for the opinion of naturalists as to the interpretation of certain facts bearing upon the question of the "Non-inheritance of Acquired Characters," and as I have given much thought to the subject I venture to offer my opinion.

In two papers published in *Natural Science*, vol. i. (1892), I set forth at some length a theory of heredity which has hitherto, so far as I am aware, met with no public criticism, and which I believe sets the question at rest, not by establishing the views of either of the rival schools associated with the names of Weismann and Lamarck respectively, but by showing that another interpretation is possible, and one which while fundamentally opposed to both of these makes it possible that there may be some truth hidden in the almost meaningless statements of the Weismannians and of the Lamarckians alike.

Till "heredity" is defined, and till we know exactly what we mean by "inheritance of characters" (be they "acquired" or "blastogenic"), it is useless to argue as to whether characters are "inherited" or not.

Is the word "heredity" an abstract noun, the name of a quality, a sort of magnified "family-likeness," or is it not? Those who write of heredity are too prone to speak of "heredity" as if it were a force or combination of forces producing an effect; as an "inherent tendency," to resemble parents or other ancestors which it is perhaps not unfair to compare to the "inherent tendency" of a watch to tell the time or of a weathercock to point to the south-west. There are those who even speak of it as being "latent" for a time and then, owing to some unknown cause, "springing into activity" anew and giving rise to what we call "atavism." Even "atavism" is not infrequently spoken of, as if it were of the nature of a force or combination of forces, comparable to a "latent tendency," which after lying "dormant" or "latent" for a time in a weathercock, suddenly springs into new activity and causes it to point as of old to the south-west.

It appears to me that if we once grasp the idea that "heredity" is the name of a quality, a particular kind of "likeness" or "similarity," and nothing else, we shall be saved from much useless discussion of propositions which are intrinsically almost, if not quite, meaningless.

Artemia salina is the collective name given to a large number of individuals which have certain characters in common. It would hardly seem to be necessary to suggest the probability that this possession of many characters in common is due to the action of Natural Selection; that each new individual possesses the characters in question solely by virtue of the fact that Natural Selection has led to the production of individuals possessing the power to produce, under given constant conditions, eggs, which by virtue of their constitution will develop under given conditions into adults possessing the characters which natural selection has *under those conditions* rendered nearly constant.

It has been found that this same constitution does not necessarily lead to the same series of developmental changes *under other conditions*, and that in strong brine the eggs develop into animals which, though capable of living and multiplying under those conditions, differ in form from the ancestral *A. salina*. This new form has no more right to rank as a species than has a "worker" bee whose adult form differs from that of its parent merely on account of certain conditions to which it is exposed during development.

It appears to me to be absurd to ask whether the "acquired characters" of the so-called *Artemia Milhausenii* are inheritable or not. Experiment has shown that the constitution of the species *A. salina* has so little changed that it still has the power to produce eggs which under one set of conditions develop into *A. salina* and under another set of conditions into *A. Milhausenii*. The average constitution of the species has not varied: it still produces ova which will develop into either *A. salina* or *A. Milhausenii*, according to the conditions to which it is exposed. If we look upon the species as a whole, it is not too much to say that it exhibits *no acquired characters*. If bred in strong brine the individuals of many generations are alike, having been moulded by like influences, intrinsic as well as extrinsic. If the extrinsic influences change, new individuals differ from the old ones, simply because the constitution of the individuals as well as that of the species is such that under the new conditions the developmental changes occurring differ from those which would have occurred under the old conditions.

Whether this is true of all species and under all conditions consistent with life and multiplication, or is not true of some, is a matter for experiment, and can never be decided by argument. The experiment has been made by nature, and also by man in the case of *Amblystoma*, and with a result in exact conformity with the result in the case of *Artemia*.

The experiment has also been made with *white mice* in Freiburg, and it has been conclusively shown that under constant conditions the characters of successive generations are constant. One element of the environment in one series of cases was Prof. Weismann armed with tools for amputation of the the tails of the young mice, *plus* a determination to amputate those tails. So long as this remained a constant factor in the environment, so long and no longer did the taillessness of the adult mice remain a constant character of the species.

The Texan species of *Saturnia*, so long as the exclusive supply of *Juglans regia* is a constant factor in the environment, may or may not have a constant group of characters. That is a matter for experiment; but innumerable experiments, called collectively "domestication," have shown that whatever effect changes of certain details of the environment—such as food—may have, the suspension of natural selection will in the long run lead to inconstancy of all those characters which are relieved from its restraining influence.

If anything has ever been rendered certain in biology by prolonged experiment and observation, it is the fact that specific characters are maintained constant *by selection and by that alone*. Long continued selection—natural or artificial—may produce a seeming constancy of characters (*which we call "heredity"* !), but in the long run this constancy will vanish when the particular selection which has induced it has been suspended for a sufficiently long period.

The discusssion upon the "Inheritance of Acquired Characters," though it has led to many valuable results, has been throughout little more than a quibble, for in the whole discussion, so far as I am aware, the meaning of the word "inheritance" has never been defined. Most of the disputants appear to use the term as the name for the action of a force or combination of forces, which some have called "heredity"—a force or influence—either simple or complex—of which it is perfectly safe to deny the existence. There is no such *thing* as heredity—heredity is only a quality, a likeness or similarity, and nothing more. That likeness of characters is simply and solely due to the likeness of the influences which have produced the like characters, and pre-eminent amongst those influences is natural selection, though *every* factor of the environment has also had its part to play. So long as those like influences—

extrinsic and intrinsic—remain like, so long and no longer do their effects remain constant. What effects a change in those influences may produce experiment and observation, and these alone can determine.

For thousands of generations certain characters, such as blindness and winglessness, and enormous size of head and of jaws, and complete absence of all power of multiplication either direct or indirect, either sexual or asexual, have remained constant in "soldier" termites. No soldier termite has any marked resemblance to any one of its ancestors. Yet each is like every other "soldier," and that likeness is "heredity." Its characters have not been "transmitted," for no ancestor ever possessed them. Like conditions in successive generations lead to like results, one of which is the production of a fairly constant proportion of neuter "soldiers" and "workers" devoid of any power of reproduction. Those "like conditions" include a constant, or nearly constant, structure of the males and females, albeit the polymorphism extends to these also. The recurrence of those like conditions has been determined by natural selection, and is still maintained by natural selection. To apply the term "transmission" in such a case would be not more absurd than in any other case, supposing the word to be used in its literal sense, for, though the soldier in every case derives its characters from a line of ancestors extending back to remote periods, not one ancestor in which line has possessed those characters; yet their geographical distribution at the present time shows that the characters of "soldiers" are of remote antiquity. This constancy of characters in many generations appears to be identical with the phenomenon called "inheritance."

Heredity then is a likeness of effects due to likeness in the causes producing them. The likeness of causes has been produced and maintained by natural selection acting under fairly constant conditions. "Inheritance" is a name given to the operation of an influence which has no existence in nature. The sooner we cease to use the word altogether the better it will be for our science.

"Inheritance of acquired characters" is a mere chain of words correlated with a chain of loose ideas, but not correlated with any natural objective phenomenon. To assert it as a fact is as futile as to deny it. C. HERBERT HURST.

Owens College, Manchester, July 26.

Echinocyamus Pusillus.

MAY I direct your attention to a rather serious error contained in the review of Théel's paper on *Echinocyamus pusillus*, which appeared in your number for August 3?

Your reviewer states that "not the least brilliant and far-reaching" of the advances in our knowledge of Echinoderm morphology, made in the year 1891, is the discovery by Brooks and Field of the primary bilateral symmetry "of the water-vascular system" of Asterias.

Had such a discovery been really made, it would no doubt have justified the epithet applied to it by your reviewer; but, in the first place it was not made, and in the second Metschmkoff long ago pointed out such a primary bilateral symmetry in the embryos of *Amphiura squamata*.

Field's paper, containing the results of his own and Brooks's work, appeared in the *Quarterly Journal of Microscopical Science* for 1892. In it he gives an account of the development of the larva of Asterias; but in the oldest stage which he describes *there is not as yet a trace of the water-vascular system*. He describes, it is true, in larvæ of a certain size a right as well as a left madreporic pore; but as all "echinologists," it is to be hoped, know by this time, the pore is primarily related to the cœlom, and only secondarily enters into connection with the water-vascular system. Further, Field distinctly states that cases of a double pore had been observed previously by continental zoologists, but regarded by them as pathological; and the chief point in Field's paper is the very probable theory put forward by him that such cases constitute a distinct stage in the ontogeny of the animal. E. W. MACBRIDE.

Zoological Laboratory, New Museums, Cambridge, August 5.

ON referring to my draft notes, which I happen to have kept, I find in place of the words "water-vascular system," quoted by your correspondent, "pore canal system"; and I do not deny that I should have done better had I transcribed them unchanged. I fail at the same time to see that the "error" complained of is, in its place, as serious as my critic would imply. No one who attempts to do his duty by the colossus of biological literature would fail to be familiar either with the work of the authors whom he cites, or with his own recent beginning in a kindred direction. Admitting the claims of one and all, the work of Field appeared to me to put the probability at stake upon a much firmer basis than that of his predecessors, and it was for that reason that I emphasised it. If I err not, a journalistic notice is not a thing to be hampered with names and details, especially when written with the dual object of directing attention to a really admirable monograph, and of endeavouring to promote an amicable spirit of brotherhood among workers in science, such as we to-day very much need. THE WRITER OF THE NOTICE.

The Supposed Suicide of Rattlesnakes.

THE letter of Mr. Edward S. Holden on this subject is extremely interesting. It appears that he, like other individuals who have imagined that they have witnessed the suicide of scorpions, has fallen into the error (so common in the interpretation of biological as distinguished from abiological phenomena) of stating his inferences and beliefs as though they were observations. The "instance which occurred before my eyes" (to quote his words, which remind one of the old herbalist, Gerard) was simply that of a snake biting itself when imprisoned in a jar of water. That the blow was "deliberate," "intentional," and of "suicidal purpose" is pure speculation—and nothing occurred before Mr. Holden's eyes to warrant his entertaining such a notion. Had Mr. Holden been aware that the poison of the rattlesnake has little or no effect upon another rattlesnake, nor upon the individual from which the poison is furnished, he would probably have been less ready to conclude that the bite was one of suicidal purpose. He would then perhaps have inquired as to the depth to which the bite penetrated into the tissues of the snake, and how far such a superficial bite as a snake can inflict upon a part of its own body is likely (in the absence of any poisonous action) to be seriously injurious to the snake.

In this case, as in that of the scorpion confined in a fiery circle (experimentally studied both by myself and by Prof. Bourne, of Madras, and reported on in the Proceedings of the Linnean Society and the Royal Society) the spasmodic struggles of an animal artificially confined and tortured, have been, as we clearly demonstrated, mistaken for efforts at self-destruction. The biting of its own body by the snake may be justly compared with the "biting the dust" attributed to men who die in hand-to-hand struggle, or to the biting of their own hand or arm by unhealthy children when suffering from a paroxysm of anger. E. RAY LANKESTER.

Oxford, August 11.

Imitation or "Instinct" by a Male Thrush?

ON the evening of July 19 a young thrush was caught in my conservatory and placed in a large outside aviary. The following morning I observed the parent birds feeding the young one through the bars with worms. In the same aviary there had been for more than ten years a male thrush which had been captured when quite young and had never been mated or troubled with family cares. On observing the parents of the young bird feeding their offspring he at once followed their example. On putting some bread and milk into the aviary he flew down, took up a piece and tried to induce the young bird to open its beak. At first the young thrush appeared to be afraid of accepting food from the foster-father, but after some persuasion it allowed itself to be fed with bread and milk, hemp seed, and other food. The parent birds were watching from the outside, and during the whole time occupied by the old male in feeding their progeny were also trying to introduce food through the bars. The day after (July 21) the parent birds did not make any further attempt to introduce food, but contented themselves by watching their young one from a tree close at hand. If any of the house-cats approached the aviary the parents would at once give the alarm. In the course of another day or two they abandoned the young one entirely to the care of the old foster-father, who has proved quite worthy of his trust, as the young bird is now able to feed himself and is in a very thriving condition. The old male still insists, how-

ever, on giving it any delicate morsel he may find. This observation appeared to me to be of sufficient interest to record in your columns, as the old male bird certainly in this case learnt how to feed the young one by observing the proceedings of the parent birds. He had never reared any young ones of his own, and had never had any opportunity of seeing other families brought up in the aviary. E. BOSCHER.

Belle Vue, Twickenham, Aug. 1893.

Intrusive Masses of Boulder-clay.

THE letter of Messrs. Graham Officer and Lewis Balfour upon the glacial deposits of Bacchus Marsh suggests the desirability of uttering a word of caution against the assumption that boulder clay intercalated between two beds of rock is necessarily of intermediate age. I have repeatedly observed *intrusions* of boulder clay into the triassic sandstones of Lancashire and Cheshire, but never so striking an example as that described by Mr. Arthur R. Dwerryhouse in the current number of the *Glacialists' Magazine*. In his paper and the accompanying plate he shows how a series of glacial and triassic deposits were displayed in a trench in such a way as to give the impression that they were interbedded, sandstone being both below and above the glacial deposits. A minute examination established the fact that the drift deposits had been thrust in amongst the older rocks along a line of weakness due to the presence of a bed of marl. The intrusion had penetrated to a distance of *fifty yards* from the outcrop of the marl-bed.

I do not suggest that Messrs. Officer and Balfour have been misled by such an appearance, but merely warn geologists in general against falling into error.

We have heard much of late of floods and other catastrophes, even from geologists possessing a considerable intimacy with the phenomena of the British drift deposits. It would be interesting to learn in what way these injections of glacial sludge would be explained by the advocates of deluges.

PERCY F. KENDALL.

Yorkshire College, Leeds, August 14.

A Peculiar Discharge of Lightning.

I SHOULD like to add to the many recent accounts of lightning discharges the following particulars of which I have not yet seen any published account.

On the afternoon of Wednesday, July 26, during a storm at about 5.30, a blue flame was observed by some of the inhabitants of Epping to approach and shatter the chimney of a house upon the hill, occupied by Mrs. Brown and family at the time.

An examination of the interior of the house shows the discharge to have passed chiefly by the bell wires, which are fused, down one corner of a room upon the upper floor, breaking the back of a chest of drawers near, and setting the wall in the vicinity on fire.

On the ground-floor the discharge seems to have taken two paths to earth, viz. down the corner of a front room by means of some metallic damp-proof paper, and in the kitchen adjacent by means of some wooden cupboards, the doors of which were much broken and thrown across the room.

Mrs. Brown, who was seated in the front room, states that a few seconds before the house was struck she noticed what appeared to be a darkened space, surrounded by a crimson fringe of flame in the corner (perhaps a brush discharge), and her son in the kitchen at the time testifies to having seen a similar thing previous to what appeared to be the bursting of the luminous mass, which occurred with a loud report, filling the house with smoke and the usual accompanying smell of ozone. The walls are much damaged, and the polarity of a small compass in a drawer of a sideboard nearest the path of discharge was reversed. I considered the apparent forewarning of the brush discharge of sufficient interest to justify this letter.

WILLIAM BREW.

Electric Light Department, British Museum, August 8.

The Mean Density of the Earth.

IN a note in your issue of August 10, adding to the list of values for the mean density of the earth, which you gave on July 27, it is stated that Jolly and Poynting obtained the value 5·58. This is, I believe, the value obtained by von Jolly, but my final result, as published in the Philosophical Transactions for 1891, is 5·493.

In any account of recent work on this subject I think von Sterneck's experiments at Pribram and Freiberg deserve notice. These were made in the years 1882–5, and were pendulum experiments of the Harton Pit type. The method of comparing the times of swing of the pendulums below and at the surface was, I believe, quite new, and consisted in determining the coincidences with the same clock, which gave simultaneous half-second signals at the two stations by means of an electric circuit. The results unfortunately tend to confirm the conclusion which had, I think, been already drawn from Airy's work—that the mine method of experiment, though it may add to our knowledge of the constitution of the surface strata, is useless in determining the mean density of the earth.

Major von Sterneck's papers are published in the Proceedings of the Militär-Geographisches Institut of Vienna.

Pensarn, Abergele, August 12. J. H. POYNTING.

The Grouping of Stars into Constellations.

CAN you or some of your readers kindly give me an answer to the following questions, or tell me where I may obtain information on the subjects?

Did the Assyrians, Egyptians, Greeks, and Persians group the stars in the same manner into constellations? In cases where they did so were the constellations usually named by all the nations after the same animals?

How were the constellations, which we call after Greek heroes, named by Assyrians and Egyptians?

Do the different races of the present day, Chinese, Polynesians, Hindoos, Negroes, Americans, &c., each group the stars in a peculiar way?

If each race has its own plan of grouping the stars can we make use of this peculiarity in ascertaining the affinity of various races and nations? M. A. B.

Terriers Green, High Wycombe, August 11.

Numerous Insects Washed up by the Sea.

HAVE any of your correspondents mentioned the following fact? For the last two days, August 8 and 9, the shore at Dymchurch, Kent, and for more than two miles towards Hythe, was covered with countless quantities of winged ants washed to the shore by the waves. At low tide one sees three or four rims, so thick that each makes a black stripe, from two to three inches wide, running without interruption for more than three miles, and probably extending to a greater distance. We have had during these days winds from the north-east, very light on Tuesday morning, but strong since that.

Dymchurch, Kent, August 10. SOPHIE KROPOTKIN.

A Substitute for Ampère's Swimmer.

IN NATURE of July 27 Mr. Daniell gives a substitute for Ampère's swimmer. In Denmark we use the following simple rule given by Prof. Holten at least twenty years ago. The outstretched right hand is put in the current with the palm turned toward the magnet and the fingers in the direction of the current. Then the north-seeking pole will be moved in the direction of the thumb. HANNA ADLER.

Copenhagen, August 3.

A Correction.

IN my paper on "The Chatham Islands: their relation to a former Southern Continent," just issued among the Supplementary Papers of the Royal Geographical Society, vol. iii., there occurs a slip in the third and fourth lines from the foot of page 9, which I should feel obliged by your kindly allowing me to correct in your columns. My attention has been called to it by Prof. Newton, of Cambridge. In quoting from his and Sir Edward Newton's observations in the appendix to Captain Oliver's voyage of Leguat, as to the "now submerged Continent," of which Rodriguez, Mauritius, Bourbon, and Madagascar are, according to them, the existing fragments, I inserted the words "named *Lemuria*, by Dr. Sclater" after the word "continent." These words of mine should have occurred within square brackets, the absence of which was, I regret, overlooked in the proof. "Now the old land-connexion," writes Prof. Newton, "of the Mascarene Islands with Madagascar, of which we spoke as probable, is not at all necessarily the same thing as 'Lemuria,' which Mr. Sclater supposes to have reached some of the Malayan countries."

61, Glebe Place, Chelsea, S.W. HENRY O. FORBES.

THE ASTRONOMICAL HISTORY OF ON AND THEBES.

II.

IN relation to the extract from Brugsch, given in the last article, to the effect that there was one series of monuments with its starting point in the Delta, it must be emphatically stated that the results obtained from these monuments, studying them from the astronomical point of view, leads to a very different conclusion. Instead of one series there are distinctly two, absolutely dissimilar astronomically, and instead of one set of temple-builders going up the river there were two sets: one going up the river building temples to north stars, the other going down building temples to south stars; and the two streams practically met at Thebes, or at all events they were very fully represented there.

The double origin of the people thus suggested on astronomical grounds may be the reason of the name of "double country," used especially in the titles of kings, of the employment of two crowns, and finally of the supposed sovereignty of Set over the north, and of Horus over the south divisions of the kingdom.[1]

With regard to the start point of the temple-builders who came down the river, there is no orientation evidence, for the reason that there is little or no information from the regions south of Naga. At Naga (lat 16° 18′ N.), Meroë (lat. 16° 55′ N.), Gebel Barkal and Nuri (both in lat. 18° 30′ N.), there is information of the most important kind, but beyond Naga there is a gap; but since important structures were erected at the places named in, I think, early times (3-4000 B.C.), it is probable that the peoples who built them stretched further towards the equator.

With regard to the southern limits of Egypt in the time of Thotmes, it is supposed that the south frontier Kali of the inscriptions is probably connected with Koloë in 4° 15′ N. lat. according to Ptolemy.[2]

The authority for the general statement I have made rests upon the probable dates I have found for the first foundations of the temples of both series (N. and S. stars) which I have investigated, and here I must re-state that in almost every case that foundation precedes the generally-received date, which generally has reference to a stone building; while in all probability the first structure was a brick shrine merely, and in support of this view I may state that the looking after ruined shrines was recognised as one of the duties of kingship.

"I have caused monuments to be raised to the gods; I have embellished their sanctuaries that they may last to posterity; I have kept up their temples; I have restored again what was fallen down, and have taken care of that which was erected in former times."[3]

Not only did Thotmes III. find the original temple of Amen-Rā built in brick, but he found the temple at Semneh in brick also, and he rebuilt it in memory of Usertsen III.[4]

I have prepared a table which it is not necessary to give in this place. I bring together the foundation dates I have found most probable, bearing the above and many other considerations in mind. The dates are, of course, only provisional, since local data are in many cases wanting. Where no information is forthcoming as to the height of the horizon visible along the temple axis, I have assumed hills 1° high.

The following general conclusions may be drawn from the table:—

I. At the earlier periods there are well-marked epochs of temple-building revealed by the table.

II. The temples to the north stars, α Ursæ Majoris and γ Draconis, begin in the Delta.

III. The temples erected to the southern stars (α Centauri and Phact) begin at Gebel Barkal, Philæ, and Thebes almost simultaneously.

IV. The first north star temples for the worship of Set and Ptah were erected between 5400-4200 B.C. The series is then broken till about 3500.

V. The first south star temples (Phact at the summer solstice and α Centauri at the autumnal equinox), begin about 3700 B.C.

VI. γ Draconis replaces α Ursæ Majoris at Denderah, and north star temples are for the first time erected in the south at Karnak and Dakkeh in 3500 B.C.

VII. For the first time about 3200 B.C., N. and S. star temples are built simultaneously.

VIII. After this the building activity is chiefly limited to temples to southern stars.

If we take Brugsch's dates, we find that the foundations of the greatest number of temples were laid about the time of Seneferu, Pepi, and the twelfth dynasty. The more modern kings founded few temples, their function was that of expanding, restoring, and *annexing*. Even Thotmes III. seems to have laid no new foundations except perhaps that of the Ptah temple at Karnak, and that is doubtful.

This after all is not to be wondered at. Three thousand years of observations at least had shown that the stars were not to be trusted to fix a festival day, and the true astronomical user of the ancient temples had quite passed away. Still the ancient shrines were there, what more natural then, than to embellish them? The priests, by insisting upon the vague year had reserved to themselves a perfect means of hiding all festival difficulties for once in 1460 years; the old star would rise on the proper day of the Egyptian month, although it would be no longer visible in the temple. Indeed it is extremely probable that we have here the real reason of the priestly action. They were not fools, and they could, one would think, have had no better reason than this.

The wonderful Hall of Columns called Khu-mennu (splendid memorial), in the temple of Amen-Rā, was dedicated by Thotmes III. not only to Amen-Rā, but to his ancestors. It is interesting to note who these were in the present connection. I give them with Brugsch's dates.[1]

	B.C.
Seneferu	3766
Assa	3366
Pepi	3233
The Antefs	2500
The most famous sovereigns of the twelfth dynasty	2433—2300
30 princes of the thirteenth dynasty	2233

Of these ancestors, the first limited himself to southern temples, the majority, built near Pepi's time, were south temples. The twelfth dynasty was more catholic.

The more we continue it, the more interesting does this inquiry into the north star temples as opposed to the south star temples become.

These considerations are not limited to the temples, they apply also to pyramids.

At Gizeh we find both temples and pyramids oriented east and west.

At Gebel Barkal, Nuri, and Meroë in Upper Egypt, we find both temples and pyramids facing south-east, and at the former place, where both exist together, we find well marked groups of pyramids connected by their orientations with each temple.

In the following tables I give the values for Meroë, Nuri, and Gebel Barkal:—

[1] Brugsch, "Egypt," p. 180.

[1] Brugsch, "History," p. 6. [2] Brugsch, "Egypt," p. 184.
[3] Inscription of Thotmes III., translated by Brugsch, "Egypt," p. 188.
[4] Brugsch, "Egypt," p. 184.

Meroë.[1]

Cult.	Magnetic Azimuth.	Astronomical Amplitude.	Decl.
Pyramid 16	N. 102° E.	3½° S. of E.	S. 3¼°
Pyramid 20	N. 103° E.	4½° S. of E.	S. 4¼°
Temple near Wasser Becken	N. 112° E.	13½° S. of E.	S. 12⅞°
Pyramid 15	N. 112° E.	13½° S. of E.	S. 12⅞°
Pyramids 14, 37 ...	N. 113° E.	14½° S. of E.	S. 13⅞°
Pyramid 10	N. 116° E.	17½° S. of E.	S. 16⅞°
Pyramid 39	N. 118° E.	19½° S. of E.	S. 18⅞°
Pyramid 19	N. 83° E.	15½° N. of E.	N. 14⅞°

Nuri.[2]

Cult.	Magnetic Azimuth.	Astronomical Amplitude.	Decl.
Pyramids 10, 11, 12	N. 136° E.	37½° S. of E.	S. 35¼°
Pyramids 1, 4 ...	N. 137¼° E.	38⅜° S. of E.	S. 36¼°
Pyramids 13, 14, 15	N. 139° E.	40½° S. of E.	S. 38°
Pyramids 2, 3, 16, 17	N. 145½° E.	47° S. of E.	S. 43¾°
Pyramids 5, 6, 7, 8, 9	N. 146½° E.	48° S. of E.	S. 44¾°

Gebel Barkal.[3]

Cult.	Magnetic Azimuth.	Astronomical Amplitude.	Decl.
Temple E	N. 132° E.	33½° S. of E.	S. 31½°
Pyramid 18	N. 132½° E.	34° S. of E.	S. 32°
Temple L	N. 136½° E.	38° S. of E.	S. 35½°
Pyramids 9, 13 ...	N. 136° E.	37½° S. of E.	S. 35¼°
Pyramid 11	N. 140° E.	41½° S. of E.	S. 39°
Pyramid 1, 2	N. 141° E.	42½° S. of E.	S. 39¾°
Temples J and H ..	N. 145° E.	47½° S. of E.	S. 44¼°
Pyramid 20	N. 146° E.	47½° S. of E.	S. 44¼°
Pyramids 2, 15, 16, 17	N. 147° E.	48½° S. of E.	S. 45¼°
Temple B...	N. 152° E.	53½° S. of E.	S. 49¾°
Pyramids 5, 6, 7, 8, 10	N. 153° E.	54½° S. of E.	S. 50½°
Pyramid 19	N. 156° E.	57½° S. of E.	S. 53°
Temple A	N. 170° W.	88½° S. of W.	S. 71¼°

It seems quite justifiable from the above facts to conclude that the pyramids and temples oriented S.E. and, as I hold, to *a* Centauri when it heralded the autumnal equinox, were not built by people having the same astronomical ideas, worships, and mythology as those who built at Gizeh due E. and W., and marked the autumnal equinox by the heliacal rising of Antares.[4] The only thing in common was noting an equinox, and so far as this goes we may infer that neither people dwelt originally in the Nile Valley, but came by devious ways from a country or countries where the equinoxes had been made out.

J. NORMAN LOCKYER.

[1] For plans see "Lepsius," vol. ii. 133 and 134. A west variation of 8½° has been assumed.

[2] For plans see "Lepsius," vol. ii. 130. A west variation of 8½° has been assumed.

[3] For plans see "Lepsius," vol. ii. 125 and 127. A west variation of 8½° has been assumed.

[4] There is a point of great interest here. It would seem from Captain Lyons' examination of the temples at Wady Halfa, which I make out to have been oriented to *a* Centauri, that when the two races were amalgamated in later times, both the stars to which I have referred as heralding the equinox were personified by the same goddess Selk

APPARATUS ILLUSTRATING MICHELSON'S METHOD OF OBTAINING INTERFERENCE BANDS.

IN the *American Journal of Science* for August, 1881, Captain Michelson described an ingenious method for producing interference bands, used by him in determining the relative motion of the earth and the luminiferous ether. Light from a lamp at *a* falls on a thinly silvered mirror *b*, where it divides into two rays—one, *b c*, reflected from the thinly silvered surface, is reflected back to *b* by the plane mirror *c*; the other ray traverses the glass plate *b*, and falls on the plane mirror *d*, whence it is reflected back to *b*. Here both rays reunite, and pass onward toward *e*. The mirrors *d* and *c* are silvered and polished on their front surfaces. By this means bands, similar to Newton's rings, are obtained between the mirror *c* and the reflection of *d* in *b*; the retardation of one ray with respect to the other being the length *b c*–*b d*; *f* is an unsilvered piece of plane glass, cut from the same piece as *b*, and placed in the ray *b c*, so as to compensate for the ray *b d* passing twice through *b*; otherwise, owing to the dispersive power of the glass in *b* different wave-lengths of light in the ray *b d* would be unequally retarded in comparison with the same wave-lengths in the ray *b c*. If the path *b c* be now equalised in length with the path *b d*, and if, moreover, the piece of glass *f* be exactly equal and parallel with *b*, the central band will be black, owing to the ray

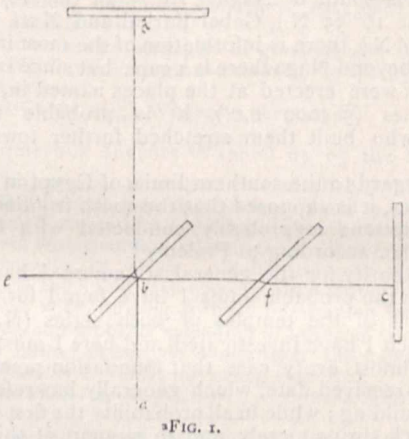


FIG. 1.

d b being reflected from the rarer medium, whilst *a b* is reflected from the denser medium.

Apparatus to show these bands can be easily and cheaply set up, and owing to the fact that an extended source of light may be used, they can easily be projected, and thus many interesting experiments shown to a large audience. The following is a description of a simple construction of the apparatus which I have found to work admirably.

All the parts are mounted on a piece of plate-glass ½″ × 9″ × 12″. The two mirrors *d* and *c*, each two inches square, were silvered by the milk-sugar process, and afterwards polished with washleather and rouge in the ordinary manner. The mirror *b* was withdrawn from the silvering solution when only a thin layer had been deposited; no polishing was necessary. The layer of silver should reflect considerably more than half the light incident upon it, as thus the reflections from the unsilvered surface of *b* become relatively insignificant. Ordinary plate-glass was used in each instance.

Each mirror was attached vertically by pitch to a stand composed of two pieces of band brass soldered together at right angles, having three feet *a*, *b*, *c* (Fig. 2). In the case of the glasses *b*, *f*, and *c* (Fig. 1), a screw of pitch $\frac{1}{20}$″ was inserted in *c* (Fig. 2) as a rough adjustment for verticality of the mirror.

The mirrors *b* and *f* were maintained in position[1] by the conical foot *a* (Fig. 2) standing in a cylindrical hole in a blank which was stuck to the glass bed-plate by pitch, *b* resting simply on the surface of the glass, whilst the foot *c* stood in a V-groove in a brass blank, also stuck by pitch to the glass bed-plate. The fine adjustments required are for *c* (Fig. 1) a motion in the direction *c f*, and for *d*, adjustments in altitude and azimuth. These were respectively obtained by placing the two feet *b* and *c* (Fig. 2) (which should be rounded) of the mirror *c* (Fig. 1) in a long V-groove (a piece of angle brass was used),

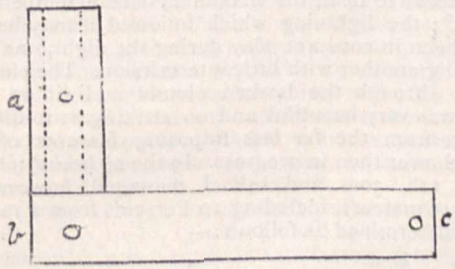


Fig. 2.—The dotted lines indicate the position of the mirror.

the third foot resting on the glass surface; the foot *c* was held against a screw passing, in the direction of the groove, through a brass blank soldered at the end thereof, which gave the longitudinal motion required for that mirror. By means of a lever of 18″ or so in length attached to it, a piece of steel wire, with a thread cut by means of stocks and dies of 40 to the inch, was found capable of adjusting to a quarter of a wave-length of light. The adjustments of the remaining mirror *d* (Fig. 1) were obtained by allowing the conical foot *a* (Fig. 2) to rest in a cylindrical hole, whilst the foot *b* rested on the glass as in the

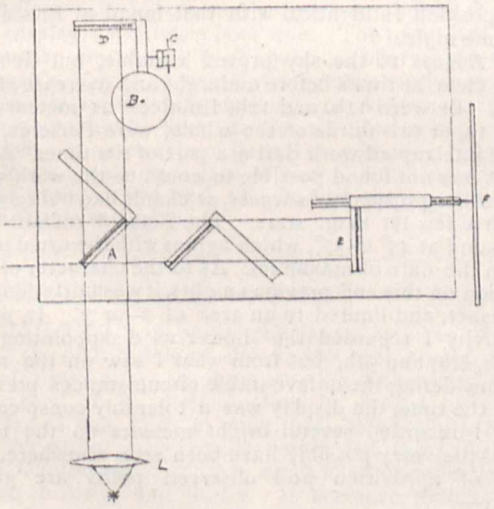


Fig. 3.

mirrors *b* and *f* (Fig. 1). The leg *c* (Fig. 2) was formed of a piece of steel wire with a screw thread cut as described above, with a large brass blank soldered to its upper end; this gave the adjustment in altitude. The adjustment in azimuth was obtained by holding the horizontal piece of band brass *b c* by means of a piece of elastic against the end of a screw of similar pitch to that last described, passing through a vertical pillar attached to the base-plate. The whole arrangement is shown in Fig. 3.

To avoid the effect of vibrations the whole may be supported on a block of stone, resting, in its turn, on hollow india-rubber balls; a plan adopted successfully by Dr. O. Lodge. When mounted in this manner the bands may be shown in a room possessing only an ordinary wooden flooring.

The bands are obtained as follows: A bat's-wing burner, or other source of white light, is placed at the focus of the lens L (Fig. 3) and arranged so as to illumine the mirror A. A card with a pinhole in it is then placed in front of the lens L, and, on looking in the direction M A, two images of this will be seen. By means of the screws B and C these two images are superimposed; and the distance A E having been adjusted by means of the screw and lever F and a steel scale, to be equal to A D, a sodium flame is placed in the focus of L and the pinhole card removed; the sodium bands will at once appear. By means of the screws B and C these are adjusted to a convenient width, and then, the bat's-wing burner having been replaced in the focus of L, the lever F is turned very slowly till the coloured bands appear. This can be done much more easily by placing a piece of platinum wire holding some sodium into the flame,

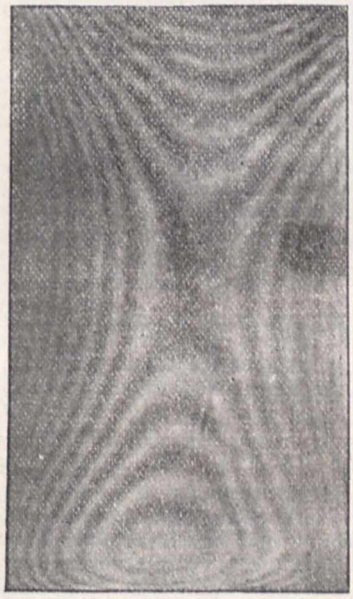


Fig. 4.—Photograph of interference band showing *cold* match.

when the bands due to the sodium will be faintly outlined on the white background, thus giving a guide as to whether or no you are turning the screw F too fast. The bands appear on the surface of the mirror E, and if an electric arc or a mixed gas limelight jet be substituted as the source of light, they can be projected on a screen so as to be visible to a large audience.

The forms of these interference bands, supposing each of the four pieces of glass to be perfectly plane and parallel, is given by Michelson (*Phil. Mag.* April 1882). The peculiar form of the bands obtained in my apparatus is shown in Fig. 4; this form is due to the curvature of the surfaces of the various glasses. A thin piece of glass or a soap film may be introduced into one of the paths and the displacement of the bands exhibited. But perhaps the prettiest experiment is to introduce the glowing end of a match into one of the rays. Suppose this ray to be A E (Fig. 3); then the appearance presented is exhibited in Fig. 5, where the bands are seen to curve round the end of the match as if it were pushing them inwards. A cold body, such as a piece of copper wire, cooled in a freezing mixture, has an opposite effect, attracting the bands into it. These effects are, of course, due to the heating or cooling of the air near the hot or cold body. Now it will be found on slowly turning the

[1] For some remarks on the general principles of these "geometrical slides" and "clamps," see Thomson and Tait's "Nat. Philosophy," part i. p. 150.

screw F (Fig. 3) so as to shorten the path A E that the bands at the side move in toward the centre, the opposite being the case on lengthening the path A E. Therefore heating the air (*i.e.* rendering it less dense) has the same effect as shortening the path (*i.e.* it accelerates the motion of the light passing along it), whilst

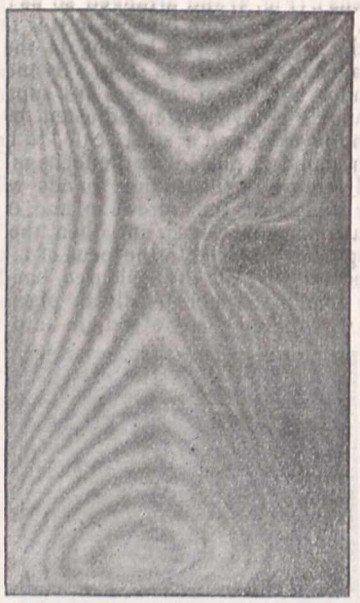


FIG. 5.—Photograph of interference bands sh wing effe:t of introducing glowing end of match.

cooling the air (rendering it denser) has the opposite effect; which demonstrates very simply the truth of the undulatory as opposed to the emission theory of light; for on the latter theory the exact reverse would be the case.

EDWIN EDSER.

THE AUGUST METEORS, 1893.

THE Perseid shower, though it cannot rival periodical displays such as the November Leonids and Andromedes when at their best, is certainly of equal interest, for it forms a tolerably rich display every year, and continues active during several weeks from a radiant which has a comet-like motion of about 1° R.A. per day eastwards. A vast number of observations have been made during the last half-century, but it must be confessed that we have by no means completed our investigation of this remarkable stream. Nor have we gained a thorough knowledge of the numerous and fairly prominent minor showers which contribute to render this epoch the most significant and the most interesting period of the year to the meteoric observer.

Either moonlight, or cloudy wet weather, prevented my obtaining any observations at the latter part of July this year, and it was not until August 4 that I commenced work. Moonlight was, however, pretty strong, and in a watch of about half an hour I only saw four meteors, including one typical Perseid from a radiant at about 36° + 56°.

On the following night, August 5, the sky was much clouded, but between 10h. 15m. and 11h. 45m. I saw, in clear spaces, twelve meteors, of which four were Perseids, indicating a radiant at 39° + 55°. The brightest meteor seen was at 11h. 3m., but it appeared behind thin cloud in the northern sky. It was fully equal to a 1st mag. star, and left a bright streak along its path from 17½° + 76° to 219° + 78°. This was not a Perseid, the direction of flight being from near γ Andromedæ.

The nights of August 6 and 7 were cloudy and no observations could be secured.

On August 8 the sky cleared and I counted 36 meteors in the two hours from 10h. 50m. to 12h. 50m. There were 12 Perseids amongst them and the radiant was well defined at 41° + 56°. At 11h. 25m. a fine Perseid about equal to Jupiter flashed out in the region of Polaris and left a streak of nearly 20 degrees along its course.

August 9 proved fine, but lightning was extremely frequent and vivid during the whole night, and considerably interfered with the observations. It proceeded from clouds low in the east and north quarters, but apart from that the firmament was very clear. The day had been one of excessive heat, the maximum shade temperature being 84°; the lightning which followed it may be said to have been in constant play during the night, one flash succeeding another with little intermission. The effect as it burst through the broken clouds and lit up their borders was very beautiful and so striking as to distract attention from the far less imposing features of the meteor shower then in progress. In the 2½ hours' interval between 11h. 30m. and 14h. I managed, however, to observe 45 meteors, including 20 Perseids from a radiant which I determined as follows:—

h. m.	h. m.		°		
11 30	to 12 0	...	42 + 56	...	4 meteors.
12 0	to 13 0	...	43 + 57	...	9 ,,
13 0	to 14 0	...	43 + 57	...	7 ,,

Adopting the mean centre as at 43° + 57°, I think the position may be considered a very accurate one for the date. I saw no exceptionally brilliant meteors during the night, though several of the 1st mag. were recorded, and the Perseids struck me as being fainter than usual. Most of them traversed swift short paths not very far from the radiant, so that the position of it could be determined very satisfactorily. Mr. Booth of Leeds informs me that he found the Perseid radiant at 43° + 57° from 15 meteors of this shower observed on August 9. This position is identical with that found at Bristol on the same night.

On August 10 the sky proved variable, but it was pretty clear at times before midnight and overcast afterwards. Between 11h. and 12h. I noticed 21 meteors, of which 14, or two-thirds of the whole, were Perseids, but clouds interrupted work during a part of the time. After 12h. it was not found possible to continue the work with any further prospect of success, as clouds had obliterated all but a few 1st mag. stars. The Perseid radiant was now found at 45° + 57°, which agrees with the usual position on the date of maximum. As to the character of the radiation on this and previous nights, it was fairly definite and exact, and limited to an area of 2° or 3°. In point of activity I regarded the shower as disappointing on the 5th, 8th, and 9th, but from what I saw on the 10th, and considering the unfavourable circumstances prevailing at the time, the display was a tolerably conspicuous one. I recorded several bright meteors on the 10th, and, as they may possibly have been seen elsewhere, the times of apparition and observed paths are given below:—

	Mag.	From		To		Radiant.	Notes.
		α	δ	α	δ		
h. m.		°	°	°	°		
11 0	1	1	+11	26	+19	304	−14 Slow.
11 21	1	331	+38½	317	+23½	Perseid	Swift, streak.
11 24	1	325	+29	314	+15	Perseid	Swift, streak.
11 43	0	42	+57	40	+53½	Perseid	Slow, b. streak.
11 56	1	140	+84½	220	+70	Perseid	Swift, streak.

On August 11 the sky was overcast.

On August 12 it was partly fine before 13h., but by no means favourable for this class of work. I counted 24 meteors, including 7 Perseids with radiant at 48°+57°.

On August 13 the conditions had greatly improved, and after midnight there was not a cloud in the sky. Watching for 3½ hours I recorded 43 meteors and found the Perseid shower still visible from a radiant at 48°+57° (8 meteors). No exceptionally bright meteors were seen, but at 13h. 5m. one about equal to Jupiter fell from

338½°+27° to 347°+19½°, its radiant being very probably at 271°+48° near the head of Draco.

On August 14 the atmosphere was unusually clear, and during the four hours from about 10h. 15m. to 14h. 15m. I observed fifty-six meteors. The Perseid shower was still distinctly visible, and the meteors pretty bright. From seven accurately observed paths a very good radiant was obtained at 49° + 57°. There was also a well-defined shower of streak-leaving meteors from Camelopardus at 61° + 59°, and these, if confused with the Perseids, would have given the latter radiant a very diffused appearance. On this and the preceding nights I saw many Cygnids and Cepheids from radiants at 292° + 53° (sixteen meteors) and 311° + 62° (fourteen meteors), and this pair of showers formed by far the most important of the minor displays of the epoch. I had in previous years detected the Cygnids, but never remember to have seen the shower of Cepheids on such activity.

On comparison of my Perseid radiants deduced, on August 5, 8, 9, 10, 12, 13 and 14 it will be seen that they exhibit an easterly movement in satisfactory agreement with my observations in preceding years. This remarkable displacement of the radiant may now almost be regarded as "an old story" but it will always remain a very significant and interesting feature of the shower both from an observational and theoretical standpoint. The motion of the radiant amongst the stars may be nearly as easily and certainly observed by an experienced and precise observer as the motion of a comet. The circumstances are different of course, for a radiant is simply an apparent position and not a visible object, but trustworthy observations define this position with considerable exactness, though it is impossible to eliminate all the sources of error.

Mr. Corder, at Bridgwater, informs me that on August 10, before 14h. he counted 129 meteors, but he regarded the display as rather a poor one. The mean position of the radiant was at 44°+57½°, but he considers that it shifted from 40°+56½° to 47°+58½° during his observation.

Mr. Corder, watching until 15h. on August 13, counted 77 meteors, but he says the Perseids had almost ceased, and gave an uncertain radiant, but such as it was could be located near the stars B and C Camelopardi. He found a very active and well-defined shower of Cygnids from the point 293° + 50°. W. F. DENNING.

CHOLERA AND ARTICLES OF DIET.

ALTHOUGH in by far the larger number of cases the distribution of cholera has been traced to the use of impure water, yet there are a few authentic instances on record of its dissemination by means of various articles of diet, such as milk, fruit, salad, whilst Kossel and Steyerthal quite recently report two cases (*Deutsche med. Wochenschrift*, 1892) in which its communication was traced to bread and butter. It becomes, therefore, not only of interest but importance, to ascertain what is the vitality of the cholera organism when purposely brought in contact either superficially or incorporated with various articles of food. Researches in this direction have been undertaken from time to time by various investigators, Babes, Celli and others, whilst Dunham's experiments published in the *Medical Record* for 1892 are amongst the most recent and exhaustive on this subject. This author found that cholera organisms purposely introduced on to salad leaves and placed in a covered dish and kept at the ordinary temperature of a room, retained their vitality for five days, on cooked cauliflowers for from six to ten days, and on the same vegetable uncooked for thirteen days. On a sliced strawberry they did not survive more than twenty-four hours.

Some important contributions to our knowledge of this subject have been made by Friedrich, and are brought together in an elaborate memoir, "Beiträge zum Verhalten der Cholerabakterien auf Nahrungs und Genussmitteln" published in the *Arbeiten a. d. Kaiserlichen Gesundheitsamte*, vol. viii. 1893, p. 465.

The range of materials investigated is very extensive, upwards of fifty different articles being specially studied in this respect, including numerous kinds of fruit, several vegetables, besides milk, tea, coffee and cocoa, also particular descriptions of beer and wine, whilst amongst the miscellaneous materials examined may be mentioned caviar, biscuits, bonbons, tobacco, and snuff!

In the majority of cases the bacilli were not only rubbed on to the surface of the various fruits and vegetables, but were also inoculated on to slices, so that the effect on the bacillus of the *composition* of a particular fruit or vegetable could be ascertained. When simply exposed on the exterior of a given material, the vitality of the bacillus depends chiefly on the degree of moisture which is present in its environment, this organism being specially characterised by its rapid destruction in dry surroundings, but when brought in contact with the juices it is the proportion of fruit acid and sugar present which primarily determine its behaviour. The cholera bacilli are very sensitive to acid, and hence their destruction on most slices of fruit in from one to six hours.

Thus when inoculated on to slices of bright red very juicy and sour cherries, the bacilli were annihilated in three hours, whilst when simply rubbed on the surface and kept in a moist atmosphere they were still alive at the end of five days. On the other hand, when thus treated and exposed to the ordinary air of a room, the bacilli could not be found after twenty-four hours, whilst when placed in the direct sunshine their vitality was limited to one hour and a half.

But even on slices of fruit containing a much smaller amount of acid, such as pears, the vitality of the cholera organism was not much prolonged, and the reason for this must be sought in the fact that, when grown in solutions containing sugar, this organism produces acid, and the acid thus produced impedes its further development and destroys its vitality.

On vegetables such as cucumbers, cauliflowers, cabbages, the cholera bacillus maintains its existence for several days; thus on spinach leaves preserved in a damp atmosphere, the bacilli were still present after twelve days, and even when exposed to the ordinary air of a room they did not disappear until after six days.

As regards the behaviour of the cholera organisms in tea it is interesting to note that in a 3 per cent. infusion of black Chinese tea they are destroyed within twenty-four hours, whilst in a 4 per cent. infusion no trace of them could be found at the end of sixty minutes.

Friedrich has confirmed the results of other investigators on the bactericidal properties of coffee, finding two hours' immersion in a 6 per cent. infusion of this material sufficient for the destruction of these organisms.

In various kinds of beer, Munich, Pilsener, and Lager, they could not survive more than from one to three hours, but still more rapid was their extinction in white and red wine, for five minutes after their introduction they could no longer be found in the former, whilst in the latter their vitality did not exceed twenty minutes.

From the numerous investigations recorded it is obvious that during any epidemic of cholera the consumption of uncooked fruit and vegetables should be avoided, or that at any rate precautions should be taken to ensure their sterility by careful cleansing or by the removal of the rind or skin where possible.

G. C. FRANKLAND.

NOTES.

MEN of science throughout the world will be glad to know that the honour of knighthood has been conferred upon Dr. Joseph Henry Gilbert, F.R.S., who has been associated for

more than fifty years with Sir J. B. Lawes in the agricultural experiments conducted at Rothamsted. British and foreign academies and learned societies have long recognised Dr. Gilbert's claims to distinction, and have bestowed upon him various marks of approval. We are glad now to be able to record that his scientific work has been officially recognised.

PROF. MAX MÜLLER has received from the Sultan of Turkey the gold medal of the Order of Merit, the highest honour in the Sultan's gift.

ZOOLOGISTS will learn with regret that Mr. George Brook, lecturer on embryology to the University of Edinburgh, died suddenly at Newcastle on Saturday night last. His death is a loss to zoology and to those who knew and appreciated him.

THE *Times* announces the death of Rear-Admiral T. A. Jenkins—one of the ablest officers of the U.S. Navy—at the age of eighty-two. In 1846 he prepared a report on the lighthouse systems of Great Britain and the continent. Shortly afterwards he assisted Prof. Bache in making some meteorological and hydrographical observations, and in determining deep-sea temperatures in the Gulf Stream, the vessel in which the investigations were carried on being built under his supervision. In 1852 he was appointed naval secretary of the Lighthouse Board, and from 1869 to 1871 was secretary of the Board. He was also for some time Chief of the Bureau of Navigation.

THE Franklin Institute has awarded a medal and a premium of twenty dollars, in accordance with the legacy of John Scott, of Edinburgh, to each of the following gentlemen:—Dr. Adolph Frank, Charlottenburg, Germany, for a composition of infusorial earth as adapted for filtering purposes; Frank Reddaway, Manchester, for his invention of camel-hair belting; Henry L. Bridgman, Blue Island, Illinois, for his invention of an ore sampling machine; and S. H. La Rue, Trenton, N.J., for his improvements in stoves. An Elliot Gresson medal has been awarded to Frederick E. Ives, Philadelphia, for his system of colour photography known as heliochromy. Any objections to these awards, or evidence of want of originality of the inventions named, should be lodged with the secretary of the Institute before October.

A REUTER'S telegram, dated August 11, reports that a violent shock of earthquake was felt on the previous evening in the small coast town of Mattinata. It was followed during the night by other shocks of less violence, which were felt also at Monte Sant', Angelo, Manfredonia, and Rodia—all towns on the shores of the Adriatic. Later information states that all the buildings in Mattinata were more or less seriously damaged by the earthquake, and great cracks were caused in the walls of the houses. Three persons were killed and four injured, while others were shaken or bruised. The island of Stromboli experienced a sharp shock, followed by an unusually violent eruption of the volcano.

THE Iron and Steel Institute will hold its twenty-fifth autumn meeting at Darlington, from September 26 to 28 inclusive, when several important papers will be read. Prof. Roberts-Austen, F.R.S., will contribute a paper on the influence of the rating of the rupee on the world's iron trade; Mr. H. Bauerman will discourse on the "Metallurgical Exhibits at the World's Fair"; and Mr. Kupelwieser will communicate a paper on the recent developments of the steel industry in Austria. A number of other subjects of technical interest will also be discussed. The members will have the advantage of visiting the numerous iron and steel works in the vicinity of Darlington, and arrangements have been made for excursions to Barnard Castle and Raby Castle.

THE Board of Agriculture have been requested to draw the attention of fruit growers to an international exhibition, to be held by the Russian Society of Fruit Culture, under the patronage of the Czar, at St. Petersburg, in the autumn of 1894, with the object of showing the present condition of the cultivation of fruit and vegetables, of viticulture, of the cultivation of various special plants, and the manufacture of their products. A congress of pomologists will be convened simultaneously with the exhibition. The exhibition will comprise sections dealing, among other matters, with horticulture implements and appliances, and technicality of production, and also literary, scientific, and educational accessories, collections, plans, &c. Detailed regulations of the exhibition and programmes of the various competitions will be published and distributed towards the end of this year. Persons interested in the progress of horticulture and pomology, both in Russia and other countries, are invited by the Russian Government to take part in this international exhibition and congress. Applications for further information should be addressed to the offices of the International Exhibition of Fruit Culture, Imperial Agricultural Museum, Fontanka 10, St. Petersburg.

SERIOUS floods have occurred in Galicia (says Reuter's agency) and they are exceeded in their gravity by disastrous inundations which have visited Saras and Ung, two northern countries of Hungary. The damage done in these districts is immense, and there has been serious loss of life. According to the latest accounts, the waters are now receding. Dispatches from Lemberg describe the havoc that has been wrought in the valleys beneath the Carpathians by the persistent rainfall. The rivers Dniester, Stryi, San, and Dunajec have overflowed their banks, causing great damage, especially in the districts of Zydaczow, Stryi, Przemysl, and Rimanow. At Turka twenty-two houses have been destroyed by the floods or struck by lightning, and many persons lost their lives.

DURING the past week the heat has been excessive in the midland and southern parts of England; it reached or exceeded 80° at Greenwich Observatory on eight successive days from the 8th instant, which is the longest period this summer during which such high temperatures have been recorded. On Wednesday, the 9th instant, and on Monday and Tuesday last, the temperature exceeded 85° in several places, and reached 89° in the neighbourhood of London on the latter day. On the 9th and 10th this exceptional heat culminated in severe thunderstorms in most parts of the country; in Ireland the storms and rainfall were very heavy, the amount of rain measured in the north of Ireland during the week ended the 12th instant being ·8 inch above the average. The heat on the continent has been much greater than in this country; the shade maximum at Rochefort in France reached 106° on Monday last.

DR. W. DOBERCK has communicated to *Hansa* of July 29 and August 5 an interesting article on the typhoons of the China Sea, a subject of which he has made a special study, and for the collection of the necessary materials his position as director of the Hong Kong observatory offers many advantages. So long ago as September, 1886, he communicated to the *Hong Kong Telegraph* a paper on the law of storms in the Eastern Seas. The present article embodies the facts there set forth so far as they relate to the subject in question, together with the results of the experience subsequently gained. The typhoons, like the hurricanes of the West Indies and other parts, generally give premonitory signs, such as the motions of cirrus clouds, the swell of the sea, and motion of the barometer, but there appears to be some difficulty in determining whether depressions will result in ordinary gales or in typhoons, and it is essential to determine quickly how a ship lies with respect to the advance of the centre of the disturbance. In these and other details which are of primary importance to the seaman navigating the China Seas, the information contained in the paper will be very useful.

THE report of the chief of the United States Weather Bureau for the year 1892 has recently been received, and shows that much attention has been given to the improvement of weather forecasts, the result being a success of 82·9 per cent. in the combined predictions of weather and temperature for twenty-four hours in advance. Until recently the issue of predictions was restricted to the Washington office, but now a number of competent observers make forecasts for their immediate vicinity. In order to render this service as efficient as possible, telegraphic reports are received when considered necessary from several of the West Indian islands. Various important investigations have recently been published, and at present the subject of the rainfall of the entire country is under discussion. The policy of the Bureau favours the establishment of high-level stations, and the observatory at Pike's Peak has been reopened; advantage has also been taken of one or two balloon ascents to obtain observations made in free air. Every effort is being made to advance the science of meteorology; the entire territory of the United States is now covered by local weather services with the exception of Alaska, and the weekly and monthly reports issued by them contain tables of meteorological data and discussions of great value to immigrants, invalids and to men of science generally. The *Monthly Weather Review*, issued by the Central Bureau, is a highly creditable work, prepared from the reports of upwards of 2600 observers. We also observe that frequent applications are made to the Bureau for climatological statistics, and that these are generally satisfied without expense to the applicants. The number of such applications during the year amounted to over 500; this fact alone is sufficient to show the liberal policy of the Bureau.

SOME elaborate investigations on the disinfecting powers of hypochloride of soda, chloride of lime, and peroxide of hydrogen have been recently published by Chamberland and Fernbach in the *Annales de l'Institut Pasteur*, June, 1893. When these materials were employed at a temperature of from 40–50° C. and higher, their action was invariably more rapid than when they were used at the ordinary temperature, thus affording a striking confirmation of Heider's experiments on the greater efficiency of disinfectants at higher temperatures, reference to which was made in NATURE for June 15. Micro-organisms in dry surroundings were found far more capable of resisting the action of these disinfectants than when exposed in a moist condition, that whereas in the latter case they were destroyed in a few minutes, in the former they defied an exposure of several hours, even to hot disinfectants. If, however, such dried germs be subjected to one hour's immersion in water they lose their power of resistance, for on the subsequent application of the disinfectant they succumb rapidly. These authors insist, therefore, on the importance of first spraying the walls of a room with water before commencing their disinfection. In conclusion a solution of chloride of lime (prepared by extracting one part of chloride of lime with ten parts of water and diluting the clear extract with ten times its volume of water) is recommended as an exceedingly efficient as well as economical disinfectant for practical purposes.

AN interesting example of the degree of resistance to high temperatures exhibited by some micro-organisms has lately been published in the *Centralblatt für Bakteriologie*, May 17, 1893. Whilst preparing nutritive gelatin-peptone in the usual manner, Heim found that, despite all precautions of sterilisation &c., numerous yellow or reddish-yellow centres subsequently appeared in the culture material. On isolating out these colonies and further studying them, these growths were ascertained to be derived from two spore-producing bacilli which had resisted the usual 10–20 minutes' exposure to steam on three successive days. On further studying these organisms it was found that one of them required three hours' continuous steaming before being destroyed, whilst the other was not annihilated until this had been prolonged for seven hours. These extremely hardy spores were traced to the leaf-gelatin employed, and as in many respects they resembled certain soil-microbes, Heim supposes that in some manner or other during its preparation the gelatin must have come in contact with soil. Still more recently a cladothrix has been found in water which, on account of its ability to resist high temperatures has been called *Cladothrix invulnerabilis* (*Centralblatt f. Bakteriologie*, vol. xiv. p. 14). It was still endowed with vitality after having undergone six successive exposures to ordinary intermittent sterilisation at 100° C.

M. LOUIS BOUTAN has succeeded in taking submarine photographs under various conditions, by a method described in the *Comptes Rendus*, No. 5. A camera constructed for several successive exposures was enclosed in a metal box provided with plane-parallel glass windows mounted in copper rings. The apparatus was mounted on a heavy stand provided with weights, so as to give it a steady footing on the sea bottom. Near the shore, in depths not exceeding 1 or 2 m., the camera could be placed in position without the necessity of the observer entering the water, and negatives were obtained by direct sunlight in about 10 minutes. With an exposure of 30 minutes negatives could be obtained at depths of 6 or 7 m., the apparatus being put up by a diver. The best images were obtained by placing a blue glass in front of the lens, but even the best showed a want of depth which could only be relieved by using a very small diaphragm. This difficulty would disappear if the lenses were adapted to submarine work to begin with. Pictures of the sea-bottom were also obtained instantaneously during a storm by means of a flash-light, consisting of an alcohol lamp fed by a reservoir of oxygen. Magnesium powder was projected into the flame by pressing a rubber ball. The depth at which these photographs can be taken is at present limited to that which can be attained by the diver.

THE absorption of light by platinum at different temperatures is discussed in a highly interesting paper recently communicated to the Accademia delle Scienze di Torino by Dr. G. B. Rizzo. He succeeded in obtaining transparent films of platinum produced under such conditions as to exclude the possibility of oxidation on raising the temperature. The apparatus employed consisted of two glass cylinders joined by a thin tube. Another tube was soldered to the middle of the latter, and connected with an air-pump and a reservoir containing nitrogen. The tubes were filled with nitrogen several times, and exhausted, so as finally to contain a rarefied atmosphere of nitrogen. One of the platinum electrodes was partly encased in glass, and connected with the negative pole of a Rhumkorff coil excited by six Bunsens, the other electrode being connected to the positive pole. Under these conditions the negative pole was volatilised and deposited as a thin film upon the walls of the glass cylinder containing the electrode. The glass cylinder was then disconnected by filling the apparatus with nitrogen to atmospheric pressure, melting the thin tube under the blowpipe, and drawing it out to a rod to be broken off. The platinum electrode was bent out of the way by melting its glass sheath, and the result was a cylinder of glass containing a fine deposit of platinum and filled with nitrogen. This cylinder was placed in an iron cylinder in a small gypsum furnace, and heated by a spiral tube of small gas jets. Light was transmitted through windows in the iron tube, and a Krüss universal spectroscope was used to compare the spectra transmitted through the glass and platinum, and through the glass only. The temperatures were measured by the calorimetric method. After allowing for the various reflections undergone by the light, it was found that

as the temperature increased, the transparency of the film increased, especially in the more refrangible region. It may be added that this phenomenon, if found to hold generally, establishes a new correlation between light and electricity, the increase of electrical resistance of a conductor being accompanied by an increase of transparency.

THE alternate current supplied by the Innsbruck Central Station has been utilised by Dr. G. Benischke for the purpose of investigating the dielectric constants of some solids by the method of Gordon as improved by Lecher. This current charged the condenser positively and negatively at equal intervals, thus avoiding residual effects of all kinds. In order to obtain greater sensitiveness the alternate current was transformed to higher differences of potential by means of an induction coil. It was found that the dielectric constant is independent of the strength of the field in the condenser, and hence also that there exists no perceptible conductivity in the dielectric. The constant of paraffin was found to be 1·89, of ebonite 2·03, of sulphur 2·42, of common glass 4·17 to 4·52, of plate-glass 3·85.

WE have received the supplement to the calendar of the Royal University of Ireland for the year 1893. It contains the papers set at the University's examinations during 1892.

THE report of the fourth meeting of the Australasian Association for the Advancement of Science, held at Hobart Town, Tasmania, in January, 1892, has just reached us. It is edited by Mr. A. Morton.

THE *Midland Naturalist* contains an address delivered by Mr. W. H. Wilkinson, President of the Midland Union of Natural History Societies, on "The Life-History of the Diamond-Back Moth" (*Plutella cruciferarum*). We note that at the annual meeting of the Union on July 11 it was decided to discontinue the publication of the journal.

MESSRS. CROSBY LOCKWOOD AND SON will shortly publish "The Miner's Handbook," compiled by Prof. Milne, F.R.S., of the Imperial University of Japan. The volume is of especial interest on account of the fact that it is being printed under the author's direction at Tokio.

A CORRESPONDENT, "H. K. R.," writing from Victoria, Australia, with regard to a letter in our issue of March 30, refers us to another and in some respects simpler rule for finding the day of the week which corresponds to any given day of the year, to be found in Dr. Charles Hutton's "Mathematical Recreations," published in London in 1803.

THE U.S. Department of Agriculture has just issued a systematic and alphabetic index to new species of North American Phanerogams and Pteridophytes published in 1892, by Miss Josephine A. Clark. The index forms the seventh number of the third volume of contributions from the U.S. National Herbarium.

DR. MCALPINE has prepared a report for the Victoria Department of Agriculture on a poisonous species of Homeria found near Melbourne, causing the death of cattle feeding upon it. The species is *Homeria collina*, Vent.—var. *Miniata*, commonly known as Cape Tulip. There is evidence that it is fast spreading over the Colony, and strenuous measures will have to be taken to eradicate it.

WE have received the following excerpts from the Proceedings of the United States National Museum: Catalogue of the crabs of the family Maudæ in the U.S. National Museum; list of Diatomaceæ from a deep-sea dredging in the Atlantic Ocean off Delaware Bay, and scientific results of explorations, by the U.S. Fish Commission steamer *Albatross*; also notes on Erian (Devonian) plants from New York and Pennsylvania.

THE Memoirs and Proceedings of the Manchester Literary and Philosophical Society (vol. vii. No. 2) contains the second part of Prof. W. C. Williamson's General, Morphological, and Histological Index to his collective memoirs on the Fossil Plants of the Coal Measures. Prof. Harold B. Dixon, F.R.S., contributes a long paper on the "Rate of Explosions in Gases," and in collaboration with Mr. B. Lean, one on the "Length of Flame Produced by the Explosion of Gases in Tubes."

A COPY of Prof. C. V. Riley's presidential address of "Parasitism in Insects," delivered before the Entomological Society of Washington in 1892 has just reached us. It goes to show that "the parasitic forms and the parasitic habit have appeared late in the history of insect evolution on the globe." A number of papers by Prof. Riley on various entomological subjects have also been received. Among them is one on the habits and natural history of the Ox Bot-fly, *Hypoderma bovis*, in the United States. This has hitherto been supposed to be the common species of both America and Europe, but Prof. Riley finds that the species has not been observed and recorded in North America, hence he considers its presence as merely conjectural. The American species is *Hypoderma lineata*, Villiers, and it seems probable that when the life history of the European *H. bovis* has been worked out it will be found to coincide with the American Bot-fly as described by Prof. Riley.

A NEW mineral of exceptional interest, inasmuch as it contains about six and a half per cent. of the extremely rare element germanium, is described by Prof. Penfield, of the Sheffield Scientific School, U.S., in the August number of the *American Journal of Science*. Germanium was discovered in the year 1886 by Prof. Winkler in the Freiberg mineral *argyrodite*, a double sulphide of silver and germanium. The remarkable manner in which the new element was found to correspond with the *ekasilicon* predicted by Prof. Mendeleèff will be still fresh in the minds of chemists. Germanium thus belongs to the fourth or tetravalent vertical group of the periodic classification, occupying the space previously vacant between silicon and tin vertically and gallium and arsenic horizontally. Its atomic weight of 72·3 corresponds almost exactly with the number assigned to the missing *ekasilicon* by Prof. Mendeleèff. The occurrence of this interesting element appears, as far as the writer can gather, to have been noticed previously in only one other mineral specimen besides *argyrodite*, namely, in *euxenite* by Prof. Krüss, two years after its discovery in the former mineral. Since that time Prof. Winkler has prepared a large number of its compounds and from time to time described their properties, so that we now possess a considerable amount of information concerning germanium. The third mineral now announced was brought from Bolivia by Mr. Canfield as a new, rich, and very beautiful silver ore, and submitted to Prof. Penfield for examination. It has been termed *canfieldite* in honour of its finder. The presence of germanium was suspected from its behaviour when heated in closed and open tubes and on charcoal, inasmuch as it much resembled the behaviour of *argyrodite* under similar circumstances. Perhaps the most remarkable characteristic of germanium is that it forms a *white* sulphide, GeS_2. On heating *canfieldite* in a closed tube the sublimate of sulphide was observed to be white, and, moreover, when the mineral is heated on charcoal a white sublimate of oxide and sulphide is produced near the residual bead of metallic silver, together with a number of milk-white semi-transparent fused globules characteristic of germanic oxide, GeO_2. Eventually most of the compounds of germanium were prepared from the mineral and their properties found to correspond in all respects with those described by Prof. Winkler. A sulphosalt soluble in solutions of caustic alkalies like the sulphosalts of tin, antimony, and arsenic was obtained, and the alkaline so-

lution yielded a precipitate of the white sulphide upon the addition of a dilute acid. When this sulphide, GeS_2, was heated in a current of hydrogen, small glittering scales of the lower sulphide, GeS, much resembling crystals of specular iron ore, were formed just beyond the heated portion of the tube; upon continued heating complete reduction occurred, metallic germanium itself being deposited upon the walls of the tube in small greyish-white octahedral crystals which exhibited a particularly brilliant metallic lustre.

Canfieldite, upon analysis, yields numbers which indicate that its composition is Ag_8GeS_6, or $4Ag_2S.GeS_2$. Prof. Penfield points out that the published analysis of Prof. Winkler's for *argyrodite* agrees much better with the same formula than with the formula $3Ag_2S.GeS_2$, which he ascribes to it in his memoir. Prof. Penfield confirms this by another analysis of *argyrodite* conducted with an excellent specimen in his possession. The two minerals would thus appear to possess the same composition. They are not identical, however, for *argyrodite* crystallises in the monoclinic system. *Canfi ldite* crystallises in cubic octahedrons modified by dodecahedral faces; the crystals are black with a blue or purple sheen, they exhibit a magnificent metallic lustre and are extremely brittle. *Argyrodite* and *canfieldite* are therefore dimorphous forms of silver germanium sulphide.

NOTES from the Marine Biological Station, Plymouth.—Last week's captures include the Hydroid *Myriothela phrygia*, the semi-parasitic Rhabdocœle *Fecampia erythrocephala*, and the Mollusca *Favorinus albus* and *Rostanga coccinea*. The floating fauna has changed very slightly since last week, but several other autumn forms have made their appearance. Radiolaria have been present in fair numbers; the Anthomedusa *Podocoryne* (= *Dysmorphosa*) *carnea* has been plentiful, the majority possessing buds upon the manubrium; and the beautiful larvæ of the Prosobranch *Rissoa* and of the Opisthobranch *Ægirus punctilucens* have also been taken. The Turbellaria *Fecampia erythrocephala* and *Cycloporus papillosus*, and the Crustacean *Hyas coarctatus* are now breeding.

THE additions to the Zoological Society's Gardens during the past week include two Ruffed Lemurs (*Lemur varius*, ♂ ♀) from Madagascar, presented by Mrs. Brightwen; three Long-eared Owls (*Asio otus*) and one Tawny Owl (*Syrnium aluco*) from Europe, presented by Mr. Edmund Hart, F.Z.S.; a —— Falcon (*Falco* ——) from ——, presented by Lord Lilford, F.Z.S.; five shags (*Phalacrocorax graculus*) from Scotland, presented by the Maclaine of Lochbuie; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. E. Palmer; a Black-headed Caique (*Caica melanocephala*) from Demerara, deposited; a Regent Bird (*Sericulus melinus*) from Australia, purchased.

OUR ASTRONOMICAL COLUMN.

THE ORIGIN OF NEW STARS.—Prof. A. W. Bickerton writes to us from Christchurch, New Zealand, as follows:—"More than a year has elapsed since the first notice appeared of the new star in Aurigæ, and up to date no generally-accepted explanation of the special phenomena noted has been offered in any of the leading journals. May I beg to draw the attention of your readers to several articles bearing directly upon the mode of origin of new stars, published fifteen years ago in the Transactions of the N.Z. Institute? The explanation there offered appears to fit in almost exactly with the actual conditions as observed in this particular case. The papers referred to are contained in vols. 11, 12, and 13 of the Transactions of the N.Z. Institute. A summary of these papers also appears in the Proceedings of the Australasian Association for the Advancement of Science for the year 1891." Upon referring to the above references we find that Prof. Bickerton believes that new stars are caused by the "grazing" collision of stars like the sun. His researches show that "The temperature developed is independent of the amount of grazing. With similar substances it depends only on the velocity destroyed, so that the coalesced body produced by the merest graze must be as hot as though the whole sun collided. The molecular velocity of such a high temperature may be sufficient to carry away every particle entirely into space, the mass of the body not having sufficient attractive power to retain them. Hence an intensely brilliant body is produced in less than an hour; it then expands and increases in size and total luminosity for perhaps a few hours to a day or so; then the diffusion would be so great as to gradually lessen luminosity, until in a few months or a year the star would have disappeared into space. This represents all the peculiarities of temporary stars. If the graze be more considerable the attraction will be greater, yet the molecular velocity is the same: a hollow globe of gas may then result, giving us a planetary nebula. According to Lord Lindsay this is the condition of the temporary star in the Swan."

THE SPECTRUM OF THE RORDAME-QUÉNISSET COMET.—Prof. Campbell in *Astronomische Nachrichten*, No. 3177, gives a detailed account of the visual and photographic observations that he has made of the spectrum of this comet at the Lick Observatory. The following are the visual observations, and in the fourth column are given Kayser and Runge's wave-lengths for the edges of carbon bands.

July 11.	July 12.	July 17.	Carbon bands.	Description of bright lines and bands.
600	...601	... — ...	619 595	...Maximum of red band, broad, faint.
562	... —	... — ...	563	...Red edge of yellow band.
—	... —	...5633 ...	5635	...Very faint line terminating in yellow band.
—	.. —	... 558 ...	5585	...Bright line in yellow band.
5162·1	...5161·8	...5163·9 ...	5165·2	Very bright line terminating in green band.
5124	...5127	...5128 ...	5129	...Very bright line terminating in green band.
—	... —	... 509 ...	—	...Very bright line terminating in green band.
4734	.. —	... — ...	4737	...Red edge of blue band.
—	... —	...4734 ...	4737	..Bright line terminating blue band.
—	... 434	... — ...	—	...Bright region in continuous spectrum, faint.
—	... 421	... — ...	—	...Bright region in continuous spectrum, faint.

In addition Prof. Campbell has obtained two photographs of the comet-spectrum extending from wave-length 487 to 387. Twenty-eight bright lines have had their positions determined in the photographic spectrum, fourteen of which appear to correspond to lines and bands of carbon and cyanogen as given by Kayser and Runge. It is pointed out, however, that the wave-lengths of the comet-lines are systematically less than Kayser and Runge's by one or two tenth-metres. Prof. Campbell thinks this may in part be due to the fact that the cometary spectrum consists of unsymmetrical bands rather than lines, and partly to motion in the line of sight.

ATMOSPHERIC REFRACTION AND STAR PHOTOGRAPHS.—Now that stellar parallax is determined from photographic data, and a catalogue of stars is being prepared from the images impressed by celestial points upon sensitive films, it becomes necessary to investigate the effect of each and every cause tending to vitiate the results. Prof. A. A. Rambaut considers the most important of these disturbing causes in a paper on the distortion of photographic star images due to refraction read before the Royal Dublin Society on April 19, and just published in a separate form. Prof. Rambaut had previously published formulæ (*Astr. Nach.* 3125), by which the correction for refraction to the relative position of any two stars on a photographic plate can be computed in a convenient manner, and he has now followed these up by determining the distortion that takes place in the shape of a star-image during the exposure. His conclusion is that within the limits of an exposure of fifteen minutes' duration, "so long as the zenith distance does not exceed 60° no sensible error can arise through the distortion of a star by refraction if the measures are in all cases made from the centre of the image, and the coefficients in the formulæ of reduction are computed for the time corresponding to the middle

of the exposure, but if photographs obtained with longer exposure are utilised for the determination of the relative position of stars, it will be necessary to know what star on the plate was used as guider, and the distortion by refraction must be investigated for all stars at any considerable distance from it." From this it will be seen that the photographs of stars obtained for the determination of parallax, or in connection with the star catalogue, are unaffected by the result, since the exposure in each case is usually less than the limit defined by Prof. Rambaut.

ASTRONOMY POPULARISED.—We have previously referred to a proposal to issue a new astronomical periodical, designed for amateurs, teachers, students of astronomy, and the public generally. The first number of this *Popular Astronomy* will be published about September 1, by Mr. W. W. Payne, Goodsell Observatory, Carleton College, Northfield, Minn., U.S. Messrs. William Wesley and Son, 28 Essex Street, Strand, London, are the agents for England. The periodical will be issued monthly, but no numbers will be published for July and August of each year. One of the features will be a scheme of work suitable for a small telescope, field glass, opera glass, and the naked eye. Those who wish to know their way about the sky will find their wants supplied, and home readers will be catered for by means of lists of best books and schemes of study. From these and other matters mentioned in the prospectus it seems probable that the periodical will possess the features that command success.

COMET APPEARANCES IN THE YEAR 1892.—Prof. H. Kreutz has collected together all the appearances of comets during the past year, this list appearing in the *Vierteljahrschrift der Astronomischen Gesellschaft*, 28 jahrgang, parts 1 and 2. In addition to short descriptions of the appearances put on by them at the times of discovery, and to the values of the elements of the new ones, he gives references to all the observations that have been made of them. Among those that receive more than usual attention are Comet 1892 I., discovered by Swift; Comet Holmes (1892 III.), Winnecke's Comet (1892 IV.), and Comet 1892 V. (Barnard), since it was the first (excluding that photographed in the Solar Eclipse of May 17, 1882) discovered by photography.

GEOGRAPHICAL NOTES.

THE Society for the study of French Congo has organised a strongly-manned expedition to survey the valley of the Kuilu and Niadi rivers, in order to ascertain the feasibility of constructing a railway from the coast town of Loango to Brazzaville on Stanley Pool. A geological and botanical staff accompany the survey party, and the whole is under the command of M. A. Le Châtelier, who, with fifteen French members of the expedition, sailed from Marseilles last week.

RUSSIAN authorities are determined this year to test the capabilities of the Kara Sea route to Northern Siberia. A small fleet of three vessels, specially built on the Clyde for navigation on the Upper Yenesei, has recently set out in charge of Russian naval officers, who are confident of making a rapid journey. Capt. Wiggins is also in charge of some vessels laden with railway material for the great trans-Siberian line, which are now on their way to the Kara Sea. Dr. Nansen, in the *Fram*, must now be very near the entrance to the Kara Sea, and the nature of the ice there will determine which of the three routes into the sea will be attempted. The ultimate establishment of a commercial steamer service is only a matter of money.

IN a racy little pamphlet, *La Géographies dans les Chaires de l'Université*, Dr. Maurice Viguier makes a raid on a number of elementary text-books published by the leading geographical professors of Paris, and he succeeds in showing many errors of statement which should be set right. He goes on to argue that the inaccuracy of these popular schoolbooks, written to satisfy an arbitrary syllabus, proves the geographical incompetence of the authors. Few eminent men in any country could stand such a test, and in truth the faults cited and held up to ridicule so cleverly are faults of composition rather than of fact, and the worst are due to the ambiguity of words in common speech.

PRELIMINARY arrangements are being made for the meeting of the Sixth International Geographical Congress in London in 1895. This congress will be under the patronage of the Queen, and will bring together the geographers from all countries for the discussion of questions in which the international or universal side of geography will be kept to the front. The month of the proposed meeting has not yet been fixed.

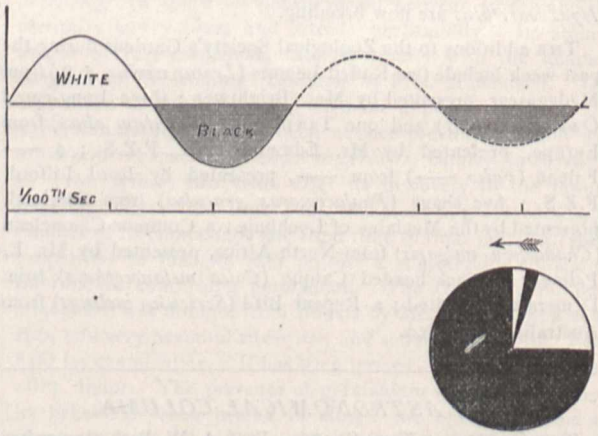
THE Central African telegraph line, projected by Mr. Cecil Rhodes, has been already commenced, and contracts have been signed for its construction from Fort Salisbury as far as Lake Nyasa. The wire will be carried on iron poles, and taken across the Zambesi (a distance of about half a mile) overhead at a height sufficient to allow the traffic on the river to pass entirely unimpeded. The advantage of this line in bringing the region of the great lakes into telegraphic touch with Europe will be very great.

CHARPENTIER'S EXPERIMENTS DEMONSTRATIVE OF AN OSCILLATORY PROCESS IN THE ORGAN OF VISION AND OF ITS DIMENSIONS.

ONE of the fundamental positions in Hering's physiological theory of visual sensation is that each sensificatory datum diffuses in the retino-cerebral organ beyond its precise locus of incidence, and thus directly modifies contiguous sensificatory data. A very elegant experimental substantiation of this position is contained in two simple optical observations by Charpentier[1] (of Nancy) giving not only the clearest possible demonstration of the fact itself, but an approximate measure of the physiological duration and velocity of the phenomenon.

These experiments are (1) that of the "black sector," demonstrative of a retino-cerebral oscillation; (2) that of the "fluted band," demonstrative of the propagation of that oscillation.

(1) *Charpentier's experiment of the Black Sector.*—A black disc with a white quadrant, revolving once in two seconds, illuminated by a very bright light (preferably direct sunlight). Observer's eye fixed upon centre of disc. A narrow black sector appears on the white quadrant near the receding edge of the black surface. This is interpretable as a rebound effect indicative of an oscillatory process in the retino-cerebral organ; the first effect at the arrival of the white border is the sensation of white, and this first effect is followed by an after-effect that is black. On closer examination it may be noticed that the angular breadth of the black sector is equal to the breadth of the white



interval between it and the receding black border, and that these breadths increase and diminish with increase and diminution in the speed of revolution; estimating from this speed and from the apparent extent and position of the black band, Charpentier finds that the white phase and the black phase have each a duration of ·014 to ·016 second, *i.e.* that a total oscillation of the two phases lasts ·028 to ·032 second, *i.e.* that the oscillation frequency is 36 to 31 per second.

Nothing can be clearer and more striking than this experiment; provided a strong light is used, it can be roughly demonstrated without any elaborate apparatus; it can be done by a black and white disc slowly turned round by hand, or by a

[1] *Comptes Rendus*, Soc. Biol., Mai 16, 23, 30, 1891. *Comptes Rendus*, Acad. Sc., Juillet 27, 1891. *Arch. de Physiol.*, Juillet et Octobre, 1892.

black and white card moved horizontally in front of the eyes. The estimates that I have made with proper apparatus very closely correspond with the value as originally determined by Charpentier. With a disc revolving once in two seconds, I find the apparent angular magnitudes of the two phases equal to about 2°·5; with a disc revolving twice as fast they are about 5°.

(2) *Charpentier's experiment of the Fluted Band* is somewhat more difficult of performance and of interpretation. A black disc, 45 cm. in diameter, revolving about twice per second, with a small white spot (1 cm. × ·5), 20 cm. from the centre. Observer's eye fixed upon a bead placed in front of the disc at that distance from the centre. Under these circumstances the white spot appears stretched out to a white band with indefinite beginning and end, which appears to be composed of several alternately lighter and darker portions of longer light internodes with shorter dark nodes. Whereas in the experiment of the black sector, the apparent angular magnitude *increases* with increased speed of revolution, in this experiment the angular magnitudes of the nodes and internodes *diminishes* with increased speed (or what amounts to the same thing, with approximation of the observer's eye to the disc) and *vice-versâ*.

Charpentier explains this at first sight very puzzling relation by the following hypothesis, which is at the same time an ingenious application of a well-known physical principle to a hypothetical physiological wave transmission *and a proof of the existence of the latter*. Upon the incidence of the stimulus white, an oscillation of sensation is produced, of which the first or positive phase is white, the second or negative phase black; each phase has a duration of about 0·015 sec.—*i.e.* the entire oscillation has a duration $t = 0{\cdot}03$ sec. and a frequency n of 33 per sec. This much is demonstrated by the experiment of the black sector. Let us now suppose that the oscillation spreads from its origin in the organ of vision[1] over the remainder of that organ, as an oscillation at one point of a pond spreads over the remainder of the pond. The problem is to determine the velocity of transmission v and the wave-length l of this presumably *propagated* oscillation. This is done by Charpentier by the following physiological application of Döppler's principle *re* the apparent modification of sound-waves according as the distance between origin and ear is increasing or diminishing.

In accordance with a familiar relation, wave-length l is equal to velocity v, multiplied by duration t, or $l = vt$. In accordance with Döppler's principle the apparent rise of tone or the apparent diminution of wave-length caused by the approximation of observer and wave origin, are such that $l' = (v - v')t$, where l' is the apparent wave-length, and v' the velocity of approximation.

If we were debarred from measuring tones proceeding from stationary origins, we might nevertheless determine their wave-length and velocity by calculation from measurements of the apparent wave-lengths of tones proceeding from origins moving at different known velocities. From two equations, $l' = (v - v')t$, $l'' = (v - v'')t$, we should have $t = \dfrac{l' - l''}{v'' - v'}$ and its reciprocal, $n = \dfrac{v'' - v'}{l' - l''}$ and (substituting $\dfrac{l}{v}$ for t) $v = \dfrac{l'v'' - l''v'}{l' - l''}$ and $l = \dfrac{l'v'' - l''v'}{v'' - v'}$.

These are, in fact, the data experimentally accessible in the retinal phenomenon. We cannot (as far as is known at present) measure the velocity and length of a retinal wave with stationary origin; we must determine these values from their apparent values with the wave-origin moving at different known velocities.

Practically the velocities v', v'', &c., of the wave origin on the retina are easily adjusted; the apparent wave-lengths l', l'', &c., more or less accurately observed. Given the dimensions of the disc, its distance from the eye and its rate of revolution, the experimental velocities are easily calculated; similarly if the apparent dimensions on the disc of the nodes and internodes are accurately observed, the retinal wave-lengths corresponding with them can be accurately calculated. It is in this second determination that the chief experimental error can arise; nevertheless, considering the original conditions of the problem and and that this is, in fact, the first time it has been approached and solved by any method, the results given by Charpentier are, within limits, sufficiently demonstrative of the propagation of a retinal oscillation and of its approximate velocity and wave-length. He finds from a large number of measurements a *velocity* between the limits of 53·8 and 90 mm. per sec. (mean value, 72); a frequency between 28 and 54 (mean value, 36); a calculated wave-length on the retina of 2 mm.; and a calculated wave-duration of 0·028 sec.

Not the least satisfactory feature of these figures is that the value of the wave-duration derived by the indirect method of this more difficult experiment, practically coincides with that derived from the simple and easy experiment of the black sector.

A third experiment of Charpentier's, although not precisely confirmatory of these, seems to stand in some relation to the negative semi-vibration manifested as the black sector. A black disc with open sectors, revolving between the eye and a white sheet illuminated by direct sunlight, gives rise to the sensation of a magnificent purple colour, when the rate of revolution is such that the eye receives between 40 and 60 stimuli per second, *i.e.* when each stimulus occurs during the negative phase of the preceding stimulus. Above 70 and below 30 stimuli per second the sheet appears white. The effect is very striking and very easily obtained; in short, it is a "ladies' experiment"; its full explanation is a different matter, and far too uncertain for discussion in a short article. A. D. W.

[1] It is essentially indifferent whether we take organ of vision to signify the retina or brain or retino-cerebral apparatus. It is convenient to refer measurements to the retina itself, and to determine retinal velocity and retinal wave-length.

THE POSITION OF SCIENTIFIC EXPERTS.

FROM time to time it has been pointed out in these columns that the services rendered to litigators as such by so-called scientific experts is antagonistic to the pure spirit that should actuate men of science. For some years the position and character of the representative of science in courts of justice has been acquiring interest, not only in England but elsewhere. In fact, a few years ago a Committee of the American Association for the Advancement of Science was appointed to consider the whole matter, but no report of their proceedings has yet been published. An excellent discussion of the subject, however, comes from America in the form of a reprinted lecture on "The Scientific Expert in Foreign Procedure," by Prof. C. F. Himes, which appears in the June number of the *Journal of the Franklin Institute*. In order to direct the discussion, Prof. Himes first gives legal opinions as to the status of the expert. "Justice Miller," he says, "exhibited a plan of objection in a charge as follows:—'My own experience, both in local courts and in the Supreme Court of the United States is, that when the matter in contest involves an immense sum in value, there is no difficulty in introducing any amount of expert testimony on either side.' Another judge, in a lecture upon medical expertism, gives a similar opinion, that the ground of dissatisfaction in regard to medical testimony to both the professions of law and medicine, are reducible to one—that upon every conceivable issue expert opinions are procurable which sustain, or seem to sustain, the most contradictory views." But Prof. Himes does not take a pessimistic view of the scientific expert. He is inclined to believe that:—

"The scientific expert is simply a product, and an extreme product, of an advanced and rapidly advancing civilisation. He was recognised in the germ, to be sure, by the old Roman law, and we may assume in all systems of jurisprudence; but he has acquired an immensely increased importance, and a much wider field and a far greater frequency of employment by the recent, and very recent, marvellous advances in the applications of science—applications which have increased the sphere of things to be litigated about, which have introduced facts of an entirely new character to be adjudicated upon, to say nothing of the contribution that science has made, and is continually making, in many ordinary cases, of conclusive missing links of evidence which render decision previously uncertain, comfortably certain, and satisfactory.

"Now, one fact that seems latent in these expressions of the legal profession in regard to the scientific expert and almost the first that impresses is that in many respects he seems to be a positive annoyance to lawyers, and even to judges at times—a sort of intractable, incompatible, inharmonious factor, disturbing the otherwise smooth current of legal procedure; too important or necessary to be ruled out, too intelligent and disciplined mentally to yield without reason to ordinary rules

and regulations of the court, with which he may not be familiar, and, at the same time, possessing an undoubted influence with a jury, that it is difficult to restrict by the established rules and maxims of legal procedure."

After a consideration of the circumstances that shape the reputation of the scientific expert with the bar, bench, and laity we read :—"In considering some of the sources of dissatisfaction with the scientific experts, perhaps one of the first to suggest itself, and one of the most prolific, is the vagueness of the legal definition of the term 'scientific expert' before alluded to, but which on more careful consideration might rather be termed vagueness and variableness of the standard. Definitions of things are of ideals, and consequently definition is followed closely by the statement that the thing defined is non-existent. The ideal circle is defined, so the ideal solid, the ideal liquid ; these definitions are only approached, never realised. Degrees of approach constitute the differences. Practically the courts are limited to the best experts extant in any field, though they may at times fall far short of the ideal. But it is to be feared that in many cases the experts fall below a reasonable and possible standard, and far below the standard that would be fixed by scientific men themselves, as well as below the exigencies of the case. This may easily be accounted for. A party presents a witness as an expert. The judge must pass upon his competency upon such examination as he can make. That decision, though not necessarily, nor even by unvarying practice, a matter of discretion, will not often be reviewed by a superior court. Often, then, the best solution, certainly the easiest, seems to be to admit, even where there may be grave doubt as to qualification, and to throw the burden upon the jury, already overburdened with questions, which the theory of trial by jury assigns them, questions which they are not qualified to deal with, although they may be fully up to the average in general intelligence. At a time when experts were not much beyond men in the ordinary avocations of life it may have been reasonable to require the jury to pass upon the 'weight and credit to be given to evidence viewed in connection with all the circumstances,' but under the changed circumstances of to-day, with experts of a character, and upon questions not dreamed of even a century ago, it seems to be straining a theory too far to put upon an average jury the decision of so grave a question, as to the character of the expert, which the court may not be able to settle satisfactorily. But for the theory it would not be thought of, if a system of jurisprudence were now being devised. Now among the results incidental to a liberal interpretation of the term by the courts are many that are regarded as the gravest evils of expert testimony. With doors wide open to incompetent persons, very slight pecuniary advantage, and still more frequently the incidental benefit attributed to notoriety and advertisement would cause them to seek entrance. As a result differences of opinion may be anticipated where knowledge is wanting as a basis. Then, too, the number of such experts in any case will be greater. The cross-examination absolutely necessary to test such evidence must be exhaustive and tedious. Trials are prolonged. The expense of the administration of justice is increased without furthering its ends, and withal often with incidental discredit not only of the testimony of experts, but in a measure of the whole judicial procedure which is responsible for them ; and the jury are often left in such a state of mental confusion that the evidence can only be weighed by counting the experts. Now the rule should tend toward a greater strictness in regard to the qualifications of experts, since the progress of science tends towards a greater degree of specialisation in study, and consequently to more minute and extended evidence on the whole, with greater restrictions on the range of best evidence of any particular expert. If science stood still, or if forensic science was confined at all times to the same old ground, everything would be settled, but as it is, the new points at issue continually arising make new demands upon experts, which there may be few at first qualified to meet. The introduction of advanced scientific expert testimony is then hardly a matter of option. It is forced upon the courts by the fact that science is just as ready in the hands of the unscrupulous and dishonest to perpetrate the most flagrant wrongs as to aid in their detection, and that there is no advance in science that is not as accessible to the enemies of society as well as to society itself.

"But another, even more prolific source of complaint than laxity of rule in the admission of experts, lies in the anomalous position of the expert in many respects, and under the best circumstances. He is legally a witness, an ordinary witness, but practically with extraordinary functions and one loaded with extraordinary responsibilities, and one might add, frequently loaded with extraordinary, and even absurd, expectations. As a witness he is subpœnaed by the same form, obliged to respond under the same penalties, to take the same oath ; is subject to the same rules and restrictions, and the same treatment in court. He has no higher claim upon the State, or upon the parties for his time or his private professional knowledge, which constitutes his livelihood. He receives, in most cases, to be sure, from the party calling him, a fee agreed upon between them, and certainly out of proportion to those of other witnesses, even if it is not professional in magnitude. He assists the side on which he is called in working up its case. He suggests the cross-examination of witnesses. He thus exhibits the character of a very willing witness, of a well-paid witness, combined with a great deal of the advocate. Now he cannot be held responsible for this position, but the system of jurisprudence, which not simply permits it, which has not simply taken him, but has forced him in, and which, apparently cognizant of all, seems only able to originate complaints, rather than to provide a different character for him ; for there seems, indeed, in many of the adverse criticisms of experts, to be only a confession of weakness, rather than a disposition earnestly to consider the whole question with a view to the radical remedy of the evils. The human nature of the judge is recognised and provided against. Every safeguard is thrown around him to protect him from bias, or possible suspicion of bias, which would be almost as bad. The jury is selected so as to be free from bias, and is protected as well. Other witnesses are not expected to take the part the scientific expert is almost compelled to take. In fact, if deliberately planned, there could hardly be a network of conditions devised, calculated to produce so many of the evils of scientific expert testimony complained of, or to cloud this testimony of highest intrinsic value, having the highest degree of certainty, and in a field altogether its own."

"But in regard to the charge of bias," Prof. Himes afterwards goes on to say, "it may be admitted that the scientific expert may at times be biased, but that is only admitting that he is made of the same clay as other men. The bias, if not produced by the call, would certainly not be more of a reflection on his character than upon the system of jurisprudence which renders a call based upon bias not only possible, but almost necessary, and which provides no other method for the introduction of scientific testimony. But bias may be in nowise incidental to the call. It may be a purely scientific bias, due to some peculiar view or theory. No kind of training will fortify a man against bias at all points. In his laboratory, in conducting his investigations, the scientific expert may keep himself free from bias. The judge upon the bench is free from bias by habit, rather than by conscious effort. But even the judge, placed in some novel position of great responsibility, which this judicial habit does not fit exactly might lapse into a bias. . . .

"The criticism due to differences of opinion frequently exhibited by scientific experts can hardly be regarded as a serious matter by a profession characterised by differences of opinions on all conceivable points ; the only settled opinions known to it being those of the court of last resort, which even claims the privilege occasionally of reversing itself. Differences of opinion among scientific experts are often doubtless due to differences in scientific character, resulting from the loose rule of admission. But there may still be honest differences between experts of highest character. I think such, however, it will be found, are rarely in regard to well-established facts, but oftener in regard to probable inferences from facts, whilst entire agreement would be marvellous in matters of theory and speculation. Courts and attorneys do not discriminate sufficiently between well-established scientific facts and scientific theories. Some of the most recent and far-reaching decisions of our highest tribunals have a basis of theory rather than of fact."

This leads to a point which we have always insisted upon, namely, that a scientific expert should not be called and subsidised by a particular side, but should be appointed by the judge or jury. To quote Prof. Himes :—

"Many of the most objectionable features of the expert witness originate in the mode of his entrance into court, and it is an allowable question, whether any modification could be made in the calling of the witness. Among the reports one judge ex-

presses the opinion that, 'expert witnesses ought to be selected by the court, and should be impartial as well as learned and skilful. A contrary practice, however, is now probably too well established to allow the more salutary rule to be enforced.' Another judge suggests that the law should be so changed 'that this class of witnesses should be selected by the court, and that this should be done wholly independent of any nomination, recommendation, or interference of the parties, as much so to all intents as are the jurors.' This would not make experts *amici curiæ* any more than before, for all witnesses should be regarded in that light, but it would be a provision rather to preserve that character to them, coupled as it is with a recommendation as to compensation, so intimately connected with it. It is not the fact of extra compensation, or that the compensation is paid by the party benefited by his testimony, that creates the unfavourable impression. The other witnesses are friends of the court, by whatever party they may be called, they stand upon the same footing as to pay; but here is a witness who is paid according to a private agreement, by one of the parties; the amount is their own private arrangement on which the court is not consulted, over which the court has no control, a circumstance that imparts to him, in high degree, the character of a friend of one of the parties; and these facts as to compensation are often elicited at a time, and in a way, calculated to impair otherwise valuable testimony in the minds of the jury.

"By far the best plan seems to be that adopted in the Imperial Courts of Germany. For certain matters and lines of business permanent experts are appointed by the State, but they are not regarded as officers, but as *employés* for the time being. They have no official title, nor regular salary. The payment they receive is not enough to support them, but barely compensates them for their loss of time. For most cases the expert is appointed by the particular judge in the case, often on the demand of one or the other or both parties, but the choice of the expert lies within the discretion of the judge. He may appoint any man whom both parties suggested, or may also appoint a third man not suggested by either, but if both parties unite on one man he must listen to his testimony. If a question is involved for which regular legal experts are provided, these need only be or can be appointed. The qualifications for such a regular expert are that he should follow that particular profession or line of business habitually, and for the purpose of earning his living. The number of experts in a case is not limited by law, it rests with the discretion of the judge. The status of the expert in court is almost analogous to other witnesses, but it is not a civic duty, as with witnesses, to give evidence in court except where a profession is followed publicly and for a livelihood. The text of his oath before giving testimony is different from that of an ordinary witness; and he need not be sworn at all if both parties unite in dispensing with such qualification."

If a similar system were followed in England the testimony of scientific experts would be regarded with a little less suspicion than it is at present. Only by some such means can technical evidence of a wholly disinterested character be obtained.

SCIENCE CLASSES IN CONNECTION WITH THE LONDON COUNTY COUNCIL.

THE Technical Education Board of the London County Council has issued a series of Regulations with regard to the administration of grants to science classes. All the prescribed conditions tend to make the instruction efficient and develop technical education in the right direction. The following are those that refer to the manner in which various classes must be conducted:—

(1) That as a condition of aid being granted by the Board for the teaching of chemistry, physics, mechanics, and botany, it will be regarded as indispensable that provision should be made, to the satisfaction of the Board, not only for the experimental illustration of the lectures or class teaching, but for experimental work by the students themselves, either in laboratories belonging to the institution or, where this cannot be arranged, in the laboratories of some neighbouring institution with which the class should be associated; and every lecture must be followed by at least one hour's practical work on the same evening, or some other evening in the same week.

(2) That with regard to classes in the subjects comprised in the Science and Art Department Directory which are more strictly to be included under the head of technology, viz. building construction and drawing, machine construction and drawing, steam and the steam-engine, navigation and naval architecture, it be required, as a rule, that such classes be taught by teachers having a practical acquaintance with the industries to which they refer; provided that, in the case of teachers who have already successfully taught such classes, it shall be open to the Board, on being satisfied of the sufficiency of the qualifications, to make exceptions in particular cases. No grant will be given for classes in agriculture or mining.

(3) That for classes in geology and mineralogy suitable museum specimens be provided and examined by the pupils, and for classes in machine drawing a suitable collection of models and parts of actual machines be provided.

(4) That in the teaching of mathematics, practical geometry, building construction, machine drawing, naval architecture, navigation and nautical astronomy, "home work" be made an important feature, and that the students' work be examined and corrected by the teacher out of class hours.

(5) That in all practical laboratory classes, and in classes on mathematics, practical geometry, building construction, machine drawing, naval architecture, navigation and nautical astronomy, not more than twenty students shall be under the charge of one teacher at the same time, but where more than one teacher is present during the whole meeting of the class the number of students may be increased in proportion to the number of teachers.

(6) That in all subjects there be a sufficient supply of apparatus and materials for efficient teaching, and that such apparatus and materials be effectively used.

(7) That no payment be made on account of pupils who, in the opinion of the Board, may not reasonably be expected to profit by the teaching provided (*e.g.* pupils in navigation or nautical astronomy, or in the advanced stage of theoretical or applied mechanics who have insufficient knowledge of mathematics; those in building construction or machine drawing who have no knowledge of elementary mechanics, &c.).

The Board is prepared to consider applications for assistance to erect laboratories and provide the necessary equipment. It will also make grants in aid of the purchase of apparatus for science teaching. With so many advantages, technical education in the administrative county of London should grow apace.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE following is the list of candidates successful in the competition for the Whitworth Scholarships and Exhibitions, 1893:—Scholarships (tenable for three years, having an annual value of £125):—William Hamilton (Glasgow), John G. Longbottom (Keighley), Arthur E. Malpas (London), Richard J. Durley (London); Exhibitions (tenable for one year, having a value of £50):—Charles F. Smith (Glasgow), John Ball (Derby), William Buchan (Glasgow), John B. Chambers (London), Henry J. Loveridge (Southsea, Portsmouth), William F. Ireland (Glasgow), George W. Fearnley (Shipley), Oliver Styles (Edinburgh), George M. Russell (Portsmouth), Alexander A. Jude (Hull), Edward R. Amor (Devonport), Joseph Jeffery (Birmingham), Paul J. Reynolds (Plumstead, Kent), Thomas Pilkington (London), Richard Reynolds (Cardiff), George Wilson (Sheffield), Walter O. Hammant (Plumstead, Kent), John Orr (Airdrie), William I. Chubb (London), Henry Smith (Brighton), Frederick D. Green (Wanstead, Essex), John Powell (Crewe), James H. Hardy (Woodley, near Stockport), James H. Shepherd (Swindon), Herbert Thompson (Sheffield), Evan Stevens (Swindon), Henry E. Morrall (Wolverton), Herbert Bates (Manchester), Charles H. Hill (Stratford, London), William F. Massey (Newport, Salop).

The Scholarships Committee of the 1851 Exhibition Science Scholarships has issued a list of appointments for 1893. Four scholarships awarded in 1891 have been renewed for a third year in order to permit the holders to complete their investigations. These scholars are James H. Gray, John Joseph Sudborough, Harry Ingle, and Thomas Ewan. The following scholars of 1892 have had their scholarships renewed for a second year:—Andrew John Herbertson, James Blacklock Henderson, John Macdonald, Lionel Simeon Marks, George Lester Thomas, Harold Hart Mann, James Terence Conroy,

Thornton Charles Lamb, Edward Arnold Medley, William Henry Oates, William Gannon, Frederick J. Smale, Samuel Henry Barraclough, David Hamilton Jackson, Edward Taylor Jones, James Bernard Allen. The list of science scholars of 1893 is as follows:—Herbert William Bolam, George Edwin Allan, James Wallace Walker, Arthur Lapworth, John Ellis Myers, Arthur Walsh Titherley, Edward Chester Cyril Baly, John Cannell Cain, Ella Mary Bryant, James Darnell Granger, Mary O'Brien, Frederick George Donnan, James Alexander MacPhail, Norman Ross Carmichael, Wm. Henry Ledger, George Wm. Macdonald. The institutions to be invited to nominate science scholars for 1894 are:—the University of Edinburgh, the University of Glasgow, the University of Aberdeen, Mason College, Birmingham, University College, Bristol, Yorkshire College, Leeds, University College, Liverpool, University College, London, Owens College, Manchester, Durham College of Science, Newcastle, University College, Nottingham, Firth College, Sheffield, University College of South Wales, Cardiff, Queen's College, Cork, Queen's College, Galway, the University of Toronto, Dalhousie University, Halifax, Nova Scotia, the University of Adelaide, and the University of New Zealand.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 7.—M. Lœwy in the chair.—On the periodic maxima of spectra, by M. Aymonet.—On the heat spectrum of fluorine, by M. E. Carvallo. A comparison between the results obtained by the author and simultaneously by Messrs. Rubens and Snow, of Berlin. In those portions which are common to all three observers, the agreement is perfect, although the results were arrived at by very different methods.—On the absorption of light by liquid bromine, by M. Charles Camichel. Liquid bromine absorbs luminous rays very energetically, especially the most refrangible ones. Thus, a thickness of bromine of a wave length and a half of D light exerts a considerable absorptive action upon the green ray of thallium, and a layer of six times that thickness absorbs the same radiation to such an extent that measurements become difficult. A drop of bromine was introduced between two pieces of glass constructed for observing Newton's rings. These glasses were mounted in a screw frame resting upon the carriage of a dividing engine, by means of which they could be moved in front of one of the collimators of a Gouy spectrophotometer. The thickness of the layer was measured by observing Newton's rings in monochromatic light. Two luminous pencils proceeded from the same source, one traversing the polarising collimator, the other the bromine glasses and then the ordinary collimator. Two patches were thus produced, which were equalised by the analyser when the bromine glasses were full and empty respectively. It was found that the absorption followed the exponential law between thicknesses of 0·5 and sixty times the principal wave length of sodium.—On the origin of atmospheric oxygen, by Mr. T. L. Phipson. Various plants, such as *Poa*, *Trifolium*, *Antirrhinum*, and *Convolvulus* were placed under glass shades with their roots immersed in water containing free carbonic acid and certain salts, shut off from the light, and their upper portions exposed to a north light in atmospheres of carbonic acid, hydrogen, and nitrogen respectively. It was found that in carbonic acid the plants were able to live for some time, but did not prosper. In hydrogen they fared better, but the gas gradually disappeared, probably combining with the oxygen evolved by the plants. *Convolvulus* throve very well in an atmosphere of nitrogen, especially if mixed with a third part of carbonic acid. After several weeks the composition of the gas began to approach that of our atmosphere, no change of volume having taken place. The bearing of these facts upon the history of the earth's atmosphere may prove important.—Of the isomorphism of anhydrous alums, by M. T. Klobb.—Influence of solar radiation upon plants, by M. G. Landel. Variations of intensity of solar radiation appear always to act in the same sense upon plants, as regards the quantity of flowers and the proportion of red pigment colouring the various parts. These variations differ much according to the species. In some the red pigment is well developed in the shade, whilst others remain perfectly green. The inflorescence in certain species does not seem to be sensibly modified by shade; in others the number of flowers is less.—The young bulbs of the *Dioscoreæ*, by M. C. Queva.

BOOKS AND PAMPHLETS RECEIVED.

BOOKS.—Mathématiques et Mathématiciens; Pensées et Curiosités deux edition: A. Rebière (Paris, Nony).—Solutions of the Exercises in Taylor's Euclid, Books 1 to 4: W. W. Taylor (Cambridge University Press).—A Treatise on the Mathematical Theory of Elasticity, Vol. 2: A. E. H. Love (Cambridge University Press).—A History of the Theory of Elasticity and of the Strength of Materials, Vol. 2, Parts 1 and 2: the late I. Todhunter, edited and completed by K. Pearson (Cambridge University Press).—British Rainfall, 1892: G. J. Symons and H. S. Wallis (Stanford).—Birds in a Village: W. H. Hudson (Chapman and Hall).—Pocket-book of Useful Formulæ and Memoranda for Civil and Mechanical Engineers, 23rd edition: Sir G. L. Molesworth and R. B. Molesworth (Spon).—Report of the Fourth Meeting of the Australasian Association, held at Hobart in January, 1892 (Sydney).—Royal University, Ireland, Examination Papers, 1892 (Dublin, Thom).—Griffin's Electrical Engineer's Price Book: H. J. Dowsing (Griffin).—Les Turbines: G. Lavergne (Paris, Gauthier-Villars).—Fourth Report of the Department of Science and Art (Eyre and Spottiswoode).—Electric Lighting and Power Distribution: W. P. Maycock, Parts 2 and 3 (Whittaker).—Geology, an Elementary Hand-book: A. J. Jukes-Browne (Whittaker).—Electricity and Magnetism: S. R. Bottone (Whittaker).

PAMPHLETS.—U.S. Department of Agriculture, Report of the Chief of the Weather Bureau for 1892: M. W. Harrington (Washington).—Catalogue of the Crabs of the Family Maiidæ in the U.S. National Museum: M. J. Rathbun (Washington).—The Planet Venus: E. M. Clerke (Witherby).—Notes on the Trunk Skeleton of a Hybrid Grouse: R. W. Shufeldt.—Report upon the Scott-Moncrieff System for the Bacteriological Purification of Sewage: A. C. Houston (Waterlow).—On the Distortion of Photographic Star Images due to Refraction: Prof. Rambaut (Dublin).—A Preliminary Report on the Aquatic Invertebrate Fauna of the Yellowstone Natural Park, &c., &c.: S. A. Forbes (Washington).—Notes on a Few Fossil Plants from the Fort Union Group of Montana: F. H. Knowlton (Washington).

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