THURSDAY, DECEMBER 28, 1882

MATHEMATICS IN AMERICA

American Journal of Mathematics, Pure and Applied. Published under the Auspices of the Johns Hopkins University. Vols. III. and IV. (Baltimore: Isaac Friedenwald.) And other Mathematical Journals.

THE *American Journal of Mathematics* was established in 1878 under the auspices of the Johns Hopkins University at Baltimore, and four handsome quarto volumes of 400 pages each have now been published. Prof. Sylvester was editor-in-chief of the first three volumes, being assisted by Mr. Story as editor-in-charge, but the last volume bears Sylvester's name alone as editor.

A notice of the first two volumes of the Journal appeared in NATURE, vol. xxii. p. 73, and the hope was there expressed that it might have as great a future before it as awaited Crelle's Journal half a century before. A careful examination of the last two volumes shows that the promise of the earlier volumes has been so far maintained, and that the Journal has already acquired a distinctive character of its own. It almost invariably happens that mathematical journals exhibit marked characteristics, and that certain branches of the subject occupy a pre-eminent position. One paper leads to another relating to the same questions, and the original bias of a journal is generally due, both directly and indirectly, to its editor, as authors naturally prefer to send contributions where they are more likely to be understood and appreciated. That this is especially the case with the American Journal is what we should expect, as besides being the principal contributor, the editor is professor in the institution with which it is connected, and many of the papers are by his former pupils and colleagues. Although a very distinct tendency is thus evident in the direction of the large group of subjects (and more particularly Higher Algebra and Higher Arithmetic) with which the name of Prof. Sylvester is associated, it is not to be supposed that the Journal has become narrow in its scope. On the contrary, the whole range of mathematical subjects is very fairly represented, as will appear from the following paragraphs, which contain a list of the papers in vols. iii. and iv., an attempt being made to group them to some extent according to subjects.

The arithmological papers are numerous. Prof. Sylvester gives closer limits for a quantity which occurs in Tchebycheff's well-known investigation of the number of primes inferior to any given prime; he contributes also a note on the trisection and quartisection of the roots of unity, and an instantaneous proof of a theorem of Lagrange's on the divisors of a certain quadratic form. In a paper on a point in the theory of vulgar fractions he gives a method of developing any vulgar fraction as a sum of certain special fractions, each having unity as its numerator. This development he terms a sorites, and he remarks that it was suggested to him by the chapter in Cantor's *Geschichte der Mathematik*, which gives an account of the singular method in use among the ancient Egyptians for working with fractions: it was their curious custom to resolve every fraction into a sum of simple fractions according to a certain traditional method, which however only leads in a few simple cases to a sorites. There are two papers by Mr. O. H. Mitchell, both relating to the theory of congruences: one of them contains a generalisation of Fermat's and Wilson's theorems. There is also a short note by Prof. Newcomb on the relative frequency of the occurrence of the digits as leading figures in logarithmic tables.

The contributions to the higher algebra occupy a very conspicuous place. The important tables of the generating functions and ground forms of binary quantics which have been calculated by Prof. Sylvester and Mr. F. Franklin, with the aid of a grant from the British Association, are continued. Mr. Franklin is also the author of a separate paper, in which he gives a consecutive account of the methods, due to Cayley and Sylvester, of calculating the generating functions for binary quantics and thence determining the number of fundamental invariants and covariants of any order and degree. Prof. Sylvester gives a determination of the impossibility of the binary octavic possessing any ground form of degree 10 and order 4. There is a paper on the 34 concomitants of the ternary cubic by Prof. Cayley, who also gives a specimen of a literal table for binary quantics and certain tables for the binary sextic; and there are some notes on Modern Algebra by M. Faà de Bruno, of Turin. Mr. Mitchell and Mr. T. Muir, of Glasgow, give theorems relating to determinants.

Prof. Wm. Woolsey Johnson is the author of a paper on strophoids. The term strophoid has been applied by French writers to a cubic curve, the symmetrical form of which Dr. Booth discussed under the name of the Logocyclic curve. The author gives to the term a more extended signification, and defines a strophoid as the locus of the intersection of two straight lines which rotate uniformly about two fixed points in a plane. Dr. Booth's curve is included as a particular case of the class of curves which Prof. Johnson terms right strophoids. Prof. Sylvester considers the theory of rational derivation on a cubic, and Mr. Story is thus led to discuss the subject more fully in a separate memoir: the points on the curve which are considered are those whose coordinates can be expressed as rational functions of an arbitrary initial point on the curve. Mr. Samuel Roberts contributes a paper on the generalisation of local theorems, in which the generating point divides a variable linear segment in a constant ratio; and there is a note by Miss Christine Ladd on segments made on lines by curves.

There are three papers on solid geometry, all by Mr. T. Craig: they relate to the orthomorphic projection of an ellipsoid upon a sphere, to certain metrical properties of surfaces (in n dimensions as well as in three dimensions), and to the counter-pedal surface of an ellipsoid. The surface which the author designates the counter-pedal is the locus of the intersections of central planes parallel to the tangent planes of the ellipsoid with the normals at the corresponding points of contact; its equation is worked out, and is found to be of the tenth order. Mr. E. W. Hyde contributes a note on the centre of gravity of a solid of revolution, and there is a discussion by Prof. Stringham on the regular figures in n-dimensional space.

Prof. Cayley gives a note on the analytical forms or

ramifications which he has termed trees. This is a subject which has applications to the theory of chemical combinations, and it is one to which Prof. Cayley has already devoted attention, a long memoir of his upon it having appeared in the Report of the British Association for 1875. Prof. Cayley also is the author of a short paper on certain imaginaries connected with the product of two sums of eight squares. Among papers on general analysis should also be mentioned a determination by Prof. Stringham, of the number of possible finite groups of quaternions, a group being defined as in the theory of substitutions; and a note by Mr. Story on non-Euclidean geometry.

A number of papers relate to the differential calculus. There are two by Prof. Sylvester on the solution of classes of difference and differential equations. No less than four relate to development in series by Taylor's and other expansion theorems and the forms of the remainder: these are by Mr. J. C. Glashan of Ottawa, Mr. McClintock, and Mr. A. W. Whitcom. Prof. Crofton gives some remarkable theorems involving symbols of operation, and Mr. J. Hammond considers the theory of general differentiation, a subject that received attention from Liouville, Peacock, and others half a century ago, but which has attracted but little notice in recent times. Mr. Franklin contributes a short note on Newton's method of approximating to the roots of equations, and Mr. Glashan gives certain formulæ relating to the change of the independent variable in differentiations.

More than one whole number is devoted to a reprint of the late Prof. Benjamin Peirce's valuable memoir on Linear Associative Algebra, of which only a small number of copies in lithograph were made in the author's lifetime for circulation among his friends. This well-known paper was read before the National Academy of Sciences at Washington in 1870: it is here reproduced with notes and addenda by Mr. C. S. Peirce, the son of the author, and occupies 133 pages. Mr. C. S. Peirce himself, who is known not only by his logical writings but by his stellar photometric researches, is also the author of two papers, the one on the algebra of logic and the other on the logic of number. In connection with this subject a paper by Miss Ladd on De Morgan's extension of the algebraical processes should be noticed.

There is a short bibliographical paper relating to Alhazen's problem by Mr. Marcus Baker. The problem is from two points in the plane of a circle to draw lines meeting at a point on the circumference, and making equal angles with the tangent at that point. The author also gives an extension of the problem to the sphere.

Only three papers belong to mathematical physics. One, by Prof. H. A. Rowland, relates to the general equations of electromagnetic action, with application to a new theory of magnetic attractions and to the theory of the magnetic relation of the plane of polarisation of light. The other two are on hydrodynamics, they are by Prof. Rowland and Mr. Craig, and relate respectively to the motion of a perfect incompressible fluid and to certain possible cases of steady motion in a viscous fluid. There are some notes on moving axes by Prof. Loudon of Toronto; and Prof. Sylvester considers the theory of mechanical involution, which is the name he has given to the relation between six lines in space so situated that forces may be made to act along them whose statical sum is zero. Linkages form the subject of a paper by Mr. F. T. Freeland.

Astronomy is represented by two papers: one by Prof. Newcomb, on a method of developing the perturbative function of planetary motion; and the other by Mr. G. W. Hill, on Hansen's general formulæ for perturbations. The object of the former paper is to exhibit a method of effecting the development in powers of the eccentricities. The author remarks that in consequence of the complex character of the series this development has been but little used even in the cases of nearly circular orbits, when its application would be most convenient, but that, as the disturbing force is given as an explicit function of all the elements, it is of more interest to the mathematician than any other. In his method of development Prof. Newcomb directs especial attention to the expression of the coefficient of each power of the eccentricity in terms of the coefficients of lower powers, and to the expression of the coefficient in each term involving the perihelia of two planets as the symbolic product of coefficients involving the perihelion of one only. Mr. Hill's paper contains a transformed form of Hansen's expression for the perturbation of the mean anomaly, which is more simple and more convenient for computation. There is also a short note by Mr. Ormond Stone relating to formulæ in elliptic motion.

The titles of the papers speak for themselves, and but little comment is needed. It will be seen that the two volumes represent a considerable amount of mathematical work, a fair proportion of which may have real influence on the advancement of the science. Some of the papers, as must evidently be the case, are needlessly pretentious in form, and the new matter they contain might be advantageously stated in less space. The effect of Professor Cayley's visit to Baltimore is apparent in the papers which occur in the last number issued, and we believe that the lectures which he gave at the Johns Hopkins University will shortly appear in a future number.

The dates which the numbers of the Journal bear are the dates when they ought to have appeared, assuming it to be published quarterly in March, June, September, and December, and not the dates when they did actually appear. Thus the last number issued bears date December, 1881, but in the case of this number the inconvenience attending so great a discrepancy between the nominal date and the date of publication is partially remedied by the words "Issued July 18, 1882," at the foot of the last page. It seems a pity to retain the nominal dates on the wrappers, as they may be misleading. It will be difficult to regain the lost time, and there is but little advantage in stating the time when the number should have appeared. The volume and number and date of publication are all that need to be given.

On the wrappers of the numbers of vol. iv. appears an announcement in which a prize of 1500 francs and a perpetual free subscription to the journal from its commencement are offered to the first person who, before January 1, 1883, discovers and transmits to the Editor a valid proof or disproof of the proposition that a ground form and a syzygant of the same degree and order cannot appertain

to the same binary quantic, "provided that the Editor shall not himself have previously discovered the same, and given public notice thereof." The truth of this proposition has been assumed as a fundamental postulate in the calculation of ground forms, and its importance cannot be over-estimated. It is, however, somewhat of an anachronism to draw attention to it by the offer of a prize. Such prizes exist in Universities and in the older academies, but by many they are not regarded with much favour. It seems unlikely that any competent person would be tempted to investigate the subject by hope of the reward. Pure mathematics offers no mercenary inducements to its followers, who are attracted to it by the importance and beauty of the truths it contains; and the complete absence of any material advantage to be gained by means of it, adds perhaps even another charm to its study.

The late Prof. Benjamin Peirce denoted the base of the Napierian logarithms and the ratio of the circumference to the diameter of a circle by two special symbols turned opposite ways, somewhat resembling a 6 and a 6 reversed. The forms of these symbols would seem to imply that 2·71828... and 3·14159... were regarded as allied to one another, and in some reciprocal or inverse manner too, though it is not easy to see what the author's point of view was. Two writers in the *American Journal* use Prof. Peirce's symbols in place of π and e, and this is to be regretted, as any departure from the recognised notation in elementary matters is always unfortunate. Even if the symbols were happily chosen, which does not appear to be the case, they would require the cutting of new type, and it is absolutely certain that there is not the least chance of their general adoption. If now used by a few prominent writers in America, they may spread to such an extent as to make it very difficult for their successors to get rid of them. The preservation of the international character of mathematical notation is of paramount importance, and the existence of local notations, especially when they find their way into text-books, is a calamity. In England the Cambridge notations, $\sin^{-1}x$, due we believe to Herschel and Babbage, and the factorial notation due to the late Prof. Jarrett, are still retained by many English writers, although it has long been evident that there is no chance of their adoption by continental mathematicians. It is always desirable to adhere to an established notation, if it is generally understood and accepted, even if it is unsatisfactory, rather than attempt to replace it by a better one, unless there seems very good reason to suppose that the attempt will be successful.

In the previous article in NATURE reference was made to the services which Dr. J. E. Hendricks, of Des Moines, Iowa, has rendered to mathematics in America by the publication of the *Analyst*, which he established in 1874, and has continued to the present time. This journal is published every two months, and has now completed its ninth volume. In spite of typographical and other difficulties the editor has published it regularly, and it shows no signs of diminished vitality or interest on his part. It has been self-supporting, and its success is due to the genuine love of their subject felt by the editor and the contributors. A great part of each number is unfortunately devoted to problems—the lowest form of mathematics—and the space available for more valuable matter is thus considerably diminished. One also is tempted to wish that the editor would show greater strictness in curtailing or excluding the writings of certain contributors, but nevertheless the *Analyst* contains not a few useful papers. It is easy to see the blemishes in such a journal by merely turning over the pages, but it is not so easy to estimate the services which it confers upon science by inducing teachers to look beyond the text-books and interest themselves in a subject for which a genuine taste can only be acquired by attempting to do work for oneself. The large quarto page of the *American Journal* and the elaborate nature of some of its papers render it unsuitable for the short notes and the more unpretentious class of papers in which the author lays but little claim to originality. For these the *Analyst* is available; but, after all, the chief value of such a publication consists in the interest in mathematics it excites and fosters in those who could be reached in no other way, and the inducement it affords for those who are unable to devote their whole time to the subject to nevertheless undertake useful and profitable work. No previous American mathematical journal has ever been published regularly for nine years, and Dr. Hendrick has reason to feel proud of the success of his efforts.

In 1877 Mr. Artemas Martin, of Erie, Pennsylvania, issued the first number of the *Mathematical Visitor*, a quarto journal which was published annually until Jan., 1880, and since then has appeared semi-annually. The first volume ended with the number published in January, 1881. The journal consists entirely of problems and solutions, there being a senior and a junior department. Several of the problems relate to probability questions and involve very complicated and elaborate integrations. As the solution of a prize question, Mr. E. B. Seitz gives, in the number for January, 1879, the values of the coefficients obtained by reverting a general series proceeding by ascending powers of the variable, as far as the sixteenth order. The journal is beautifully printed, and is set up by the editor himself. In the number for January, 1880, he says: "This number of the *Visitor* has been delayed some months, in consequence of the sickness of the editor, who has done all the type-setting with his own hands. He is not a practical printer, and never had set up a stickful of type till last May or June."

At the beginning of the present year Mr. Martin issued, besides the *Visitor*, a new publication, entitled *The Mathematical Magazine: a Journal of Elementary Mathematics*, of which four numbers have now appeared. It is of large quarto size, and, like the *Visitor*, is printed by the editor's own hands. No mathematical journal, if it is to contain anything of real value, can be elementary. Mathematics is an old science, and the really elementary parts of it must be acquired from text-books and by means of the examples which the student works out for himself as exercises. Elementary mathematics is a subject for the school-room, but is unsuitable for a journal, and such a publication as Mr. Martin's, if it continues elementary, is educational rather than scientific. In no instance, we believe, has it been found possible to restrict a mathematical journal really to the elements of the subject alone, though of course elementary articles, and articles which are of interest to junior readers, form

a considerable portion of each number of some mathematical publications. Mathematical investigations that are really valuable can never be made elementary, and the questions that can be treated by elementary mathematics are too trivial to deserve recognition in a scientific journal.

We may notice a demonstration of Euclid i. 47, by the late General Garfield, which appears in the first number. If the figure is completed it is in fact an intuitive geometrical proof that $(a+b)^2 = c^2 + 2ab$, where a and b are the sides and c the hypotenuse of a right-angled triangle. The construction is to divide the four sides of a square each into two parts, a and b, in the same order, and to join the points of division. Each of the joining lines is thus equal to the hypotenuse c, and the whole square, $(a+b)^2$, is evidently equal to the inside square, c^2, and the four triangles in the corners, each of which is equal to $\frac{1}{2}ab$. The figure is practically the same as in the well-known proof in which the squares a^2 and b^2 are placed side by side and divided by only two lines in such a manner that the parts may be moved by mere translation (without rotation) so as to form the square c^2, but the special features which give this proof its remarkable elegance are absent. Garfield's proof is Indian in its character, and must have been known to Bhascara, but in the rather more elegant one given in the Vija Ganita (1150) the lines are drawn from the angles of the square c^2 parallel to the sides of the triangle, and include a square $(a-b)^2$, each of the triangles in the corners being $\frac{1}{2}ab$ as before, so that the theorem proved is $c^2 = (a-b)^2 + 2ab$. If the points of division in the figure in § 150 of the Vija Ganita, in which it is shown that $(a+b)^2 - 4ab = (a-b)^2$ are joined, the figure includes both Garfield's and the Hindoo constructions. The construction given by Garfield must have been of course discovered over and over again, and, on its own account, it is so self-evident as only to be interesting historically in connection with the Indian proofs.

If therefore we include among journals one published at such long intervals as half a year there are now no less than four journals, devoted exclusively to mathematics, published in the United States.

With reference to the list of mathematical journals given in the previous article in NATURE, it may be mentioned that the Belgian journal, the *Nouvelle Corréspondance Mathématique*, which was edited by M. Catalan, with the co-operation of MM. Mansion, Brocard, Neuberg, and others, was discontinued at the end of 1880. It has been replaced by a new journal, *Mathesis*, which has since been published monthly under the editorship of MM. Mansion and Neuberg, and has now completed its second volume. In this journal the titles of elementary articles are marked by a cross; there are not on the average more than one or two so marked in each number.

A new Scandinavian mathematical journal is shortly to appear under the editorship of Prof. H. G. Zeuthen, of Copenhagen, and Prof. Mittiag-Leffler, of Stockholm. It is to be hoped that it has a great scientific career before it, and assuredly no journal will bear on its title-page the names of more illustrious mathematicians, or will have started under more favourable auspices.

J. W. L. GLAISHER

QUAIN'S "ANATOMY"

Quain's Elements of Anatomy. Edited by Allen Thomson, E. A. Schäfer, and G. D. Thane. Two volumes. Ninth edition. (London: Longmans, Green and Co., 1882.)

Lehrbuch der Neurologie. Fortsetzung von Hoffmann's "Lehrbuch der Anatomie." Von Dr. G. Schwalbe. (Erlangen: Eduard Besold, 1880 and 1881.)

THE appearance of a new edition of Quain's Anatomy is always regarded with attention and interest by teachers of anatomy. The high reputation of its successive editors, Richard Quain, William Sharpey, G. V. Ellis, Allen Thomson, and John Cleland, and the care which has been taken to revise each edition and to incorporate with it the latest additions to anatomical knowledge, have caused this work to be universally regarded as an authority, and have gained for it the position of a standard treatise on Human Anatomy.

The new edition, the ninth, which has just appeared, has been prepared under the editorial supervision of Professors Schäfer and Thane, and Dr. Allen Thomson. The first volume, which has been revised by Prof. Thane, contains the descriptive anatomy of the bones, joints, muscles, blood-vessels, but not the heart; cerebro-spinal and sympathetic nerves, but not the brain and spinal cord; with a chapter on superficial and topographical anatomy, in which the editor has been assisted by Mr. R. J. Godlee. The second volume has been for the most part revised by Mr. Schäfer, and contains the histology, and the anatomy of the viscera, including the heart and central organs of the nervous system; whilst a special chapter on embryology has been written by Dr. Thomson.

The separation of the anatomy of the heart from that of the other parts of the vascular system, as well as of the anatomy of the brain and spinal cord from the nerves which arise from them, and from the sympathetic system, both of which are so intimately connected both anatomically and physiologically with both brain and cord, was first made in the eighth edition; for prior to that time they had always been described along with, and as parts of their respective systems. This arrangement, which is also carried out in the present edition, is, in our judgment, most unphilosophical, for it both destroys the continuity of description, and leads the student to dissociate in his mind the origin of the nerves and blood-vessels from their distribution. Such a dissociation might indeed, as regards the nervous system, have been excusable at the time when both the distributory portions of the cranial and spinal nerves and the sympathetic system were believed to be developed quite independently of the cerebro-spinal axis, and only to become connected with it secondarily. But now-a-days, since through the researches, more especially of the much-lamented F. M. Balfour, both the cranial and spinal nerves and the sympathetic have been shown to be true offshoots of the cerebro-spinal axis, and like it of epiblastic origin, to dissociate them, even for descriptive purposes, in a systematic text-book, is, we believe, injurious to real progress. The editors of "Quain" would, we suppose, scarcely think of describing in one volume the gangliated cord of the sympathetic, and in another the nerves which arise

from it, and yet to do so would not be more illogical than the course they have pursued of separating the description of the cranio-spinal nerves from that of the central nervous axis. If the anatomical description of the human body is ever to be put on a scientific basis, it must be founded on the facts of comparative anatomy and of development, and the great aim of descriptive writers should be to accommodate their descriptions to these facts.

After this protest against some features in the general arrangement of the book, we may now glance at the manner in which the process of revision has been performed by the different editors. The first volume bears throughout the mark of careful revision by Prof. Thane. We have compared many of the descriptions with those of the corresponding structures in the immediately preceding edition, and we notice many changes both in the matter, and in the mode of expression. Various redundancies have been expunged, errors have been corrected, new facts have been introduced, and to some extent the descriptions generally have been re-arranged. Several new woodcuts have been inserted, and those illustrating the vascular system have been made more diagrammatic by colouring the arteries red and the veins blue. We notice, however, that Prof. Thane, as is unfortunately only too common with some English human anatomists, does not properly discriminate between the meaning of the terms mesial line and mesial plane. In his description, for example, of the recto-vesical portion of the pelvic fascia, he speaks of its being "continuous from side to side across the middle line in front of the bladder," forgetful apparently of the fact that the descriptive term "middle line" expresses a line on the surface, either anteriorly or posteriorly, as the case may be; whilst the imaginary plane between these anterior and posterior mesial lines is the mesial plane of the organ or region.

The greatest amount of change, however, as was naturally to be expected from the subjects discussed in it, has been made in the second volume, and more especially in the chapters edited by Mr. Schäfer. The important section on General Anatomy, or Histology, has been in some measure re-arranged, and many of its chapters rewritten. The latest investigations into the structure of the nucleus, the part which it plays in the multiplication of cells, and the process of maturation of the ovum, have been explained and illustrated by woodcuts. The description of the structure of the individual tissues has obviously been carefully revised, and various changes both in the way of addition and omission have been made. Not the least important is the addition to each chapter of the titles of the most recent papers on the subject-matter of the chapter.

If we were disposed to be very critical, we could undoubtedly lay our fingers upon more than one statement to which objection could be taken. And there is indeed one point that we cannot pass over without notice, as it illustrates that in the comparatively small matter of editing a work on anatomy, as in the much larger subject of administering the finances of Egypt, a Dual Control has many disadvantages. Mr. Schäfer, for example, in his references to the layers of the embryo, in which the several tissues take their rise, employs the terms ectoderm, mesoderm, endoderm, to express the three layers of the blastoderm, and in this respect adopts the nomenclature most commonly used by German anatomists; whilst Dr. Thomson, in his references to the same layers, almost invariably speaks of them as epiblast, mesoblast, and hypoblast, which, indeed, are the terms commonly in use amongst British anatomists. The employment in different sections of the same work of two distinct words to express the same structure, is an error in judgment, and certainly not to the edification of the student.

The chapter on the spinal cord and brain has also been greatly modified, and in re-arranging it Mr. Schäfer has largely availed himself of the book, the title of which stands second at the head of this article. The history of this book is somewhat curious. Originally it appeared as a German translation of an earlier edition of "Quain," edited by Prof. Hoffmann. Then, somehow or other, the name of Quain dropped out of the title in the later German editions, and now, as regards the chapters on the Nervous System, written by Prof. Schwalbe, it is essentially a new book, and in our opinion contains the best account of the anatomy of the nervous system in man which has yet been published.

Of the section on Embryology, prepared by Dr. Thomson, we have not space to say more than that it narrates in a convenient compass the successive series of changes which result in the formation of the adult human body; that the descriptions are clear, well arranged, and embrace the latest investigations; and that on points under dispute the author expresses himself with reservation and caution, in a manner characteristic of the writings of this anatomist.

LETTERS TO THE EDITOR

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

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

Transit of Venus, December 6

In the morning here the sky was clear, and the sun remarkably free from spots. I noticed only 4 small ones on the disk: quite a contrast to the monstrous appearance a month ago.

Being neither equipped nor qualified for technical astronomical observations, I did not attempt to do more than to give a popular demonstration of the transit of Venus to between 30 and 40 friends interested in the phenomenon. My experience of stargazing was chiefly obtained upwards of 50 years ago, before I became otherwise occupied; and then I found for myself, that the best way of studying solar phenomena, whether eclipses or spots, was by projecting on white paper or cardboard, the image of the sun from the telescope, focussed a little beyond the point for direct vision through the dark eye-glass; extraneous sunlight being shut out by a napkin suspended around the telescope. I presume that this method is well known, and it seems to have been adopted, (with the additions of a dark chamber) by Mr. Campbell of Islay, in the exhibition of the transit at Cannes, so well described by the Duke of Argyll in Nature (p. 156). In this way with a small achromatic of 32 inches focal length, and 2¼ inches aperture we saw well all the chief features of the transit, (which I need not describe, as this has already been done by more competent observers), and this without fatigue to the eyes, and the unnatural colouring, inseparable from looking through the telescope with darkened glasses. Further, as the time of sunset approached, at about 3.30 p.m., we had in our camera view the additional charm of the colours

of the objects in view. As, in the Italian sky the golden orb sank with the dark planet spot on its disk, under brightly tinted clouds, shaded off in streaks of tender grey into the azure above, with the blue rose-tipped mountains of the Esterets beneath,—the scene was one as fascinating in beauty as it was interesting in science. All these tints appeared distinctly, albeit faintly, in the telescopic image on the card.

One point was remarkable,—that whilst the shades of the mountains were all *blue*, the dark round spot of the planet on the sun was almost *black*. It was the darkest object in the field of view. Partly, but I hardly think entirely, this may be explained by its being higher, and less subjected to the decomposing power of the lower atmospheric layers. I have endeavoured to represent in water-colours this view of the transit of Venus. The result, of course, cannot be reproduced in print, but any of your readers who may be visitors at Cannes will be welcome to see it, as an original kind of reminiscence of a very rare event. C. J. B. WILLIAMS

Cannes, December 21

The Comet during the Last Month

SINCE my last communication (see NATURE, vol. xxvii. p. 110) the weather and the presence of moonshine has been unfavourable for views of the comet; but I have seen it, more or less distinctly, on seven nights, from November 22 to December 21. I will not take up your space with details, but mention, as the general result of these observations, that the comet has become smaller in dimensions, and much fainter in its light. With moonlight, no trace of a tail is visible; and the nucleus can only be discerned by telescope as a nebulous star of third magnitude. In absence of moonlight, as on December 6, 8, and 12, between 2 and 3.30 a.m., the tail was visible to a length of about 10°, with a breadth expanding from the head, with no distinguishable outline. My last view of it was on the 20th, at 3 a.m., when, with a brilliant starlight after moonset, the comet was in the south-south-east, about 20° above the horizon, with a tail about 8° long, and a nucleus, a nebulous star of third or second magnitude. Its position was about as far to the east-north-east of Procyon as that star is east-south-east of Sirius. It seems likely to be visible in clear moonless nights for two or three weeks longer. C. J. B. WILLIAMS

Cannes, December 21

The Heights of Auroras

THE observations described in your last number as having been made long since in Siberia, of lunar halos projected on auroras, have not, I believe, been confirmed by other observers; but if correct, possibly this phenomenon may be a peculiarity of auroras in Siberia, or in the Arctic regions. There seems reason to think (see Capron's "Auroræ," pp. 37-40) that auroras may be lower when near the magnetic pole than further south. If this is the case, it is so far favourable to the theory (propounded, I think, by a German writer) described in NATURE (Vol. xxv. p. 320), that the auroral zone is a plane, and not part of a sphere concentric with the earth's surface. The majority of the observations in lower latitudes cited in Capron's "Auroræ," place the phenomenon at a height of 100 miles or upwards.

The height of the spindle-shaped object seen in the aurora of November 17 is thus no argument against its auroral character, which I see no reason to doubt. It is true that in my experience (which, in this northern part of the country is probably much greater than that of your correspondents), I have never seen anything resembling it, judging from the descriptions of it; but I do not think this is a reason for supposing such an auroral phenomenon could not take place. The fact that it moved along a parallel of magnetic latitude is a very strong argument for its auroral character. Besides, its spectrum is stated to have exhibited the characteristic auroral line. I hope some one will collect all possible observations of this beam, especially from the continent, and undertake a careful investigation into its path and height. T. W. BACKHOUSE

Sunderland, December 23

The Aurora and its Spectrum

IN reference to Mr. Ralph Abercrombie's letter (NATURE, vol. xxvii. p. 173), I may mention that his remarks quite accord with an opinion expressed to me by my friend, H. R. Procter, that the "aurora is generally formed in some imperfect mist or vapour." I am intending some experiments on discharges *in vacuo* under such conditions and reduced temperatures, also on phosphorescence, in connection with which M. Lecoq de Boisbaudran has shown in his "Spectres lumineux," that we get a line in the red, brightening as the temperature is reduced. I do not read the result of my Swan lamp experiment, as Mr. Munro (same number and page) does. The lamp, when perfect, gave quite a bright white glow, with a strong carbon spectrum. I should therefore attribute the absence of the nitrogen spectrum at this time not so much to a high spectrum as to the probability that the lamp had been, as far as possible, exhausted of air, and filled with some form of carbon gas. I am not aware of any air-vacuum point at which the nitrogen bands or lines disappear, except for want of light in the discharge. With regard to the letter of W. M. F. P. on the "Meteor of November 17th," I only assumed the correctness of the figures and heights quoted in mine for the purpose of showing the complex nature of the auroral questions. I am not the less perfectly satisfied that the "beam" was a true aurora, and not a meteor, my spectroscopic observation of it putting this beyond a doubt. J. RAND CAPRON

Guildown, December 23

The Weather

IT is curious how the recent auroræ have been followed not only by a cold wave, but by a subsequent warm one, and these respectively of such extremes, that 21° at 9 a.m. on the 11th is this day replaced by 48° or 27° of difference. Equally strange have been the effects on animal and vegetable life. During the cold, an almost Arctic season in its ice-bound stillness prevailed, and a flock of wild geese crossing in front of the house (the forerunners, in public opinion, of a hard winter) represented external creature life. Now all is changed almost to spring. Roses, though somewhat nipped by the frost, seem ready to blow; flies and gnats are unthawing, and last night, in going up to the observatory, I noticed the phosphorescent glimmer of a luminous centipede under one of the shrubs, a sight I do not remember ever to have met with in winter before. J. RAND CAPRON

Guildown, December 19

A Common Defect of Lenses

A CHANCE observation a few weeks since led me to the discovery of a serious defect in the object-glass of the collimator of a spectroscope by Grubb, of Dublin, which I have been using for some time. As further investigation has shown me that the defect is very common, while at the same time it is a source of considerable error in all experiments on the plane of polarisation of polarised light, it seems worth while to call the attention of readers of Nature to it. The object-glass in question has been imperfectly annealed. As a consequence, a plane polarised incident beam is elliptically polarised on emergence from it.

If it be looked at between crossed Nicols in a pencil of parallel rays, the field of view becomes bright, and is crossed by two brushes hyperbolic in form, which for two positions of the lens became two straight lines. If again plane polarised light be allowed to pass through the lens while it is turned round its own axis, there are four positions of the lens for which the central portion of the emergent beam is plane polarised, and can be quenched by an analysing Nicol; for all other positions of the lens, the emergent beam is elliptically polarised, and the light cannot be quenched, but reduced to a minimum. Moreover, as the lens is turned, the position of the axes of the ellipse varies by nearly half a degree. I have since examined a large number of lenses, without finding one quite free from the defect. One well-known London optician declines to attempt to supply me with a two-inch object-glass which shall not show it, while another states he has never known any lens absolutely free from it.

The important bearing of the point on all investigations into the polarisation of light is obvious. The consequences it produces in modifying the results of some recently-published experiments of mine (*Phil. Trans.*, Part ii., 1882) formed the subject of a paper read at the last meeting of the Royal Society. R. T. GLAZEBROOK

Trinity College, Cambridge, December 20

New Deep-Sea Fish from the Mediterranean

MY letter in NATURE, vol. xxv. p. 535, called forth two important notes from such competent ichthyologists as Mr. J. Y.

Johnson and Dr. Th. Gill (NATURE, vol. xxvi. pp. 453, 574), to both of whom a reply is due, and should have been given sooner had I not been absent from Florence and otherwise engaged.

Firstly, I must correct my assertion as to the occurrence of *Malacocephalus lævis* in the Mediterranean; after having examined the type specimen and that mentioned by Mr. Johnson, both in the British Museum and after a further examination of my specimens, which I had considered as young *Malacocephali*, I have now not the slightest doubt that they are quite distinct. They are an undescribed and most interesting form of Macrurids allied to *Coryphænoides*, which I propose calling *Hymenocephalus italicus*. I have in my possession six specimens, both adult and young; in two of the former I have found the ovaries fully developed with mature ova.

As to the "singular fish of a deep black colour with small eyes, a naked skin, and a most abyssal physiognomy," which I got at Messina, it has no connection whatever with *Chiasmodon niger*, but is, as I before asserted, a Stomiatid, very different from all the known forms, including Dr. Günther's *Bathyophis*. It stands apart in many respects, and is the type of a new genus and perhaps of a new section of that singular family. I intend shortly to describe and figure it under the name of *Bathophilus nigerrimus*, along with other strange fish collected during my deep-sea and ichthyological researches in the Mediterranean.

HENRY HILLYER GIGLIOLI

R. Zoological Museum, Florence, December 17

Electrical Phenomenon

ON retiring to bed shortly after midnight on the 13th inst., I experienced a phenomenon which, though not of itself uncommon, was, I think, unusually developed. On pulling off a flannel vest which I wear next my skin, over my head, I became conscious of a strange sensation in the latter, accompanied by a distinct crackling noise, and bright sparks which were plainly visible in the dimly lighted room. To make sure that I was not the subject of a delusion, I repeated the operation many times, in each case rubbing the flannel half-a-dozen times—not more—against my hair. Not only were the same phenomena observable every time, but also if, after removing the flannel I then approached my knuckles to that part of it which had been in contact with the hair, a whole volley of sparks passed between the flannel and each knuckle at a distance of not less than *two inches*. As often as I repeated the experiment, so often did the phenomena repeat themselves, until I at length retired to bed not altogether without apprehension, that I might awake in the night with the bed-clothes on fire, by reason of the discharge of some extra big spark between my hair and a convenient blanket. No such catastrophe, however, occurred, and on repeating the operations the next morning, I could not reproduce the phenomena. The next evening I again repeated the experiment, and this time by very violent rubbing could just get a faint discharge between the flannel and knuckles when almost in contact. On other nights since these I have not succeeded in getting any such effect, or at most a very feeble one. To what, then, am I to attribute the marked difference of the first night? Was it due to something peculiar in the condition of the hair, the air, or the flannel? Perhaps some of your readers can suggest. As regards the first of these I ought to state that it had, on the afternoon of the 13th, been subjected to the operations of cutting, shampooing, and brushing "by machinery," at the hands of the barber. That was, however, seven hours earlier in the day, and any electricity developed by the friction of the last operation ought to have been dissipated long before twelve o'clock—especially as the night was damp and misty.

A. J. K.

29, Victoria Road, Finsbury Park, December 19

PHOTOGRAPHING THE CORONA[1]

PROBLEMS of the highest interest in the physics of our sun are connected, doubtless, with the varying forms which the coronal light is known to assume, but these would seem to admit of solution only on the condition of its being possible to study the corona continuously, and so to be able to confront its changes with the other variable phenomena which the sun presents. "Unless some means be found," says Prof. C. A. Young, "for bringing out the structures round the sun which are hidden by the glare of our atmosphere, the progress of our knowledge must be very slow, for the corona is visible only about eight days in a century, in the aggregate, and then only over narrow stripes on the earth's surface, and but from one to five minutes at a time by any one observer" ("The Sun," p. 239).

The spectroscopic method of viewing the solar prominences fails, because a large part of the coronal light gives a continuous spectrum. The successful photograph of the spectrum of corona taken in Egypt, with an instrument provided with a slit, under the superintendence of Prof. Schuster during the solar eclipse of May 17, 1882, shows that the coronal light as a whole, that is the part which gives a continuous spectrum, as well as the other part of the light which may be resolved into bright lines, is very strong in the region of the spectrum extending from about G to H. It appeared to me, therefore, very probable that by making exclusive use of this portion of the spectrum it might be possible under certain conditions, about to be described, to photograph the corona without an eclipse.

In the years 1866-68 I tried screens of coloured glasses and other absorptive media, by which I was able to insolate certain portions of the spectrum with the hope of seeing directly, without the use of the prism, the solar prominences (*Monthly Notices*, vol. xxviii. p. 88, and vol. xxix. p. 4). I was unsuccessful, for the reason that I was not able by any glasses or other media to isolate so very restricted a portion of the spectrum as is represented by a bright line. This cause of unsuitableness of this method for the prominences which give bright lines only, recommends it as very promising for the corona. If by screens of coloured glass or other absorptive media the region of the spectrum between G and H could be isolated, then the coronal light which is here very strong would have to contend only with a similar range of refrangibility of the light scattered from the terrestrial atmosphere. It appeared to me by no means improbable that under these conditions the corona would be able so far to hold its own against the atmospheric glare, that the parts of the sky immediately about the sun where the corona was present would be in a sensible degree brighter than the adjoining parts where the atmospheric light alone was present. It was obvious, however, that in our climate and low down on the earth's surface, even with the aid of suitable screens, the addition of the coronal light behind would be able to increase, but in a very small degree, the illumination of the sky at those places where it was present. There was also a serious drawback from the circumstance that although this region of the spectrum falls just within the range of vision, the sensitiveness of the eye for very small differences of illumination in this region near its limit of power is much less than in more favourable parts of the spectrum, at least such is the case with my own eyes. There was also another consideration of importance, the corona is an object of very complex form, and full of details depending on small differences of illumination, so that even if it could be glimpsed by the eye, it could scarcely be expected that observations of a sufficiently precise character could be made to permit of the detection of the more ordinary changes which are doubtlessly taking place in it.

These considerations induced me not to attempt eye-observations, but from the first to use photography, which possesses extreme sensitiveness in the discrimination of minute differences of illumination, and also the enormous advantage of furnishing a permanent record from an instantaneous exposure of the most complex forms. I have satisfied myself by some laboratory experiments that under suitable conditions of exposure and development a photographic plate can be made to record minute differ-

[1] "On a method of Photographing the Solar Corona without an Eclipse." Paper read at the Royal Society by William Huggins, D.C.L., LL.D., F.R.S., December 21.

ences of illumination existing in different parts of a bright object, such as a sheet of drawing paper, which are so subtle as to be at the very limit of the power of recognition of a trained eye, and even, as it appeared to me, those which surpass that limit.

My first attempts at photographing the corona were made with photographic lenses, but uncertainty as to the state of correction of their chromatic aberration for this part of the spectrum, as well as some other probable sources of error which I wished to avoid, led me to make use of a reflecting telescope of the Newtonian form. The telescope is by Short, with speculum of 6 inches diameter, and about 3½ feet focal length. A small photographic camera was fastened on the side of the telescope tube, and the image of the sun after reflection by the small plane speculum was brought to focus on the ground glass. The absorptive media were placed immediately in front of the sensitive film, as in that position they would produce the least optical disturbance. Before the end of the telescope was fixed a shutter of adjustable rapidity which reduced the aperture to 2 inches. This was connected with the telescope tube by a short tube of black velvet for the purpose of preventing vibrations from the moving shutter reaching the telescope. On account of the shortness of the exposures it was not necessary to give motion to the telescope.

It was now necessary to find an absorptive medium which would limit the light received by the plate to the portion of the spectrum from about G to H. There is a violet (pot) glass made, which practically does this. I had a number of pieces of this glass ground and polished on the surfaces. Three or four of these could be used together, castor-oil being placed between the pieces to diminish the reflection of light at their surfaces. Some inconvenience was found from small imperfections within the glass, and it would be desirable in any future experiments to have a larger supply of this glass, from which more perfect pieces might be selected.

In my later experiments I used a strong and newly made solution of potassic permanganate, in a glass cell with carefully polished sides. This may be considered as restricting the light to the desired range of wave-length, since light transmitted by this substance in the less refrangible parts of the spectrum does not affect the photographic plates.

Different times of exposure were given, from so short an exposure that the sun itself was rightly exposed, to much more prolonged exposures, in which not only the sun itself was photographically reversed, but also the part of the plates extending for a little distance from the sun's limb.

Gelatine plates were used, which were backed with a solution of asphaltum in benzole.

After some trials I satisfied myself that an appearance peculiarly coronal in its outline and character was to be seen in all the plates. I was, however, very desirous of trying some modifications of the methods described, with the hope of obtaining a photographic image of the corona of greater distinctness, in consequence of being in more marked contrast with the atmospheric illumination.

Our climate is very unpropitious for such observations, as very few intervals, even of short duration, occur in which the atmospheric glare immediately about the sun is not very great. Under these circumstances I think it is advisable to describe the results I have obtained, without further delay.

The investigation was commenced at the end of May of this year, and the photographs were obtained between June and September 28.

The plates which were successful are twenty in number. In all these the coronal form appears to be present. This appearance does not consist simply of increased photographic action immediately about the sun, but of distinct coronal forms and rays admitting in the best plates of measurement and drawing from them. This agreement in plates taken on different days with different absorptive media interposed, and with the sun in different parts of the field, together with other necessary precautions observed, makes it evident that we have not to do with any instrumental effect.

The plates taken with very short exposures show the inner corona only, but its outline can be distinctly traced when the plates are examined under suitable illumination. When the exposure was increased, the inner corona is lost in the outer corona, which shows the distinctly curved rays and rifts peculiar to it.

In the plates which were exposed for a longer time, not only the sun but the corona also is photographically reversed, and in these plates, having the appearance of a positive, the white reversed portion of the corona is more readily distinguished and followed in its irregularly sinuous outline than is the case in those plates where the sun only is reversed, and the corona appears, as in a negative, dark.

Prof. Stokes was kind enough to allow me to send the originals to Cambridge for his examination, and I have his permission to give the following words from a letter I received from him: "The appearance is certainly very corona-like, and I am disposed to think it probable that it is really due to the corona."

Prof. Stokes' opinion was formed from the appearance on the plates alone, without any knowledge of their orientation, and without the means of comparing them with the eclipse plates taken on May 17.

I have since been allowed, through the kindness of Capt. Abney, to compare my plates with those taken of the corona in Egypt during the eclipse of May last. Though the corona is undergoing doubtless continual changes, there is reason to believe that the main features would not have suffered much alteration between May 17 and September 28, when the last of my plates was taken. This comparison seems to leave no doubt that the object photographed on my plate is the corona. The more prominent features of the outer corona correspond in form and general orientation, and the inner corona, which is more uniform in height and definite in outline, is also very similar in my plates to its appearance in those taken during the eclipse.

Measures of the average height of the outer and of the inner corona in relation to the diameter of the sun's image are the same in the eclipse plates as they are in my plates taken here.

There remains little doubt that by the method described in this paper, under better conditions of climate, and especially at considerable elevations, the corona may be successfully photographed from day to day with a definiteness which would allow of the study of the changes which are doubtlessly always going on in it. By an adjustment of the times of exposure, the inner or the outer corona could be obtained as might be desired. It may be that by a somewhat greater restriction of the range of refrangibility of the light which is allowed to reach the plate, a still better result may be obtained.

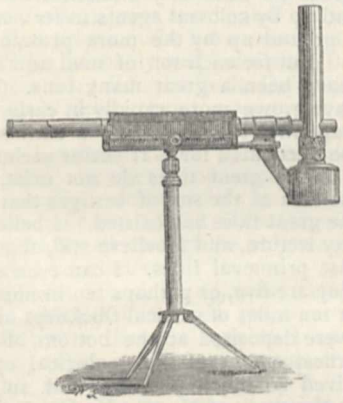
Plates might be prepared sensitive to a limited range of light, but the rapid falling off of the coronal light about H would make it undesirable to endeavour to do without an absorptive screen. Lenses properly corrected might be employed, but my experience shows that excessive caution would have to be taken in respect of absolute cleanness of the surfaces and of some other points. There might be some advantage in intercepting the direct light of the sun itself by placing an opaque disk of the size of the sun's image upon the front surface of the absorptive screen. Though for the reasons I have already stated I did not attempt eye-observations, there seems no reason why, with suitable screens and under suitable atmospheric conditions, the corona should not be studied directly by the eye. There might be some

advantages in supplementing the photographic records by direct eye-observations. I regret that the very few occasions on which it has been possible to observe the sun has put it out of my power to make further experiments in these and some other obvious directions.

P.S.—[I have Capt. Abney's permission to add the following letter this day received from him;—"A careful examination of your series of sun-photographs, taken with absorbing media, convinces me that your claim to having secured photographs of the corona with an uneclipsed sun is fully established. A comparison of your photographs with those obtained during the eclipse which took place in May last, shows not only that the general features are the same, but also that details, such as rifts and streamers, have the same position and form. If in your case, the coronal appearances be due to instrumental causes, I take it that the eclipse photographs are equally untrustworthy, and that my lens and your reflector have the same optical defects. I think that evidence by means of photography of the existence of a corona at all is as clearly shown in the one case as in the other."—*December* 15, 1882.]

A WEDGE AND DIAPHRAGM PHOTOMETER

A NEW photometer, shown in perspective in the figure, has lately been constructed by Mr. Sabine. The stand supports a straight horizontal tube, at one end of which is a paraffin lamp, and at the other an eyepiece. The middle portion of the tube is cut away, and has, slipped over it, a collar to which a frame is attached, carrying a wedge of neutral-tinted glass, adjustable by means of a rack and pinion. Inside the collar is fixed a transverse disc of ground opal glass, which the paraffin lamp illuminates to a definite degree. This disc constitutes the field of comparison, the illumination of which is



adjustable by means of a series of diaphragms of known aperture at the end near to the paraffin lamp. At the side, between the wedge and the collar which carries it, is a narrow pane of ground opal glass, just behind which a small mirror is fixed at an angle of 45° to the axis of the tube. This mirror is supported from the centre of the transverse opal disc in such a way, that the support is hidden from the observer by the mirror itself, an arrangement which insures the apparent juxtaposition of the illuminated surfaces which have to be compared. The light to be measured is placed on the right-hand side of the photometer; and the collar is turned so that the light falls normally upon the face of the wedge, passes through the wedge, through the pane of opal glass, and is incident upon the mirror, which reflects a portion of it to the eye of the observer. The wedge is then shifted, if necessary, to interpose a greater or less thickness of absorbing medium, until a balance is obtained, that is until the apparent illumination of the mirror is equal to that of the field of comparison, in the middle of which it is seen. If the range of the wedge is insufficient to admit of this, the degree of illumination of the field is altered, by means of the diaphragms, and the wedge is then adjusted.

The employment of glass wedges for photometric comparisons is not new, having been already used by both Xavier de Maistre and Quetelet; but no practical photometer based upon this method has hitherto been constructed. The employment of diaphragms for extending the range of the wedge is found to work well and to enable the operator to adjust the illumination of the field with exactitude, the bright part of the paraffin flame being of course, kept opposite to, and so as to well cover the diaphragm aperture. A table is constructed giving for each position of the wedge and for each diaphragm, the value, in standard candles, of any light placed at a distance of one metre from the instrument; and if the light be placed at any other distance, the number in the table has simply to be multiplied by the square of the actual distance in metres. For ascertaining approximately the amount of light which passes through any given coloured glass, for example, orange glass, the eyepiece is furnished with a rotary disc containing small panes of white and different coloured glasses, either of which can be interposed at pleasure.

This photometer is being made by Messrs. Elliott Bros., in two forms, one for use as a portable photometer, as shown in the figure, and the other on a more solid stand, for laboratory purposes.

ON THE OCCURRENCE OF GREAT TIDES SINCE THE COMMENCEMENT OF THE GEOLOGICAL EPOCH[1]

IT will I daresay be within the recollection of many of those who are now present that I was honoured by the invitation to deliver the opening lecture in this hall last year. In response to that invitation I addressed to you a discourse which I ventured to call "A Glimpse through the Corridors of Time." Accounts of it have appeared in very many quarters, both at home and abroad. I am myself responsible for the account which appeared in the columns of NATURE, as well as for the pamphlet form in which the lecture has since been issued. The chief reason why I now recur to the subject remains to be stated. Among the various comments which have been made upon that address, some are by no means favourable to the views I ventured to put forward, and they have been the theme of considerable discussion. Up to the present I have not made any reply to the criticisms which have appeared; I postponed doing so until a suitable opportunity should have arisen for a review of the whole subject. Your kindness in inviting me once again to address this great Institute has afforded such an opportunity, and with your permission I propose to preface the subject of my lecture this evening by a reply to those critics who have honoured me with their attention.

Let me recall to you very briefly the subject of that lecture, so as to enunciate clearly the point as to which an issue has been raised. You will perhaps recollect that the lecture treated principally of the tidal relations between the earth and the moon, of the influence of the tides during ages past, and of the future which awaits the earth-moon system during ages to come. I pointed out that at the present moment the orbit of the moon must be gradually growing in size, that this gradual increase of the distance from the earth to the moon is essentially non-periodic, and thus is totally different to the ordinary lunar irregularities which are recognised in rigid-body astronomy. As a consequence of this incessant growth in the moon's distance we see that in past ages the moon must have been appreciably nearer to the earth than it is

[1] Extract from a lecture delivered at the Midland Institute, Birmingham, on November 20, 1882, by Prof. Robert S. Ball, LL.D., F.R.S. Communicated by the Author.

at present, and that if we look far enough back we must inevitably come to an epoch when the moon would seem to have been quite close to the earth; indeed looking earlier still we are not without reasons for believing that in primeval times the earth and the moon formed but a single body.

Wondrous as this narrative may seem, yet on a due consideration of the mathematical evidence in its favour we are constrained to admit that it must be substantially correct. Unless some notable agency at present unknown to us has intervened in past time, the course of events must have run along the lines we have indicated. We make but little pretence to give the date when the moon seems to have commenced its independent existence, nor to indicate the chronology of the epochs when its distance increased by one thousand miles after another. All that seems certain is that the events we are at present discussing must have occurred millions, many millions of years before man placed his foot on this planet.

The cause of these mighty series of changes is still in hourly operation. The ebb and flow of the tides around our coasts is only the survival of greater tides with which in earlier days the ocean must have throbbed. The earlier we look back the mightier must have been the daily ebb and flow. I even invited you to look back to an excessively remote epoch when the moon was only at a fraction of its present distance, and when the daily rise and fall, instead of being counted in tens of feet, must have been reckoned by hundreds. Even up to this point there has been little or no controversy, there can be none. I do not know that any one has attempted to deny that the earth must once have experienced these mighty tides either in the actual body of the earth, or in the ocean on its surface. The controversy has arisen on the question as to whether these great tides had subsided before the commencement of the geological epoch, or whether they were contemporaneous therewith.

In my lecture in this hall last year, I made the suggestion that the reign of the mighty tides did perhaps extend into the commencement of the geological epoch. I further ventured to suggest that these great tides had left their traces on the solid crust of the earth. I quoted eminent geological authority to show that the rocks at the base of our stratified system are of the most stupendous volume and thickness. It has always been a difficulty to determine how the present geological agents could have manufactured so mighty a mass, and I appealed to the great tides as a grinding engine competent to aid in this work. At this point issue has been taken, and it is now my duty to review the arguments which have been adduced as bearing on this question. But let me here make a single remark which disposes of many of the objections that have been raised. Several of my critics do not seem to have observed that I postulated the mighty tides for the manufacture of the earliest primæval rocks, and *for these alone*. "Take, for example," I said (p. 25), "that earliest and most interesting epoch when life perhaps commenced on the earth, and when stratified rocks were deposited five or ten miles thick, which seem to have contained no living form higher than the eozoon, if even that were an organised being." Again and again I stated that I merely referred to these primitive strata. Yet this is a point that many of my critics have ignored. They have been at pains to prove that colossal tides did not exist in the comparatively modern geological epochs, and many interesting facts have been adduced. But such considerations have only an infinitesimal bearing on the position I adopted. Even the coal-measures are a modern formation when compared with the primeval rocks, for the manufacture of which I suggested the mighty tides.

The controversy as to the great tides has principally ranged around the question as to whether the primitive rocks present any indications of great tidal action. I am not a practical geologist, and am most anxious to obtain the views of those that are. Now these opinions are to be had. They are to be found in the correspondence in NATURE at the commencement of this year; yet there is, as might have been anticipated, considerable differences of opinion. I first refer to Prof. Hull's letter (NATURE, vol. xxv. p. 177), and find that this most competent authority adduces direct evidence of tremendous denudation in the Palæozoic ages, such as might have been produced by mighty tides. On the other hand, Prof. Newberry (vol. xxv. p. 357) says that there is no direct evidence whatever to show that the denuding agencies were greater in former times than now. In the following number of NATURE we have letters from Dr. Callaway and Mr. Hale (p. 385) to show that Prof. Newberry's conclusions are not necessarily valid. Mr. S. V. Wood and Mr. J. Vincent Elsden bring forward facts which go to support Prof. Newberry's view, while Dr. Callaway, though carefully declining to commit himself to the high tides, controverts Mr. Wood and Mr. Elsden (p. 409). Now all these gentlemen speak with special knowledge, but it is not easy to deduce from this correspondence as to which side the balance of skilled opinion really inclines. It would almost seem as if a very fundamental point had escaped attention. Prof. Geikie, in his great work just published, tells us that these great tides could not have existed in geological times, because, if they had done so they must have left certain traces, and we do not find these traces. The fundamental question is, What traces of great tides ought we to expect to find if those great tides had really existed? It would seem that unless this question be first answered, it is impossible to dispose of the question with the brevity which Prof. Geikie has adopted. I apprehend it cannot be doubted that by the great tides the materials of stratified rocks would be rapidly formed, and that in suitable localities these materials would be deposited to form rocks. I can see no necessary difference between a ton of mud ground up by colossal agents in former days, and a ton of mud ground up by the more prosaic agents of modern days. But for each ton of mud now made there would then have been a great many tons. The strata would thus have grown more rapidly in early times, and thus the exceptional thickness of the earliest stratified rocks could be accounted for. It seems useless to assert that vestiges of the great tides do not exist, unless we can form some idea of the sort of vestiges that should be expected if the great tides had existed. I believed at the time I gave my lecture, and I believe still, that we do see vestiges of vast primæval tides. I can even count these vestiges. They are five, or perhaps ten in number; they are the five or ten miles of vertical thickness of stratified rocks which were deposited at the bottom of the ocean during the earliest stages of the geological epoch.

I have derived so much pleasure and so much instruction from the study of Mr. G. H. Darwin's writings, that on this ground alone I would be reluctant to have any difference of opinion with him. Indeed, seeing that the earth-moon history is one which he has made peculiarly his own, and illuminated by discoveries which I believe to be the most important contributions made to physical astronomy in modern times it would seem presumption in me to venture to differ from him. Mr. Darwin, writing in NATURE, vol xxv. p. 213, has asked me to reconsider the views I had set forth as to the probability of the great tides being contemporaneous with geological phenomena. Mr. Darwin shows that at the time when the great tides supposed in my calculation existed, the earth must have been spinning round once in seven hours, and that this would involve trade-winds of 3½ times their present velocity and vertical storms of prodigious violence, and then he adds:—

"Now if this state of things existed in geological history, we should expect to find the earlier sedimentary

rocks of much coarser grain than the modern ones. But I am not aware that this is the case. Again, to understand such blasts, the earliest trees should have trunks of enormous thickness and their leaves must have been very tough, or they would have been torn to shreds. There seems to be no reason to suppose that the trees of the Carboniferous period present marked peculiarities in these respects."

"It is on these grounds that I venture to dissent from Mr. Ball in the geological interpretation to be placed on the tidal theory, and I think we must put these violent phenomena in pre-geological periods."

But is it necessarily true that the prodigious tides must have produced a coarser material as the result of their grinding than is found in the later rocks? I can imagine it to be contended that the more powerful mill would produce the finer flour, but in truth I really do not see that we have any *a priori* grounds for deciding whether the *débris* produced by mighty tides should be fine or coarse. Have we not illustrious authority for invoking our "Domestic Productions" to throw light on obscure questions removed from actual observation. Let us look at the biggest tides we know of, and see whether they are associated with fine mud or with coarse. I appeal to every one who has stood on the Clifton Suspension Bridge or walked on the Beach at Weston-super-Mare to answer this question. In both cases they will see mud of a fineness and a stickiness that is proverbial; yet that mud is washed twice every day by the mightiest tides in the British Islands. I do not say, nor do I believe, that the fineness of the mud in the Avon is the consequence of the great tides; but I think the illustration is a fair reply to an argument which says the tides in ancient days cannot have been of great size, because the mud with which those tides are associated is not coarse.

In the second place, Mr. Darwin urges against me the trees of the Carboniferous epoch, and his inference that the tremendous tides cannot have existed in the Carboniferous epoch is probably well founded. But I have not said that these tides did exist in the Carboniferous epoch. I can only again repeat that my argument supposed that the mighty tides may have existed in the times when the very earliest stratified rocks were deposited. In the course of ages, as the moon receded, so the tides gradually dwindled down until in the comparatively modern time indicated by the Carboniferous epoch, they may have been small enough to be connected with the wonderful coal vegetation.

I had, as I was bound to do, most carefully weighed the words in which I addressed you from this place last year. I was aware that the opinion I advanced would meet with opposition. This was a reason why I should consider the subject most carefully before I spoke, but it was not a reason why I should withhold the views at which I had arrived. I have again considered the matter with the results now set forth, and I have seen no reason to depart in the slightest degree from the position which I had previously adopted.

MARS[1]

THE similarity which has long been thought to exist between our own globe and the planet Mars would naturally commend itself to careful examination at the hands of such observers as possess instruments adequate to the inquiry. The shadowing of large portions of its surface with patches which easily lend themselves to the supposition of being collections of water, the occasional indistinctness of their outlines, so strongly indicative of atmospheric obscuration, the clothing of either pole with the semblance of a snowy mantle obedient in its extent to solar action, all this would bespeak of itself a critical investigation. And the challenge has been taken up from an early period, and to an extent which would probably surprise those who are unfamiliar with the subject. Already in 1873 the number of drawings collected by Dr. Terby of Louvain, than whom no man is more intimately conversant with areography, amounted to 1092, and the nine subsequent years, which have included among others the celebrated representations of Green and Schiaparelli, have greatly augmented that imposing number. We should be mistaken, however, if we were to estimate the progress of our knowledge by the multiplication of designs. In this case the ancient saying πλέον ἥμισυ παντός would probably express too large a proportion. The increase, if in some respects not to be regretted, brings with it additional elements of uncertainty, if not of error. Many representations might be discarded with positive advantage to the final conclusion: like numerical observations whose unworthiness is detected by their wide deviation from the mean of the rest, the result is all the surer for their exclusion. An unpleasant experience proves that the most careful observer is not always the most successful draughtsman, nor in such matters is zeal any pledge of excellence. Comparison of the results obtained by different astronomers leads to the conclusion that, after due allowance has been made for instrumental and atmospheric differences, all men do not see alike, or interpret in the same way what they see, or transfer the image to paper with equal success. Here it is that photography, though not exempt from defects and hindrances of its own, is now beginning to render invaluable aid. But such an object as the disc of Mars would not lend itself very readily at present to the camera, and the pencil and the brush must do the best they can till some further advance is made to supersede them.

But however improved may be our future representations, and whatever may be the result—on every supposition most interesting—of the keen scrutiny that is in store for the next opposition of the planet, it would undoubtedly be an injudicious course to discard as unworthy of study and comparison the delineations of earlier days. Less valuable, if standing alone, they may attain considerable importance in the elucidation of some otherwise unexplained difficulty; and evidence which, unsupported, might be of little weight, may acquire especial consequence from its collateral bearing on more direct testimony. The comparatively rude and defective sketches of a long-passed era, contained in the publication before us, executed in a spirit of unwearied industry and unimpeachable fidelity, but under the influence of a mistaken impression, form a striking illustration of the previous remarks.

The history of the "Areographische Beiträge" is connected with a very lamentable occurrence in the life of the worthy old Hanoverian observer, Dr. Johann Hieronymus Schröter. He had long been settled in a Government office at Lilienthal, not far from Bremen, where his almost innumerable observations on sun, moon, and planets (with stars he did little) had been carried on with reflectors of various sizes—two by Sir W. Herschel of 4 and 7 feet focal length, others by Schrader, of Kiel, of 7, 11, 15, and 27 (26 English) feet, and a 4-inch object-glass by Dollond, equatorially mounted. His passion for observation would never have allowed so interesting an object as Mars to escape him, and accordingly we find that between the years 1785 and 1803 he had accumulated 217 designs, with a corresponding description marked by all the minute preciseness of detail and inference which characterise his other labours. The work had been promised for publication at Easter, 1812, but had been somehow delayed, when an event occurred on the night of April 20, 1813, in connection with the occu-

[1] "Areographische Beiträge zur genauern Kenntniss und Beurtheilung des Planeten Mars." Von Dr. J. H. Schroeter: herausgegeben von H. G van de Sande Bakhuyzen, Director der Leidener Sternwarte. 8vo, 447 pp., with Atlas. Leiden: E. J. Brill.

pation of Bremen by the French, under the rapacious and unscrupulous Vandamme, the story of which we may allow the sufferer to relate in his own words.

"Through the most barbarous fury, in consequence of an equally barbarous sentence, the whole unoffending soft "vale of lilies" (Lilienthal) was, without previous inquiry, destroyed by fire. Without possibility of succour, they burnt down also the Royal Government offices; I lost the whole of my furniture, and what was most distressing of all, with a considerable damage also to the bookshops of Europe, the sole stock of my collected works and writings laid up in the government buildings. Even my observatory, preserved by Providence from the conflagration, was a few days afterwards broken into, plundered, and through the destruction of the clocks, breaking off of the finders, and robbery of the smaller instruments, scandalously ruined. Having been displaced from my post, my income had been previously by degrees so very much reduced, that I was compelled to forego everything but absolutely necessary expenditure, and to be laid aside in a scientific slumber." Elsewhere he says that even his journals had perished; and at the date of writing the introduction to his "Observations and Remarks upon the Great Comet of 1811" (January 22, 1815), from which the foregoing passage is taken, he complains that his circumstances were still so reduced, that his observatory, for want of time and money, remained for the most part in a state of confusion. So great are even the minor miseries of those "wars and fightings," of which many speak with such apathetic unconcern. It is painful to add to these sad details that this seems to have been Schröter's final effort, for after a twelvemonth of bodily and intellectual decay, he expired August 29, 1816, leaving behind him a worthy memory, to which, till of late years, our own country has done but inadequate justice.

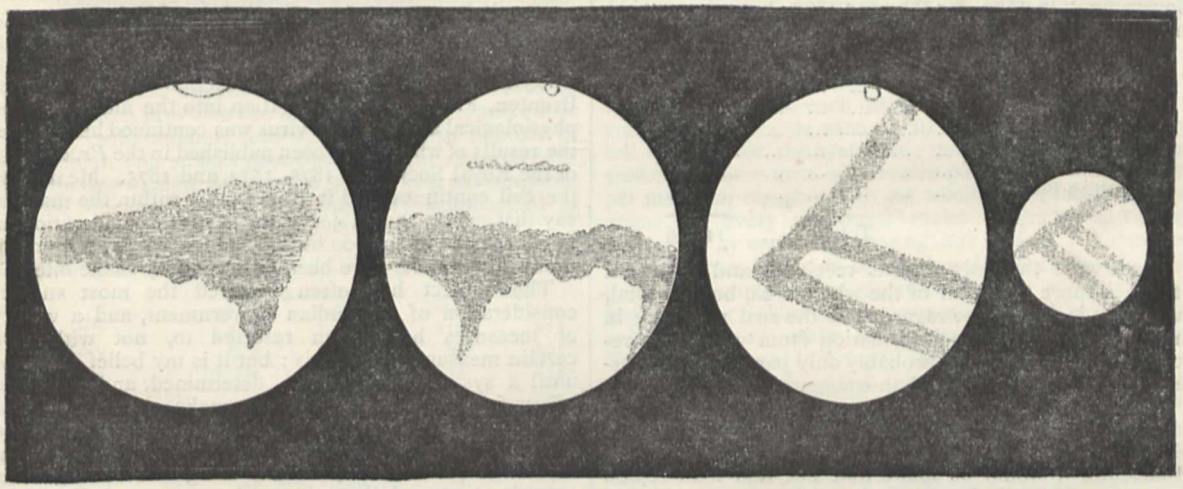
The "Areographische Beiträge" remained in MS. at his death, having escaped the calamitous fire, but so narrowly, that two out of the sixteen copper plates of figures had to be engraved again; no idea seems to have been entertained of publication, but they were safely preserved by the author's family. Their existence having been ascertained many years ago, by the present writer, through the kindness of Dr. Peters of Altona, a negotiation was set on foot for their acquisition by the Royal Astronomical Society; this proved ineffectual; but, in consequence of the attention directed to them, they were allowed to be inspected by Dr. Terby, to whose able and comprehensive analysis of the MS. as published in the *Memoires* of the Belgian Royal Academy of Sciences in 1873, the present notice is deeply indebted; and astronomers will be glad to learn tnat they have now been purchased by the University of Leiden for the library of that observatory, and that, after an obscurity protracted through seventy years, they have at last been published in a complete and handsome form, under the able and accurate editorship of the director of that institution.

The work, though characterised, like other productions of the same author, by a needless amount of prolixity, is well deserving of careful study, as indicating or confirming some valuable conclusions, and affording material for suggestive thought. The whole observations are pervaded by an impression that the obscurer portions of the disc are condensations in a vaporous atmosphere. The author, with a singular misconception of terrestrial analogy, supposes throughout that such cloudy masses viewed on their upper or enlightened side would appear darker rather than lighter than the surface beneath them; admitting at the same time that the configuration of that surface may so modify the superjacent atmosphere, as to cause a permanence, or, at any rate, recurrence of vaporous formation, from which the rotation may be, and has been determined. The occasional invisibility of dark spots, which has been recorded by too many observers to be brought in question, would be explained by Schröter in accordance with this theory, and may possibly be due to atmospheric causes; though, as Terby has pointed out, it may often have arisen from the difficulty of tracing any markings in the neighbourhood of the limb. It is more difficult to account satisfactorily for the movements ascribed by Schröter to the action of winds, of which he has specified in an elaborate table no less than forty-six instances, not much differing in velocity from those on earth, and in the great majority of cases conspiring with the direction of the rotation. Error may have crept in with regard to the identification of some of the spots; and both from the designs and descriptions a suspicion arises that some of the minuter details, which now serve for the recognition of distinct but similar regions, escaped the observer's notice. There must have been some cause for this unfortunate defect which detracts materially from the value of his work, and the removal of which would have been the one step in advance which, as Terby remarks, would have put him in possession of the true interpretation of what he saw. It would be a ready explanation to refer it to the imperfect defining power of his instruments; and I have somewhere read of a comparison instituted after his time between his much-prized speculum of 9½ inches aperture and 12⅔ feet focal length, and a Fraunhofer object-glass of, I think, about 4 inches, to the disadvantage of the former. Yet on the other hand he began his work with telescopes by the elder Herschel; and his 7-foot instrument by this great maker does not seem to have been superior to that of the same dimensions by Schrader, the manufacturer of all his larger ones. This point is therefore not quite clear. We might have attached some importance to his own admission, in his work on the moon, that his vision was less microscopic than that of his assistant, Harding, but for the fact that the latter occasionally aided him in these observations on Mars. Whatever may have been the cause, the frequent absence of minuter detail must have served to confirm Schröter's misapprehension of what he saw. And it must be borne in mind, in estimating his observations in general, that it was his habit to undertake investigation with a preconceived idea. In the case of the moon, his prepossession in favour of changes no doubt occasionally misled his judgment; and on Mars he might be prepared to look out for atmospheric movement by the known phenomena of Jupiter. An anticipation of this kind may not be without incidental advantage in directing and sharpening attention: our search may be aided, and with perfect fairness and honesty, by the foresight of the result: but at other times such expectations may be equally or more prejudicial; and they probably were so in the instance before us. And it is at any rate a possible suggestion that, with regard to some of these supposed changes, Schröter's ideas may have been unconsciously biassed by his study of the surface of Jupiter. The perspective foreshortening of that great globe having no effect on the aspect of its more conspicuous and familiar markings, from their equatorial direction, and being non-apparent in the transits of the shadows of the satellites, the eye may come to regard it too much as a flat disc, and to appreciate too little the extensive changes which mere foreshortening produces among markings arranged in any other direction.

But whatever explanation may be attempted of Schröter's illusion, which, as his editor remarks, increases the value of his figures by securing their perfect independence, or however we may regret an apparent want in some cases of more distinctive detail, there can be no doubt that the work is worthy of attentive study. Dr. Terby has pointed out one curious inference—that at that epoch certain dark markings bore a relative proportion to each other, too different from that which obtains at present to be easily explained away. Nor does this result by any means stand alone: and it has considerable value as affording

collateral testimony to subsequent observations pointing in the same direction. To say nothing of other authorities, the accurate designs of the deeply-regretted Burton, and the latest delineations of Schiaparelli (independent of the wonderful duplication of the narrow streaks) concur with the drawings of Schröter in indicating one of two suppositions as regards the dark patches of Mars; either they must be liable to long-persistent and very deceptive alteration of visible outline from atmospheric causes, or their own extent must be so variable as to awaken a doubt whether the right key of the mystery is after all in our hands. We have long believed that we hold it, and terrestrial analogy has been thought sufficient to account for all we can see. The result of the next opposition in 1884 may be found to confirm the old hypothesis; but it is not beyond possibility that it may shake it, even past recovery.

Besides the conclusion thus briefly indicated, several other points of varying degrees of interest are touched upon in this comparatively bulky treatise, some of which we may refer to, though only with a passing notice. Schröter paid considerable attention to the polar whiteness; but while he admits the probable analogy of terrestrial snow, he is less confident than some other observers as to any marked influence of solar radiation. Terby, however, has pointed out the cause of his misapprehension, and his substantial agreement with his compeers. He was aware of the irregular outline of the snowy regions, and thought them slightly different in colour, the south pole verging towards yellow, the north blue. On the question of rotation he could obtain no satisfactory result, as might have been expected from his idea as to the instability of the markings; his values being discordant at different periods: the mean, 24h. 39m. 50·2s., was only about 29s. less than that of Sir W. Herschel, but very wide of Proctor's elaborately deduced value, 24h. 37m. 22·735s.—a fact pointing probably to the same conclusion as before, that from some as yet imperfectly explained cause, the exact position in longitude of some of the features of the planet is not fully ascertained. The amount of the polar flattening of Mars is, as is well known, matter of much uncertainty. Herschel made it as much 1-16; Dawes, nothing, or even negative. Our observer, nearest to the great English authority, found it less than 1-81, a quantity fairly evanescent. The method of measurement which he adopted throughout all his researches was that of the apparatus which he calls the "projection machine." In this simple



contrivance both eyes are employed simultaneously, the one in viewing the telescopic image, the other in bringing to coincidence with it, a squared-out area in the case of the moon, a series of discs for the planets, in either instance with provision for varying distance and illumination. This binocular mode of measurement, if open to some sources of error not incidental to the ordinary apparatus, appears hardly deserving of the censure so freely bestowed upon it by Beer and Mädler, who were not always fair towards the labours of Schröter; and notwithstanding the perfection to which the wire micrometer has been brought, might perhaps be revived for some purposes with advantage. The diameter of Mars obtained in this way by Schröter, 9″·84, does not differ much from the 9″ 8‴ (a curious instance, by the way, of notation by *thirds*), of Sir W. Herschel, or from more modern values—some proof, it may be thought, of the competency of the apparatus to obtain a close approximation.

Observations included in this volume of a partial flattening of the limb of Mars and of the abnormal breadth and want of symmetry in the phasis, however improbable they may at first appear, are not without parallel in the case of other planets, or the experience of other observers. If, as it must be assumed, these are nothing more than illusions, the record of them is still valuable in the probable event of their occasional recurrence.

To this brief and imperfect notice are appended three sketches from the Atlas—the two first as specimens of Schröter's mode of delineation—the third as bearing so striking a resemblance to one of my own, shown on a smaller scale beside it, that it might, in the absence of more accurate data, serve as the basis of an approximate value of rotation.

The dates respectively corresponding to these designs are 1798, Sept. 9d. 8h. 4m.; Oct. 17d. 7h. 39m.; Nov. 13d. 7h. 10m.; 1862, Dec. 10d. 9d. 30m.

T. W. WEBB

DESTRUCTION OF LIFE IN INDIA BY POISONOUS SNAKES

IN January, 1870, being then in Calcutta, I collected statistical information which afforded proof that the loss of human as well as animal life in India from the bite of venomous snakes was very great; and as it seemed to me that this ought to be, to a great extent, preventible, I extended my investigations with the view of obtaining accurate information as to the characters and peculiarities of the venomous snakes themselves, the localities in which they most abound; the *modus operandi*

of the poison; the circumstances under which the bites are inflicted; the value of any known remedies in the treatment of those bitten, and what measures might possibly be devised for diminishing this serious evil.

After a long and careful investigation of the whole subject, I drew up a detailed report, containing the results of my inquiry, and presented it to the Government of India, with a request that, when published, it should be distributed throughout India, among civil and medical officers, with a view of enabling them to take measures for the protection of human life, and the destruction of the creatures which caused such frightful mortality. I also endeavoured to point out the mode in which the poison destroys life, and to indicate such rational measures as might be of service in the treatment of those bitten.

I am not aware how far the advice I then tendered has been acted on, but I am glad to find, by a recent resolution published in the *Gazette of India*, that some progress is being made, and that the mortality of 1881 has been somewhat less than that of 1880, from this cause, and that this desirable result is due to the measures that have been taken by Government to procure the destruction of the poisonous snakes.

From the returns furnished to me at the instance of Government in 1870, for the year 1869, I made out that the human deaths from snake-bite were as follows in—

Bengal, including Assam and Orissa	6645
North-West Provinces	1995
Punjab	755
Oude	1205
Central Provinces	606
Central India	90
British Burmah	120
Total... ...	11,416

These were the only returns received, and represent not much more than half of the whole area, but the total, large as it is, cannot be regarded as the real mortality in these provinces, as the information from which the records were framed being probably only partial and imperfect, it rather under-rates than exaggerates the mortality. I expressed a belief that if systematic registration were adopted, the number recorded would prove to be larger, whilst, if information were gathered from the whole of Hindostan, it would be found that not less than 20,000 persons are destroyed annually by snakes.

Certain suggestions were made as to measures for identification, destruction of venomous snakes, and for registration of deaths. These would appear, from the terms of the resolution above referred to, to have been partially adopted, with the result of causing some diminution of the evil. I pointed out that the snakes which are so destructive to life are the cobra, the bungarus or krait, the echis, and the daboia or Russells' viper, all of which are most conspicuous snakes, and easily identified. There are others, such as Bungarus fasciatus, Ophiophagus elaps, which are dangerous, but comparatively rare, and seldom bite men, whilst the hydrophidæ being confined to the sea or estuaries, are, though very poisonous, not so dangerous to man, and the trimeresuri, which are both uncommon, and at the same time are not so deadly as to endanger life. All these are depicted in coloured figures taken from life, which renders their identification simple and easy.

I further remarked that, "meanwhile there exists the obvious necessity of endeavouring to prevent the numerous fatal accidents by making generally known the appearance and habits of the poisonous snakes, and by instituting rewards for their destruction. With a plain description and a faithful representation of each species in colours, every district, medical or police officer, would be able at once to distinguish the venomous from the innocent snakes, and thus knowledge enough, at least for all practical purposes, might be imparted to intelligent native subordinates, to enable them to recognise the poisonous snakes. By offering a larger reward for these only, their numbers would soon diminish, and the people would be made acquainted with the characters that distinguish the venomous from the harmless snakes, and would learn to avoid them. Thus only, I believe, can the evil be remedied, so long, at all events, as the mode of life among the lower and agricultural classes remain what it now is. I would suggest that magistrates, district and police officers, and civil surgeons be authorised to give the following rewards for poisonous snakes:—

	Annas[1]
Cobra	8
Bungarus cæruleus	6
Bungarus fasciatus	4
Ophiophagus	8
Russell's Viper	8
Echis	4
Trimeresurus	2

The sum disbursed would no doubt be large, but the results in the saving of life and destruction of snakes would compensate for the expenditure."

Such was the state of things when I left India in 1872. The Government of India then, at my instance, appointed a commission to continue the inquiry which I had commenced three or four years previously. This resulted in several valuable reports by Drs. J. Ewart, A. Wall, and Mr. Vincent Richards, whilst, in conjunction with Dr. Lauder Brunton, F.R.S., an investigation into the nature of the physiological action of the virus was continued here by me, the results of which have been published in the *Proceedings* of the Royal Society in 1873, 1874, and 1875. Meanwhile the evil continues, and it is probably within the mark to say that, since the subject came under consideration in 1870, 150,000 to 200,000 human beings, to say nothing of domestic animals, have been destroyed by snake bites.

The subject has often received the most anxious consideration of the Indian Government, and a variety of measures have been resorted to, not without a certain measure of success; but it is my belief that not until a system of organised, determined, and sustained efforts for the destruction of the snakes is adopted and carried out on the lines suggested in my report, will the evil be fairly grappled with and overcome. The present resolution shows that the matter is again receiving some consideration, and there is good reason to believe that if the measures be prosecuted with energy and determination throughout India, good results will follow. But I repeat it is only by the *destruction of the snakes* that the evil can be mitigated. Something may be expected from the people themselves as their knowledge of the subject increases, as they become more familiar with the appearance or character of the venomous as distinguished from the harmless snakes, and as they gradually become convinced of the futility of all antidotes charms or spells to protect them; or should they ever alter their present mode of living in huts which have the floor on the ground surface, to huts with raised floors—a consummation devoutly to be wished, not only on account of snakes, but of malaria—but hardly likely to be realised.

For the purpose of hunting out and destroying the evil it is absolutely necessary that a fixed system of rewards should be established, and that in every district there should be an organised body of men whose duty it would be, under proper supervision, to seek out and destroy the snakes, receiving a recompense according to the importance and number of the snakes killed. Such men are to be found among certain castes, and with the aid of descriptions and coloured drawings, such as now are available, there need be no great difficulty in carrying out this much-to-be-desired object. That such a project would be costly is true, but can that cost be considered excessive

[1] Eight annas represent one shilling.

if it save thousands of lives of men and valuable animals? There can be little doubt that wherever such a system has been even partially carried out, it has been effective; it needs but combined effort to make universal, that which hitherto would appear to have been but partial success.

From the tenor of the Government resolution referred to, it seems as though an organised scheme for the destruction of venomous snakes, as well as dangerous wild animals, is now likely to be generally adopted in India, and should it be so, there is good ground for hope that the great mortality will decrease—to quote from a former paper on this subject, I would repeat: "Rewards should be offered freely for venomous snakes only. This, if steadily carried out under some responsible official, would soon diminish snakes and deaths from snake-bite; and I earnestly protest against the opinion expressed by some Indian authorities, that such rewards are useless—useless they may have been, and will continue to be, if distributed without discretion for snakes not poisonous. If this method of dealing with the matter—and who can deny its importance—be adopted (but it must be done willingly, and not with the foregone conclusion that it will fail), I am certain that, as part of a comprehensive scheme for the destruction of noxious animals generally, it will succeed."

The following is the purport of the resolution of November 8, 1882, which shows that in 1881 the number of deaths caused by snake-bite, of men and animals, contrasted favourably with that of the previous year, 1880.

The statement appended to this resolution shows in detail for each province the number of persons and cattle killed by wild animals and snakes, and the number of wild animals and snakes destroyed, with the rewards paid for their destruction during the year 1881, as compared with the previous year. The figures are summarised in the following tables:—

Number of Human Beings and Cattle Killed by Snakes

	Persons killed.		Cattle killed.	
	1880.	1881.	1880.	1881.
Madras	1,182 ...	1,064 ...	227 ...	273
Bombay	972 ...	1,024 ...	89 ...	191
Bengal	10,064 ...	9,208 ...	1,248 ...	154
North-Western Provinces and Oudh ...	4,723 ...	5,010 ...	221 ...	317
Punjab	681 ...	744 ...	78 ...	69
Central Provinces ...	901 ...	985 ...	39 ...	26
British Burma	149 ...	135 ...	194 ...	150
Coorg	3 ...	Nil ...	Nil ...	Nil
Assam	211 ...	189 ...	57 ...	16
Hyderabad Assigned Districts	125 ...	197 ...	383 ...	836
Ajmere-Merwara ...	49 ...	54 ...	Nil ...	Nil
Total	19,060 ...	18,610 ...	2,536 ...	2,032

Snakes killed and Rewards Paid

	Destroyed.	Rewards.	Destroyed.	Rewards.
	1880.		1881.	
		Rs. a. p.		Rs. a. p.
Madras	Nil ...	Nil ...	Nil ...	Nil
Bombay	177,078 ...	6,922 3 6 ...	207,113 ...	6,214 0 0
Bengal	23,201 ...	3,733 3 6 ...	19,282 ...	3,430 5 0
N.-W. Provinces and Oudh...	1,029 ...	10 2 0 ...	1,142 ...	56 5 3
Punjab.....	9,126 ...	635 5 0 ...	22,279 ...	1,587 4 0
Cent. Provinces	866 ...	336 6 0 ...	1,493 ...	562 8 0
British Burma.	997 ...	2 0 0 ...	2,990 ...	27 0 0
Coorg.....	58 ...	Nil ...	16 ...	4 0 0
Assam.....	202 ...	Nil ...	300 ...	34 0 0
Hyderabad Assigned districts	158 ...	23 14 0 ...	332 ...	45 8 0
Ajmere-Merwara	61 ...	Nil ...	21 ...	Nil
Total ...	212,776 ...	11,663 2 0 ...	254,968 ...	11,960 14 3

The deaths of human beings from snake-bite were, in 1880, 19,060; while in 1881 they were 18,610.

In 1880, 212,776 snakes were destroyed at a cost of Rs 11,663.

In 1881, 254,968 snakes were destroyed at a cost of Rs 11,961.

Thus with an increased expenditure of Rs 298 in 1881, 42,192 more snakes were destroyed and 450 lives were preserved, above the expenditure of the previous years.

With regard to the measures adopted for the destruction of venomous snakes, the following remarks are made by the Governor-General in Council:—

"As regards the destruction of venomous snakes, special measures were adopted in some provinces, of which it appears desirable to give a brief account in case they may be considered suitable for adoption elsewhere. In Bengal a scheme has been sanctioned by the local Government in the case of the Patna Division, under which persons destroying snakes can obtain certificates from certain selected planters vouching for the poisonous nature of the snakes destroyed. The production of such a certificate entitles the holder to secure from the local authorities the reward offered whenever he finds an opportunity of applying for it. As observed by the Government of Bengal, this concession will probably be found to add much to the convenience of persons claiming rewards, and to act as an inducement towards the destruction of poisonous snakes. The expediency of extending the scheme will be considered by the Local Government when the result of the current year's operations are known. In the North-Western Provinces and Oudh the Lieutenant-Governor and Chief Commissioner has sanctioned the entertainment tentatively in each district of those provinces of a staff of Kanjars, or men of similar caste, who trap and kill reptiles, for the systematic destruction of venomous snakes. These men will receive pay at the rate of Rs. 2 per mensem, together with an additional reward of two annas for every venomous snake in excess of twenty destroyed by each man during any month. A gang of snake-hunters is also to be employed at each tahsili, and, if the measure proves successful, it is proposed that similar gangs should be eventually appointed to each police circle of other local area. It appears to the Governor General in Council that a plan for the destruction of snakes such as that initiated in the North-Western Provinces and Oudh, is likely to prove far more efficacious than the mere offer of rewards, although it is true that unless such operations are confined to towns and villages and their neighbourhood, where it is believed that the largest number of deaths occur from snake-bite, they will probably be very costly. His Honour the Lieutenant-Governor of the Punjab has issued a circular to commissioners and superintendents in the Punjab, drawing attention to the matter with a view to the adoption of measures for destroying snakes by system of rewards to be granted by district committees and municipalities. Casts and lithographed pictures of the more common species of deadly snakes have already been supplied to the police stations in some districts, and deputy commissioners have been requested to suggest to municipal and district committees the desirability of procuring similar means of reference for the purpose of testing applications for rewards. In British Burma the Chief Commissioner, with a view to encourage village snake-hunts in the rice plains, has arranged to grant sums varying from Rs 10 to Rs 20, according to the number of houses, in aid of a feast or *pweh* at the end of the annual hunt to every village which successfully carries out such an undertaking.

"On the whole, the results recorded during the year under review appear to the Government of India to be more satisfactory than those of the previous year. The Governor General in Council is glad to notice that the question of taking measures to reduce the lamentable loss of life which is at present caused by wild animals and venomous snakes is receiving the earnest consideration of Local Governments and Administrations, and His

Excellency in Council will await with interest the reports showing the results of the special measures which have been adopted in some provinces. It is clear that much still remains to be done; but if sustained efforts are made and well-considered plans adopted for the extermination of wild beasts and deadly snakes, His Excellency in Council believes that the number of deaths from these causes will in course of time be materially reduced.—Simla, November 8, 1882."

From the above it appears that more vigorous measures than any hitherto adopted have been taken for the destruction of venomous snakes, and the contrast of the results of 1881 with those of 1880, warrant the anticipation of further benefit if these measures are only carried out with a sustained determination to succeed. It is mainly a question of perseverance and the expenditure of money, and one can hardly imagine a more desirable object on which to expend both energy and rupees. But it is essential that the system be laid down on some general principles for the whole of India, to be worked out in detail, according to the needs or peculiarities of each district. There should, in short, be a department with a responsible chief and subordinate agents, for whom certain rules should be laid down to be carried out steadily and without hindrance throughout the country, leaving much of the detail to the discretion of local authorities. I would insist on the importance of carrying it out on broad principles everywhere. When such a department is constituted under a proper head—and there are many persons well fitted for such a duty—then, I believe, venomous snakes and other noxious animals will decrease in numbers, and people will cease to be startled by these appalling losses of life. J. FAYRER

SIR J. WHITWORTH'S MECHANICAL PAPERS[1]

THE fact that, by an order in Council of August 26, 1881, some 300 Whitworth gauges of various dimensions have been adopted as standards by the Board of Trade, is so important a recognition of the value of the labours of Sir J. Whitworth in improving mechanical measurement, that the occasion has been selected for republishing certain papers which have been long well known among engineers, but which have not hitherto been accessible to the public generally.

The first paper in the series is on plane metallic surfaces, and the proper mode of preparing them, and contains an account of an invention of great simplicity, but of the highest practical importance. Such plates, when worked up to an extreme degree of accuracy and finish, form an approximation to a plane surface which would surprise and delight any geometrician who had an opportunity of critically examining and testing their qualities. They consist of an assemblage of minute bright surfaces very evenly distributed over a plate of cast iron, and very near together.

As to their qualities, there is not space here to describe them, but they have formed the subject-matter of an excellent lecture by Prof. Tyndall, at an evening meeting of the Royal Institution in the year 1875.

Passing from these so-called true planes, we refer to a step involving an original conception which has led to the construction of the new standard gauges. The production of an approximately true plane surface gave an increased value and importance to the feeling of contact between prepared metallic surfaces, and resulted in the invention of a measuring machine which was made to depend on the sense of touch instead of upon optical contrivances, and was founded entirely on truth of surface.

[1] "Papers on Mechanical Subjects." By Sir Joseph Whitworth, Bart., F.R.S., D.C.L., Vol. I. True Planes, Screw Threads, and Standard Measures. (London: Spon.)

The improvement consisted in the substitution of end for line measure, and inasmuch as these are technical terms, it may be well to explain them.

As stated in the last paper of the series, the English standard yard is an example of line measure, being represented by the interval between two lines drawn across two gold studs sunk in a bronze bar about 38 inches long, the temperature being at 62° Fahrenheit.

The standard yard, from the subdivisions of which the standard inch has been obtained on the Whitworth system, is a rectangular metal bar with plane sides capable of resting along its whole length in rectangular V grooves, which are plane surfaces, while the ends of the bar are planes lying perpendicular to its axis. The bar is exactly 36 inches long, and the measurement is complete when the degree of contact between its ends and two small true planes abutting against them is ascertained. Such a measurement is, of course, end measure, and its accuracy depends throughout upon truth of surface, and also upon truth of position of surface. The ends of the bar must be perpendicular to its axis, and the planes which feel those ends must be truly parallel to each other, and one at least must be movable to and fro without deviating at all from the position of parallelism to its fixed neighbour,

Then comes the question of the amount of shifting of the movable plane. That is done by a micrometer screw, the linear motion for one graduation of the micrometer head, which can be easily read without a lens, being in some cases 1-10,000th of an inch, and in other cases 1-1,000,000th of an inch.

There is not space to discuss the measuring machine, whether as capable of producing cylindrical gauges varying by 1-10,000th of an inch, or as capable of reproducing a standard inch or a standard yard to a degree of accuracy which leaves the microscope far behind in the contest.

It must suffice to point out that the reprinted papers are full of interest, as showing the manner in which Sir J. Whitworth has thought out and accomplished the work of improving the construction of machinery, and it is matter of regret that those who are occupied in teaching mechanics have not better opportunities than now exist of becoming practically conversant with the subject-matter of the collected papers.

NOTES

FROM Punta Arenas, near the extremity of South America, intelligence has been received that the fourth section of the German expedition sent out to observe the transit of Venus has been particularly successful, Professor Auvers having managed to take exceedingly good photographs and numerous measurements.

A TELEGRAM received from Monte Video states that the *Volage* has anchored in these roads from Santa Cruz in Patagonia. Capt. Fleuriais and observers of the transit of Venus were on board, returning to France with their instruments, photographs, and other documents.

M. TRÉPIED, in a communication to the Paris Academy on his observation of the transit in Algiers, states that clouds rendered the ordinary observations of little value, but that some good results were obtained with the spectroscope on the borders of the planet in the region from A to E; while some photographs were obtained in the green, the blue, and the violet. The examination of the spectral lines in the groups A, B, α, in the regions comprised between α, D, E, did not show, M. Trépied states, anything which could be attributed to a selective absorption produced by an atmosphere on the planet. The same inference is deduced from the photographs.

AT a recent conference of members of the British Association, held in the rooms of the Geographical Society, a protest was drawn up against the proposed meeting of the Association in Canada in 1884.

AT the Conference of Head Masters the other day one of the subjects discussed was the teaching of geography in schools, in which many varied and vague opinions were expressed. There is in this country too great a tendency to treat geography as mere topography, the mere dry bones of the subject, which can only be clothed with flesh and endowed with life through the medium of the physical and natural sciences. We advise those teachers who desire to make the subject of geography both interesting and useful to make themselves acquainted with the programmes of German schools and universities under that head.

THE Société d'Encouragement has held its annual meeting for 1882, and awarded its gold medal to M. Gaston Planté for his work in the accumulation of electricity.

IN a recent number of the St. Petersburg Academy's *Bulletin* (1882, t. xxviii. p. 163), Herr Kortazzi reports on his observations of Jupiter at Nikolajus from September, 1879, to December, 1881, giving, in four plates, forty-seven drawings of the planet. The time of rotation, calculated from the red spot, he finds to have continually diminished, but not according to an ascertained law; more recently an increase has appeared. The spot can hardly (he considers) be regarded as gaseous; it is more likely a liquid or even solid mass forming part of the planet's surface. In the former case it might be considered a large lake in an ocean of other liquid, which covers the southern hemisphere of Jupiter, and it might be expected that this lake, owing to currents flowing over the surface of the planet, would gradually be diffused and spread over the whole surface, or at least over the whole parallel. If the spot were a solid projection from the solid body of the planet (if such a body there be), it would be impossible to account for the observed changes of position. The most plausible view is, that it is a solid floating mass on the surface of an ocean; but even this hypothesis the author considers bold, since we are not entitled to infer by analogy from terrestrial phenomena the nature of forms on Jupiter which may be very different in internal nature from the earth.

A DOE having horns, which gave it the appearance of the male animal, was recently killed in the woods of Herr Pönsgen, near Aix-la-Chapelle. The longer horn was about 19 centimetres in length. Such a case is rare, though small rudiments of horns are sometimes met with in old does. A picture of the animal is given in the *Revista Scientifico-Industriale*, October 31.

TO illustrate the effect of expansion of the bulbs of liquid thermometers on the indications of those instruments, Prof. Govi connects a capillary tube with a bulb of ebonite, and partly fills it with mercury. Such a thermometer does not indicate gradual variations of temperature (within certain limits). With rapid variations, the mercury shows *inverse* movement (descending with heat and rising with cold), but after some time the original level is restored, while the excess of heat or cold is lost. The phenomena are due to the almost perfect equality of the coefficients of cubical dilatation of ebonite and mercury, at least between zero and 50° or 60° C.; and to the fact, that with sudden changes of temperature, the bulb responds first, and being a bar conductor, transmits slowly to the mercury.

A RECENT report by M. de Bezerédy, Government Commissioner for cultivation of silk in Hungary, shows that the industry is making considerable progress in that country. In 1881 there were 2976 producers, who obtained 41,537 kilogrammes of cocoons in 426 communes, and the produce was sold for 41,816 florins. The corresponding figures for 1880 are: 1059 producers, 10,132 kgr., 109 communes, and 11,062 florins. The Commissioner sold in Italy the produce of 1881 for 63,000 florins, and the profit so realised allowed of the institution of a model school for silk-cultivation, without exceeding the credit voted by the Chamber. This school has received three primary teachers sent by the Minister of Public Education, and three from the Minister of Commerce; three more are maintained at private expense. These nine will acquire knowledge to be afterwards utilised in their place of residence. Further, a professor in the Model School of Graz has given public lectures on the rearing of silkworms in several villages, and more than 80 kgr. of cocoons have been distributed continuously to cultivators. Lastly, 28,956 mulberry trees have been planted at Government expense. The report recommends the establishment of spinning mills in the country, and the plantation of mulberry trees on land belonging to the communes, and on the Government roads. The climate of certain regions of Hungary is highly favourable to the production of silk.

THE *Times* Geneva correspondent states that the recent heavy rains, which recommenced on Friday, with, if possible, greater violence than before, are producing disastrous consequences in various parts of Switzerland. A considerable extent of ground, covered with vines, at Espesses, in Canton Vaud, is slipping rapidly towards Lake Leman, and, unless the measures taken by the engineers succeed in arresting its progress, must soon be engulfed. An earthslip has also taken place near Troistorrents, and another at Pully, in the same neighbourhood. Up to the end of November there had been 200 rainy days in that part of Switzerland since the beginning of the year, and only 50 days of sunshine.

AN international exhibition will be held in Calcutta next December. There will be nine principal sections: (1) fine arts; (2) apparatus and application of the liberal arts; (3) furniture and objects used in dwellings; (4) clothing, including fabrics; (5) products of mining industry, forestry, &c.; (6) apparatus and processes in the common arts; (7) food; (8) artisans' workmanship; and (9) children's work. An attempt will also be made to hold an exhibition of live stock, agricultural and horticultural products, and of a loan collection of paintings, sculpture, and works of art generally. The usual gold, silver, and bronze medals will be awarded by special juries of experts. The exhibition will be opened on December 4, 1883, and will close on February 29, 1884.

M. GERMER BALLIÈRE has published an edition of Father Secchi's "Les Étoiles" in two volumes, as a part of the Bibliothèque Scientifique Internationale.

THE Academy of Moral and Political Sciences has announced the conditions of the competition opened every year for the prize of 200*l.* to be devoted to the author of the work, which is to "faire aimer," morality and virtue, and "faire repousser," vice and egotism.

M. DE CHANGY, the first electrician who attempted to manufacture incandescent lamps *in vacuo* about twenty years ago, has constructed a small model for demonstration. The carbon is rectilinear, which permits a very small length to be given to it. It is to be lighted with bichromate of potassium elements. In his former attempts M. de Changy advocated very small carbons cut in the graphite from the retorts. Now his fibres are carbonised according to the common practice.

THE introduction of western improvements into China by Europeans is evidently a work beset with many difficulties. Some years ago the only railway in the country was purchased by the Government from the proprietors and promptly torn up; but now the officials themselves are laying down railroads from the mines in North China to the nearest canal. The telegraph also

had to encounter a vigorous opposition from the authorities and people for many years; at present, however, the capital is connected by wire with the coast. The electric light is the latest improvement which has excited the suspicion and dislike of the Mandarins. The foreign settlement at Shanghai has for some time been lighted on the Brush system, apparently much to the comfort and jubilation of the denizens of the "model settlement," as the foreign portion of the city is generally called. The promoters appear, however, to have reckoned without the Chinese officials. They probably thought that where gas was permitted, there could be no objection to electricity. The Chinese Governor of the district appears to be of a different opinion. He has addressed a letter to the senior Foreign Consul requesting the removal of all the electric lamps. He has read, he says, in translations from European papers, that terrible accidents have arisen from electricity, and flatly refuses to permit the residents of Shanghai to be exposed to such dreadful risks. Hundreds of thousands of houses might be destroyed, millions of lives might be lost; even the walls of the city might be blown down if anything went wrong with the machines. He has strictly forbidden his own countrymen to use it, and has peremptorily ordered those who have already adopted it to discontinue it forthwith. Whether this ukase will be immediately obeyed or not it is impossible to say; but past experience leads us to the conclusion that if the Chinese have determined to set their face against the electric light, no power on earth can get them to permit it in their territory. Their leading principle in these matters seems to be a dislike of all innovation until its necessity is clearly demonstrated by *their own* experience, and a determination that new inventions or appliances shall not be foisted or forced on them from outside. The late difficulty with Russia showed them the imperative necessity of being prepared for war, and of having their capital in direct communication with the outer world. Ironclad ships and rifled guns are accordingly being purchased with extraordinary rapidity; forts are being erected at various points on the seaboard, and a telegraph line about 800 miles in length was constructed in the course of a few months. Perhaps, after all, the Chinese policy in this respect is not so wrong-headed as it sometimes appears. It certainly saves them from the wiles of speculators and promoters of all sorts.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus radiatus* ♂ ♀) from India, presented by Mr. Nathaniel Cotton; two Slender Loris (*Loris gracilis*) from India, presented by Dr. H. W. Lentaigne; a Leopard (*Felis pardus*) from India, presented by Capt. Park; a Crimson-crowned Weaver Bird (*Euplectes flammiceps*) from Madeira, presented by Mr. E. W. Gain; a Common Heron (*Ardea cinerea*) from Scotland, presented by Mr. W. H. Henderson; eleven Muscovy Ducks (*Cairina moschata*) from South America, presented by Major Finlay; a Hoary Snake (*Coronella cana*), a Crossed Snake (*Psammophis crucifer*), a Rhomb-marked Snake *Psammophylax rhombeatus*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two Golden-winged Woodpeckers (*Colaptes auratus*) from North America, purchased; a Golden-Eye (*Clangula glaucion* ♂), British, on approval; a Molucca Deer *Cervus moluccensis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

STELLAR PARALLAX.—The results of a series of observations with the filar micromerer on the Washington refractor for the determination of the annual parallax of α Lyræ and 61 Cygni have been printed in advance of the publication of the yearly volume of observations. The measures were made by Prof. Asaph Hall, those of α Lyræ extending from May 24, 1880, to July 2, 1881, on seventy-seven nights, and those of 61 Cygni from October 24, 1880, to December 7, 1881, on sixty-six nights. The magnifying power employed was 383. Prof. Hall remarks that since observations of the angle of position made with the micrometer-circle are less accurate for distances that enter into the determination of parallax, he observed simply the difference of declination of α Lyræ and the companion of the tenth magnitude, and in the case of 61 Cygni the difference of declination of the smaller component and a star of 9·5 magnitude about 3′·3 south of the double star, which is D.M. + 38°, No. 4345. α Lyræ was observed both with bright and dark wires, for 61 Cygni only the dark wires were used. It may be noted that the star measured is the following component of the double star. The course of observation pursued for each night's set of measures is described, and except on one occasion the same programme was followed throughout.

The resulting parallax for α Lyræ is, 0″·1797 ± 0″·00561; the time required for light to pass from the star to our sun is thus found to be 18·11 Julian years.

For 61 Cygni the parallax is 0″.4783 ± 0″·01381, and light requires 6·803 Julian years to traverse the space that separates this star from the sun.

The parallaxes it will be seen, are obtained by the differential method, and are thus relative, or they are the differences of the parallaxes of the two stars. To get the absolute parallax of the bright star it is necessary to add the parallax of the small star. Prof. Hall says that he might have effected this by means of the parallaxes for stars of different magnitudes given in Struve's table in his "Études d'Astronomie Stellaire," but as the whole matter is uncertain, he has omitted this reduction.

Dr. Ball, Astronomer Royal for Ireland, continues his researches on stellar parallax, at the Observatory of Dunsink, Dublin, and has lately published a determination of the parallax of 6 (Bode) Cygni, which is the well-known double star No. 2486 of Struve. The components are of 6 and 6·5 magnitudes. The existence of a parallax to a very measurable amount was suggested during the course of a series of preliminary observations in 1879 and 1880 for the detection of such proximate objects, and a systematic course of observation was commenced on October 3, 1880, and continued to December 22, 1881. 6 B. Cygni is No. 196 of Argelander's list of 250 stars having large proper motion, given in vol. vii. of the Bonn Observations, where it has attributed to it an annual motion of 0″·636 on an angle of 346° 27′; Argelander's positions belong to the preceding component. Dr. Ball has employed the following one in his investigation. Measures were obtained on twenty-six nights, the mean date being 1881·5207; they were made from a star *n.f.* of the 10·5 magnitude, the adopted mean distance of which is 170″·692.. and position angle 78° 18′ 61″. If this small star is assumed to be at rest, and Argelander's proper motion attributed to the double star, the annual increase of distance is + 0″·02, and that of angle + 12′·796; thus almost the whole proper motion applies to change in the position angle. The observations show that there is no regular increase of distance, and hence, Dr. Ball observes, there is *primâ facie* evidence that the comparison star does not participate in the proper motion.

The resulting parallax of Σ 2486 is—

From the distances	0″·5039 ± 0″·060
From the angles	0″·383 ± 0″·13

Combining these two values, we have for the parallax 0″·482 ± 0″·054. It is intended to make another series of observations of this star, the present result being regarded by Dr. Ball as merely provisional, though he thinks it can hardly be doubted that a parallax of very considerable amount really exists. The place of the star is in right ascension 19h. 8m. 20s., with 49° 35′·3 north declination for 1855·0.

COMET 1882 *b*.—The following positions of this comet for midnight at Greenwich, though liable to an error of several minutes of arc, may serve for finding it in the telescope without difficulty.

	Right Ascension. h. m.	Declination. ° ′	Log. distance from Earth.	Log. distance from Sun.
Dec. 31	7 14·9	−29 28	0·2365	0·3883
Jan. 2	7 9·2	29 10		
4	7 3·7	28 49	0·2489	0·3991
6	6 58·4	28 26		
8	6 53·3	28 2	0·2622	0·4096
10	6 48·4	27 36		
12	6 43·8	27 8	0·2763	0·4196
14	6 39·4	26 40		
16	6 35·2	26 10	0·2912	0·4294
18	6 31·2	25 40		
20	6 27·4	−25 9	0·3067	0·4388

AMERICAN RESEARCHES ON WATER-ANALYSIS

AT the instance of the U.S. National Board of Health, a comprehensive investigation relating to analysis of water has been recently conducted by Prof. J. W. Mallet, F.R.S., of Virginia, assisted by several chemists and others. It was proposed to examine carefully the chief processes in use for chemically determining the organic matter or its constituents, in drinking water, to test the absolute and relative accuracy of the results these processes are capable of yielding, and, as far as possible, to ascertain the nature and scope of the practical conclusions available for sanitary purposes, which might thence be secured.

A preliminary report of this inquiry has appeared in a recent supplement (No. 19) of the National Board of Health *Bulletin*, and we propose here to give our readers some idea of the nature of it.

Three chemists took charge severally of the so-called "combustion process" of Frankland and Armstrong, the "albuminoid ammonia process" of Wanklyn, Chapman and Smith, and the permanganate process as advocated by Tidy; they were, Mr. W. A. Noyes, who worked at Baltimore, Dr. Ch. Smart, U.S.A., at Washington, and Dr. J. A. Tanner, U.S.N., at Virginia. The water-samples were collected and distributed to these three places by Prof. Mallet, and all three chemists were required to examine their several samples on the same day. Prof. H. Newell Martin undertook a simultaneous microscopic examination, and pathological observations on the effect of injecting the waters, concentrated by evaporation at a very low temperature, under the skin of rabbits.

After a large amount of preliminary and special work, nine classes of waters were obtained or prepared, for the main series of test analyses, as follows :—

Class I.—Natural waters, believed to be wholesome (including the water-supply of some of the principal cities).

Class II.—Natural waters which were believed to have actually caused disease in those who drank them.

Class III.—Natural waters of doubtful, but more or less suspected character.

Class IV.—Artificially prepared waters, made by adding to wholesome water, certain amounts of various infusions of *vegetable* organic matter, such as drinking water is liable to be contaminated with.

Class V.—Waters prepared with various forms of *vegetable* refuse, from manufacturing or industrial operations.

Class VI.—Waters prepared with *animal* (or partly animal) organic matter of natural origin.

Class VII.—Water prepared with *animal* refuse from manufacturing or industrial operations.

Class VIII.—Water prepared by adding morbid products of human disease.

Class IX.—Solutions, in distilled water, of carefully determined amounts of pure substances of definite chemical compositions.

The report first deals with the *degree of accuracy* of the three processes examined, a matter which may be looked at in two ways: first, as to the concordance of the results of each process in duplicate or triplicate experiments on the same water; and secondly, as to the agreement of the results with the actual, quantitatively known, contents of a particular water.

In the case of multiplied experiments on the same water, then, the author first shows in three successive tables (for the three processes), the divergence of individual results from the mean, as *percentage on the mean*. It appears that, on the whole, the most closely concordant results were furnished by the permanganate process, and the least so by the combustion process, the albuminoid-ammonia process holding the intermediate position.

Next, a table is given showing the extent of agreement of results obtained by the different processes with quantities of organic constituents known to be actually present. The figures strikingly indicate certain important defects of the several processes, though (it is pointed out) they must be looked at in a broad, general way, remembering the small number of organic substances treated and their special characters.

The Report proceeds to deal with the effects on the results by the different processes of varying the extent of dilution of the same organic substances in water. Here, as regards the combustion process, distinct confirmation is had of the existence of two forms of constant error affecting evaporation. The weaker the solution the less is the amount of organic carbon obtained, and the larger the figure for organic nitrogen. If the usual interpretation of Frankland's C : N ratio be applied, the curious and important result of these sources of error follows, that the more dilute an organically-polluted water is, the more animal-like in origin will its polluting material seem to be; while the stronger it is, the greater will be the tendency to refer the contamination to a vegetable source—a distortion of conclusions manifestly in the opposite direction to that commonly assumed to be safe.

As to the albuminoid ammonia process, the weaker the solutions, the higher are the results obtained, both for free and albuminoid ammonia, the influence common to both being probably that of imperfect condensation in the distillation having a less effect as the quantity of ammonia is stronger. The lower results from the stronger solutions for albuminoid ammonia, may be partly due to the fixed charge of alkaline permanganate to the quantity of organic matter to be acted upon.

The results of the permanganate process are shown to be much less influenced by varying dilution within the limits of those experiments than those of the other processes.

The following, briefly stated, are the author's

Special Conclusions as to the Combustion Process

1. The combustion itself, carried out according to Frankland's directions, is a process of great delicacy, and quite satisfactory in its details, with proper precautions, and in trained hands. Gaseous volumetric analysis with the aid of the Sprengel vacuum, is sufficient.

2. The Frankland process is quite within reach of the manipulative skill of any fairly-trained chemist, but it requires practice and probably pretty constant practice. It is better adapted for large public laboratories, where many samples of water are examined, than for occasional use by a private individual.

3. The defective point is the failure of the evaporation to leave a residue representing the original organic matter; this (suspected hitherto) is regarded as now established. There are two constantly present errors (greatest with little organic matter), viz., loss of carbon, and gain of nitrogen from the atmosphere, the latter probably partly balanced by loss of that originally present in the water.

4. These errors, affecting unequally the carbon and nitrogen, are liable to alter the statement of the C : N ratio, and so distort sanitary conclusions.

5. The result for organic nitrogen, by the combustion process, is also affected indirectly by the errors connected with determination of the ammonia ("free ammonia"), as the nitrogen belonging to this has to be subtracted from the gross result.

6. A further, indirectly operative, cause of error as to the nitrogen arises from the varying loss of ammonia by dissociation of its salts during evaporation. The time occupied in evaporation, as well as the amount of ammoniacal salts present, will influence the amount of loss. Such error will mostly be very small, but may affect considerably the result for *organic* nitrogen.

7. The presence of nitrates presents a special difficulty in the combustion method. Mr. W. Williams's application of the copper-zinc couple deserves more precise quantitative examination than it has yet had. The simultaneous presence of urea with nitrites and nitrates, presents a case of peculiar difficulty, with several sources of error.

8. The formation of sulphuric acid during evaporation with sulphurous acid seems to occur oftener than has been recognised. It is much to be deprecated, though its effect, at any rate on the carbon determinations, has not seemed so great as might have been expected.

9. The combustion process, in its present form, cannot be considered as determining the carbon and nitrogen in a sense to justify the claim of "absolute" value for its results, which has been denied to those of other methods. It is but a method of approximation, involving sundry errors, and in part, a balance of errors.

10. There is, however, good ground for believing, that in many, perhaps most cases, its results for organic carbon may, with proper precautions, be made more valuable than the indications of the permanganate process, and its results for organic nitrogen more valuable than the indications of the albuminoid ammonia process.

Special conclusions as to the Albuminoid-ammonia Process

1. In the determination of both "free" and "albuminoid" ammonia there is a loss, which may be quite considerable, from imperfect condensation of the ammonia during distillation. This varies especially with the efficiency of the cooling apparatus and the time occupied.

2. Where waters contain urea and other amidated bodies, such as the leucine and tyrosine of putrefactive decay, some ammonia is so easily formed from these substances by boiling with sodium carbonate, or even without this addition, that it is impossible to distinguish sharply between pre-existing "free" ammonia (of ammoniacal salts), and that formed by the action of alkaline permanganate, the so-called "albuminoid" ammonia. This source of error as to free ammonia reacts (as noticed above), on the result for organic nitrogen by the combustion process.

3. There is no satisfactory evidence for Wanklyn's view, that, in distilling with alkaline permanganate, definite and simple fractions of the nitrogen of organic matter are given off as "albuminoid" ammonia. Such results may be varied at pleasure for most substances, by modifying the conditions of distillation.

4. If the distillation with alkaline permanganate be carried out according to Wanklyn, the nitrogenous organic matter is often so gradually acted upon, as to make the ending of the process indefinite; ammonia is still coming off when the distillation has to be stopped, the contents of the retort being nearly dry. Here an unknown fraction of the possible amount of albuminoid ammonia fails to be collected, and is but vaguely indicated by + after the figures recorded.

5. There is evidence that in some cases nitrogenous matter is volatilised during the distillation for free ammonia, which, if it had been retained, would have yielded up its nitrogen as albuminoid ammonia. Not affecting the Nessler reagent, such matter escapes detection.

6. The albuminoid-ammonia process proper, *i.e.* distillation with alkaline permanganate and determination of the ammonia evolved, is admittedly simple, and easily carried out with very little preparatory training.

7. The value of the results by this process depends more on watching the *progress* and *rate* of evolution of the ammonia than upon determining its total amount.

8. The recorded results by this process show a good deal of similarity between the figures for albuminoid ammonia and those for organic nitrogen (by the combustion process), but with frequent discrepancies of varying extent, such as prevent the one being taken as the accurate measure of the other.

Special Conclusions as to the Permanganate Process

1. The results by the Tidy method (the acidified permanganate at common temperature) and that of Kubel (operating at boiling point) differ irregularly from each other, the latter usually giving much higher figures, as was to be expected, but the ratio between the results by the two methods varies much in different cases.

2. On the whole, there seems to be a nearer approach to proportionality with the quantities of organic carbon found by the combustion process on the part of the Kubel process than on that of the Tidy process, but to this there are some very notable exceptions.

3. In a good many cases, the Kubel results are, contrary to the general rule, lower than those by the Tidy method. This seems to be due to loss of organic matter by volatilisation with the escaping steam from the boiling liquid before time has been afforded for action on the permanganate. Of course, a similar loss may have occurred in other cases, but not to the extent of reversing the general rule; and this may in part explain the absence of any uniform ratio between the figures yielded by the two methods.

4. The results by the Tidy process are liable to variation with the atmospheric temperature at the time of operation.

5. The amount of oxygen consumed by a specimen of water is probably in all ordinary cases much below that required for complete oxidation of the organic matter present, and does not stand in any fixed ratio thereto; it cannot be taken either as a measure of the organic carbon or of the total organic matter. Still a distinct general resemblance may be traced between strongly marked results, high or low as the case may be, for the consumption of oxygen on the one hand, and organic carbon (by the combustion process) on the other, and closer agreement is observable regarding waters of generally similar character.

6. The permanganate process is capable of giving more valuable information in regard to a water by watching the *progress* and *rate* of the oxidation of organic matter present than by any single determination of the actual amount of oxygen consumed in a given time.

7. For such observation of the progress of oxidation, the two determinations prescribed by Tidy, viz. of oxygen consumed in one hour and three hours respectively, are not sufficient, nor is the latter period of three hours long enough to indicate the general behaviour of the water with the acid permanganate.

As to other chemical determinations, the discrepancies in those of total solids left on evaporation, of the loss on ignition of this solid residue, and even of chlorine, are noted as illustrating the comparative roughness of the methods with which those results were obtained when very small quantities have to be dealt with. A coincidence is often presented between alkaline reaction of a water and the occurrence of nitrites and nitrates in considerable quantity, suggesting a recollection of the conditions under which nitrates are produced on a large scale in the decay of nitrogenous organic matter. Those salts, however, also occur pretty largely in some cases without alkaline reaction, and in other cases there was alkaline reaction and also much nitrogenous matter, but no nitrites nor nitrates. Ammonium nitrates seem to be rare, the basis-constituent being rather mostly non-volatile—no doubt calcium, magnesium, or one of the alkaline metals; this is noticed as in relation to possible reduction to nitrite, and consequent loss of nitrogen in the combustion process. Experiments with tannin showed the utter worthlessness of this group-reagent of Kämmerer for the purpose for which he has advocated its use. The general series of analyses of dissolved gases illustrate how the results are influenced by varying conditions of oxidisability of organic matter present, temperature, extent of exposure to the atmosphere, and interchange with it, in both direction, of gaseous constituents.

With regard to the *microscopic and pathological results*, Prof. Mallet, feeling himself incompetent to properly discuss these, prefers leaving them to speak for themselves, and merely remarks on some of the difficulties of such research, and, at the same time, on its importance and value when rightly conducted.

Passing now to sanitary conclusions and interpretation of results, the Report deals with

Chemical and Biological Results as contrasted with the actual Sanitary History of the Natural Waters Examined

Now on inspection of the tabulated results it appears that *no strongly marked generic difference is presented by the results from any of the processes for estimation of organic matter or its elements between the generally wholesome waters of Class I. and the waters of Class II. medically condemned and fairly assumed as pernicious.* This applies equally to the highest, the lowest, and the average results. No one could, with those figures to guide him, refer a water of unknown origin to one or other of the two classes, on the basis of chemical analysis by any or all of the three methods. Attention is called to the smallness of the amount of organic matter indicated as present in many of the most dangerous waters, giving important evidence against any chemical theory of the production of disease from this source (on the simple assumption that some of the chemical products of decomposition of organic matter are poisonous or noxious in their effect on the human system). Thus in the case of two waters of highly dangerous character, if the whole of the organic carbon and nitrogen present existed as strychnine, it would be necessary to drink about half a gallon of the water at once, in order to swallow an average *medicinal* dose of the alkaloid. It is not easy to believe that the ptomaines, or other chemical products of putrefactive change, can be so much more poisonous than the strongest of recognised poisons. While most of the mischief in drinking water is probably attributable to living organisms, the possibility is noted, that indirectly a large amount of organic matter in water may be more dangerous than a smaller quantity, as furnishing on a greater scale the suitable material and conditions for development of organisms. Whether variations in the mere quantity of organic matter within such limits as occur in water likely to be used for drinking are of much importance in this respect, is a question on which (in the author's opinion) depends largely the utility of all attempts to estimate the quantity of organic matter or its constituents as such.

A much more conspicuous difference between the waters of Classes I. and II. is presented by the results for nitrites and nitrates. These salts are either absent or present in but trifling

amount in the wholesome waters of Class I., but almost universally present, and often in large quantity, in the pernicious water of Class II. They are very variable as to presence and amount in the doubtful waters of Class III. This result is worthy of special attention in view of the different opinions which have been expressed (by Wanklyn, Angus Smith, Frankland, Griess, Ekin, Haines, &c.) as to the sanitary conditions of nitrites and nitrates in water.

Among the artificially polluted waters were a number of samples of such general character as to be under the gravest suspicion on sanitary grounds (suspicion corroborated in sundry cases by biological tests), in which, nevertheless, nitrites and nitrates were not found; but these waters had an extraordinarily large amount of organic matter, generally accompanied by very large amounts of ammonia.

Looking at the results for Classes I. and II., and bearing in mind the conclusions reached by Müller, Schloesing, and Muntz, Störer, Warrington, and others, as to the process of nitrification being due to presence of an organised ferment or ferments of bacterial character, "the idea suggests itself whether the noxious character of waters containing largely nitrates and nitrites—themselves presumed to be harmless—and but very little organic matter—which ought to be present, of some sort, to support the 'previous contamination view—*may not be in reality due to the presence of a special nitrifying ferment, itself to be classed among the lower organisms capable of propagating disease.*'"

Two points are noted as requiring caution in regard to the above conclusions: first, the samples may have undergone some chemical change in the interval from their collection to their reaching the analysts (but such changes could hardly have been great); secondly, it was necessary to take *exaggerated instances* of mischief; and the organic impurities present in the waters concerned may not be the same as those which would produce slighter, but, in time, serious ill effects. Slighter forms of disease, really attributable to drinking water, may perhaps be numerous, and possibly of various types, but generally the difficulty will be too great of securing, in view of the many factors concerned, any satisfactory evidence as to their cause.

In regard to determinations of chlorine, the results are in many cases of water from shallow wells, significant enough of contamination by fluid animal excreta. The amount of chlorine in the case of several wells near the sea, shows the need of thought as to the natural source of a water in drawing conclusions from the presence of chlorides. Even where chlorine has come in with organic matter, this impropriety in too hastily deciding, as is sometimes done, that a small quantity indicates vegetable, and a large quantity animal contamination is illustrated by several cases.

Prof. Martin and Dr. Hartwell were asked to independently mark waters as "dangerous" and "suspicious" on the basis of the biological observations. The results, as summarised in a table, prove that these methods will not afford the means of deciding between a wholesome and an unwholesome natural water. Several of the waters believed to be fairly wholesome, and certainly in use on a large scale, are marked "suspicious," while not one of the waters believed to have proved themselves pernicious when used by man, are set down as "dangerous." In many cases the waters which affected rabbits most, contained *very large* amounts of organic matter, so large as to probably invalidate comparison with natural waters or with the much more dilute specimens of prepared water. On the other hand, with three strengths of a solution of organic material, it was not the strongest that produced the most marked effects. The pernicious character of waters containing relatively but very little organic matter, seemed to be proved by several cases; probably supporting the idea that it is not mainly the amount of organic matter, but the presence and nature of low organisms that render drinking-water unwholesome. Much difficulty in interpretation of the biological results seems to have arisen from too great differences of absolute strength in the solutions of organic matter used.

SCIENCE AT KHARKOFF[1]

THE Society of Naturalists at the Kharkoff University is one of those which were founded a few years ago for the advancement of the natural sciences generally, and especially for the study of the natural history of Russia in the provinces that surround University towns, and which have already rendered most valuable services in both these directions. The Kharkoff Society of Naturalists, which numbers 117 members, has already published fifteen volumes of their Transactions (*Trudy*), which contain many valuable papers. Of those in the earlier volumes we will only mention, in geology: The chemical researches of rocks and coal of the Dnieper basin, by A. S. Brio; geological explorations in the government of Kharkoff and in the Coal-measures of the Don, by A. W. Guroff; and in the basins of the Dnieper and Kalmius, by M. F. Klemm; the explorations of the Delta of the Dnieper, and microscopical analyses of the Dnieper granites and of the fossil trees of Southern Russia, by M. E. Krendovsky; the very interesting researches into the formation and shapes of valleys in Southern Russia; on the crystalline rocks of the Dnieper; on black earth, on the Devonian formation of the Sosna and Tim river, and on the structure of the mountains of Taurida, by Prof. S. F. Levakovsky; and on the hydrography of the Northern Donets river, by J. T. Morozoff. The attention of the Kharkoff zoologists was especially attracted during recent years to the obnoxious insects which destroyed the crops, and we find in the *Transactions* of the Society several papers devoted to the subject, such as a complete description of the locusts and other insects inhabiting corn-fields, by P. W. Ivanoff; on the parasites of the locust and the corn-beetle, by P. T. Stepanoff; and on obnoxious insects of the province of Kharkoff, by W. A. Yaroshevsky. The same author has published also nearly complete lists of the Hemiptera, Heteroptera, Diptera, and Lepidoptera of the province of Kharkoff. Among many other contributions in zoology and physiology we notice physiological researches into the structure of the eyes of birds, on the movements of protoplasm, on the air-sacs of birds, and on the mechanism of their breathing; on the movements of *Unio*, and of *Anguis fragilis* (all with numerous plates), by R. F. Byeletzky; on water-acarides, by M. E. Krendovsky; on the *Bythotrephes* of the Sea of Azov, and on a new *Polyphemida*, by N. P. Pengo; on Infusoriæ, Turbellariæ, and Lepidoptera of the province of Kharkoff, by Madame S. M. Pereyaslavtreff; on the development of Nematodes, and on *macrobiotus macronyx*, Duj., by the late G. M. Radkevitch; and on the Araneæ fauna of the province of Kharkoff, by W. W. Reinhard. There are but few papers on botany in the *Transactions*. K. S. Gornitsky contributes a "Conspectus plantarum" of the Walki district of the province of Kharkoff; E. M. Delarme has two contributions on the anatomy of Coniferæ and on the Kirkazon plants; N. F. Kransakoff publishes a list of plants of the neighbourhood of Taganrog, and Novocherkask; and L. W. Reinhard, on the conjugation of zoospores, and on the Characeæ of Middle and Southern Russia. All these papers are profusely illustrated, and sold each separately at very low prices.

The recent (fifteenth) volume of the *Transactions* (*Trudy*), contains the work done by the Society in 1881. M. Stepanoff contributes a paper on the very unsettled question as to the metamorphosis of Bombylides. He has found larvæ of Bombylides in cocoons of *Stauronotus vastator*, Stev.; they support very well temperatures as low as −20° Cels., and can remain at life for more than one year. The opinion of M. Zetterstedt as to the larvæ of Bombylides living also freely, non-parasitically, in the soil, seemed to be confirmed by M. Stepanoff, who found them in the autumn and in the spring in the soil, but they might have already abandoned their former dwellings. M. Stepanoff gives also a complete description (with coloured drawings) of the larvæ of *Systoechus leucophagus*, Mg. —M. Kulchitzky contributes two papers; on the endings and ramifications of the motor nerves of the lower vertebrata (the author doubts that the motor nerves necessarily end in small lamellæ under the sarcolemma, as it was observed by Herr Kühne); and on the origin of the coloured globules of the blood of Mammalia; these last—the author says—arise, not out of protoplasm, but from globules of lymphoid elements which undergo a whole series of very complicated metamorphoses.—M. Yaroshevsky gives a list of Neuroptera and Hymenoptera of the province of Kharkoff. The Neuroptera of the close neighbourhoods of the Kharkoff city number no less than sixty-one species. The Hymenoptera number 400 species, of which no less than 235 are known in the neighbourhood of the Kharkoff city. The same author, in company with M. Sokoloff, contributes a paper on the state of larvæ of the corn-beetle (*Anisoplia*) during the winter. The recent ravages of the corn-beetle in Southern Russia had provoked new researches on this

[1] *Trudy Obshestva Estestvoispytatelei pri Kharkovskom Universitete* (Transactions of the Society of Naturalists at the Kharkoff University), vol. xv. 1882.

subject, and a controversy had arisen among Russian entomologists, some of them being of the opinion that the larvæ remain during the winter in the upper frozen sheet of the soil, and are in a state of sleep, while others affirmed that they go deeper into the unfrozen soil, and eat there the roots of plants, but die completely if exposed to temperatures below the freezing point. The researches of MM. Yaroshevsky and Sokoloff proved that these larvæ fell asleep when exposed to temperatures below zero, but immediately returned to life as soon as exposed to a warmer temperature. In the frozen soil, whose temperature was one degree below zero, they found plenty of larvæ of *Asilus*, *Elateridæ*, *Heliothis dipsaceus*, and whole nests of ants with their larvæ. All returned to life when warmed. M. Byeletsky contributes a paper on the respiration of the gigantic Salamander, *Cryptobranchus japonicus*, Hœv., one metre long, and weighing four kilograms (several individuals of the same species measure, as is known, four feet, and weigh nine kilograms). Siebold had already observed the very long pauses between the breathings of this Salamander, sometimes lasting for half an hour. M. Byeletsky found that at a temperature of water about 15° Celsius, his Salamander remained without breathing sometimes for an hour and a half. In the air it breathed more often. M. W. Reinhard contributes an elaborate paper on the structure and development of freshwater Bryozoa. After a sketch of our present knowledge of the subject—up to the last works of Messrs. Nitsche, Hatschek, Hyatt, and Allmann—the author describes at length the structure of *Crystatella mucedo*, giving special attention to the development of the statoblasts, and the sexual multiplication of *Alcyonella fungosa*. The paper is accompanied by seven well-engraved plates.

THE HIBERNATION OF ALETIA XYLINA, SAY., IN THE UNITED STATES, A SETTLED FACT[1]

I HAVE already shown in previous remarks before the Association that there were various theories held by competent men, both entomologists and planters, as to the hibernation of this *Aletia* (the common Cotton Worm of the South), some believing that it hibernated in the chrysalis state, some that it survived in the moth state, while still others contended that it did not hibernate at all in the United States. I have always contended that the moth survives within the limits of the United States, and in this paper the fact of its hibernation, principally under the shelter of rank wire-grass, is established from observations and experiments made during the past winter and spring. The moth has been taken at Archer, Fla., during every winter month until the early part of March, when it began to disappear, but not until eggs were found deposited. The first brood of worms was found of all sizes during the latter part of the same month on rattoon cotton, while chrysalides and fresh moths were obtained during the early part of April.

The fact thus established has this important practical bearing: "Whereas upon the theory of animal invasion from some exotic country, there was no incentive to winter or spring work looking to the destruction of the moths, there is now every incentive to such action as will destroy it either by attracting it during mild winter weather by sweets, or by burning the grasses in which it shelters. It should also be a warning to cotton-growers to abandon the slovenly method of cultivation which leaves the old cotton-stalks standing either until the next crop is planted, or long after that event; for many planters have the habit of planting the seed in a furrow between the old row of stalks. The most careful recent researches all tend to confirm the belief that Gossypium is the only plant upon which the worm can feed, so that, in the light of the facts presented, there is all the greater incentive to that mode of culture which will prevent the growth of rattoon cotton, since it is very questionable whether the moth would survive long enough to perpetuate itself upon newly-sown cotton, except for the intervention of rattoon cotton."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following Boards of Electors have been thus constituted:—

Professorship of Anatomy: Professors Flower, P.Z.S., Allen Thomson, Paget, Huxley, A. Newton, Liveing, Dr. Michael Foster, and Mr. J. W. Clark.

Downing Professorship of Medicine: Sir G. Burrows, Bart., Drs. Farre, Lauder Brunton, R. Quain, Professors Paget, Liveing, Humphry, and Mr. Main.

Professorship of Pathology: Professors Burdon Sanderson, Latham, Humphry, Paget, Sir James Paget, Drs. Michael Foster, J. F. Payne, and W. H. Gaskell.

Professorship of Political Economy: Messrs. L. H. Courtney, M.P., A. Marshall, H. S. Foxwell, R. H. Inglis Palgrave, H. Sidgwick, V. H. Stanton, H. J. Roby, and Prof. James Stuart.

Dr. Michael Foster is appointed an additional member of the Special Boards for Medicine, and for Biology and Geology.

Candidates for the Plumian Professorship must send in their names to the Vice-Chancellor on January 6; the election will take place on January 16.

A report has been issued recommending various modifications in the Previous and General Examinations; it, however, contains no indication of any approaching relief from examination in Greek, or of the introduction of French or German into the ordinary curriculum, or of any natural science subject. As the syndicate contains several names of scientific weight, this appears rather surprising.

SCIENTIFIC SERIALS

Bulletins de la Société d'Anthropologie de Paris, tome v. fasc. iii., 1882, contain the concluding part of M. G. D'Hercourt's "Ile de Sardaigne." In this paper the author considers at length the nature and presumed purpose of the massive conical structures known as *nur-aghes*, of which there are upwards of 3000 on the island of Sardinia, generally on, or near the coast. Since Diodorus of Sicily, who ascribed their origin to Dedalus, they have been a puzzle to the learned. The author's remarks on the intelligence of the modern Sardi, notwithstanding that craniometrically they rank among the lowest European races, gave occasion to various discussions at subsequent meetings.—A communication from M. Beauregard in regard to a discovery, made last January by M. Crevaux, of an ancient city of the Incas, at 10 kilom. from Salta, in the Argentine Republic, whose geographical position the latter was engaged in determining at the time.—On the various races inhabiting French Cochin China, by M. G. de Clanbry, who confirms the general view of the moral and social degradation of the Annamites. He draws attention to the slight distinctions perceptible between the men and women of these tribes in voice, length of hair, gait, features, &c., and supplies interesting details in regard to the local flora.—M. Topinard, in presenting to the Society Hölder's craniometer, based on geometric methods, described the craniometric and anthropometric instruments in use from Camper's time to our own.—On the merits of M. Beaumanoir's system of comparing the facial and cranial areas, by M. Corre.—A report by M. Deniker of the result of the official examinations for the Society, of an adult ourang-outang, and a young female chimpanzee, recently brought to Paris. The latter, as in the case observed by Darwin, showed its temper like a petulant child, by pouting, kicking, grinding its teeth, and *shedding tears*.—A paper by M. Corre, on the craniometric relations of certain anthropomorphous apes.—Report by M. de Mortillet of the labours of the Commission appointed to examine and protect the megalithic monuments of France. By the efforts of the Commissioners the remains at Carnac have been secured from further demolition, and the Locmariaquer group, including in the so-called "Roi des Menhirs" the largest known monolith, has passed by purchase under the control of the State.—On the abnormal development of the teeth in a child's jaw, belonging to the Stone Age, and found at Erlen, near Colmar, by Dr. Collignon. The general dental system shows a low racial character, while the large permanent molars had come up before the milk teeth had been shed.—A communication by M. Hovelacque, on certain ethnographic survivals in Marne and Berry. In the former province it is deemed specially unlucky to use the horses of a deceased person till after his funeral; in the latter the hives must have a black ribbon attached to them while the family wears mourning, and to avert evil fortune from the house of a departed master, one of his nearest relatives must proclaim to the bees that their former owner is dead.—M. Chervin, on the census of the French people in 1881. The author shows that the augmentation since 1876 has been only 20 per 1000 in France, while in England it was 145, and in Germany as much

[1] Abstract of a paper read at the Montreal meeting of the Am. Ass. Adv. Sc., by Dr. C. V. Riley.

as 574 per 1000! Maine and Normandy, notwithstanding their natural productiveness, are conspicuous for the regular diminution of their populations.—On a new form of sclerosis of the cerebral convolutions, by M. Pozzi, with special reference to the cerebral lesions common in insanity.—M. Duval's demand, in the name of a large number of his *confrères*, for the foundation by the Society of an annual Darwinian Conference, was opposed by M. Mortillet in as far as the term Darwinian was concerned, which he proposes to replace by that of *transformist*, arguing that the adoption of the word "Darwinism" is an act of injustice to Lamarck, whose researches entitle him to be regarded as the father of transformism. The question has been referred to the Central Committee.—M. Topinard's explanation of the funereal objects collected in the Philippines by M. Marché's mission.—A discussion on the project for a general manual of ethnographic questions, as drawn up by M. Letourneau for the Society. The plan followed, which is that adopted by the Florentine Society of Anthropology, is criticised at great length by M. Dally, who strongly objects to the phraseology and definitions employed in the questions, and in consequence of his objections M. Letourneau's proposed "Questionnaire" has been referred to a special commission for further consideration.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 14.—Note on a discovery, as yet unpublished, by the late Prof. F. M. Balfour, concerning the existence of a Blastopore, and on the origin of the Mesoblast in the embryo of *Peripatus capensis*, by Prof. Moseley, F.R.S., and Adam Sedgwick, M.A., Fellow of Trinity College, Cambridge.

The late Professor Balfour was just before his death engaged on the preparation of a monograph on the anatomy and development of the members of the genus Peripatus, together with an account of all known species. He left a series of notes, completed MSS., and drawings, which will be edited by the above authors, and issued shortly in the *Quarterly Journal of Microscopical Science*. His discoveries, however, concerning the early embryology of *P. capensis* are so remarkable that the above preliminary note has been communicated at once to the Royal Society.

The discovery is shortly as follows:—That a widely open slit-like blastopore is formed in the early oval embryo of Peripatus, which blastopore, occupying the median ventral line, becomes closed in its centre an anterior portion remaining open as the mouth, whilst a posterior portion apparently becomes the anus. The mesoblast is formed from the hypoblast at the lips of the blasotopre, and makes its appearance as a series of paired hollow outgrowths from the cavity of the archenteron. This most primitive method of the the formation of the mesoblastic somites closely similar to that occurring in Amphioxus and other ancestral forms, is of the greatest morphological significance, and it is especially interesting to find that it survives in an entirely unmodified condition in Peripatus, the adult organisation of which proves that it is a representative of an animal stock of the most remote antiquity.

Mr. Sedgwick, by examining some embryos in Prof. Balfour's collection of material as yet uninvestigated, has been able to conform his results, and also by finding earlier stages to verify certain points in the developmental history which rested at the stage at which Prof. Balfour's inquiry ceased, mainly on inference. A discussion took place, in which Prof. Huxley, Prof. Lankester, and Mr. A. Sedgwick took part. The latter pointed out the close resemblance of the early embryo Peripatus with open blastopore to an actinia, the mesoblastic pouches corresponding to intermesenterial cavities, and the blastopore to the mouth, and urged that the discovery tended to confirm Prof. Balfour's published theory as to the origin of the bilateralia from the elongation transversely of a disc-like ancestor, the ventral nerve-cords having been formed by the pulling out into long loops of a circum-oral ring.

Prof. Lankester expressed his opinion that the view that the blastopore represented astructure, which in an ancestral form acted as a mouth, must be abandoned. The blastopore is very probably merely an aperture necessarily formed in the process of production of the hypoblast by invagination, and has never had any special function. Prof. Huxley pointed out the essential difference between the peripheral nerve ring of Hydromedusæ and a true circumoral nerve ring.

Geological Society, December 6.—J. W. Hulke, F.R.S., president, in the chair.—Charles Bird, Enoch Cartwright, Henry Eunson, William Johnstone, Henry Liversidge, Henry George Lyons, Joseph Mawson, Horace W. Monckton, Henry Alexander Miers, John Postlethwaite, and Thomas Viccars, were elected Fellows of the Society.—The following communications were read:—Note on a Wealden fern, *Oleandridium* (*Tæniopteris*) *Beyrichii*, Schenk, new to Britain, by John E. H. Peyton, F.G.S.—On the mechanics of glaciers, more especially with relation to their supposed power of excavation, by the Rev. A. Irving, F.G.S. Generally, the author concluded, from mechanical and physical considerations, that far too much *erosive* power has been attributed by some writers to glaciers, and that it is doubtful if the work of actual *excavation* has been accomplished by them at all. The *differential movement* of glaciers he attributed to three causes: (1) cracking and regelation (Tyndall and Helmholtz); (2) generation of heat by friction within the glacier (Helmholtz); (3) the penetration of the glacier by *luminous solar energy*, the absorption of this by opaque bodies contained in the ice (stones, earth, organic germs, &c.), and the transformation of it in this way into *heat*. To this last he attributed the greater differential movement of the glacier (*a*) by day than by night, (*b*) in summer than in winter.

Physical Society, December 9.—Prof. Clifton, president, in the chair.—New members: Mr. H. E. Harrison, B.Sc., Mr. S. T. H. Saunders, M.A.—Prof. G. Forbes read a paper on the velocity of light of different colours. The author concluded from his experiments described to the Society a year ago, that blue rays travel quicker than red rays. M. Cornu had endeavoured to explain this result by peculiarities of the apparatus employed; but this explanation seemed doubtful. It was suggested that the experiments might be repeated with such modifications of the apparatus as would set the question at rest.—Professors Ayrton and Perry read a paper on the resistance of the voltaic arc, or the opposition electromotive forces set up. The electromotive force was measured by a voltmeter connected between the terminals of the lamp. Keeping the width of arc constant the E.M.F. was found to diminish as the current increased. Keeping the current constant, the E.M.F. increased rapidly, at first with an increasing width of arc, and afterwards more slowly. The authors gave a curve representing the change. About 80 volts are required to produce an arc of one-third of an inch. For further increase of arc E.M.F. is therefore proportional to increase of length of arc. The authors also read a paper on the relative intensities of the magnetic field produced by electromagnets when the current, iron core, and length of wire, &c., are constant, but the wire differently distributed. In *a* case the wire was wound uniformly from end to end; in *b* case it was wound from the middle to one end; in *c* case it was wound only at both ends; in *d* case it was wound only at one end. The field was measured along a line running through the axis of the poles beyond the magnet of the above plans; *a* gave the strongest field, except at short distances, when *b* was best.—Professors Ayrton and Perry also exhibited a set of three Faure accumulators in series feeding twenty Swan lamps, each lamp giving over 1 candle power. The electromotive force of each cell was about 2 volts.

Anthropological Institute, December 12.—Mr. M. J. Walhouse, F.R.A.S., in the chair.—Mr. A. L. Lewis exhibited some Neolithic flint implements and flakes found by him at Cape Blanc Nez, near Calais.—A paper by Mr. A. W. Howitt, F.G.S., on the Australian class systems, was read, in which the author discussed and explained the various rules with respect to marriage adopted by several of the native tribes.

SYDNEY

Linnean Society of New South Wales, September 27.—Dr. James C. Cox, F.L.S., &c., in the chair.—The following papers were read:—On a resinous plant from the interior, by K. H. Bennett. Specimens of the gum or resin of this plant, which Mr. Bennett described as *Myoporum platycarpum*, R. Br., were exhibited.—On three new fishes from Queensland, by Charles W. De Vis, B.A. This paper was a description of a new genus of the family Berycidæ, and a species of *Homalogrystes* and *Scolopsis*.—Contribution to a knowledge of the fishes of New Guinea, No. 2, by William Macleay, F.L.S., &c. This is a continuation of a list of the fishes found at Port Moresby by

Mr. Andrew Goldie.—Description of two fishes lately taken in or near Port Jackson, by William Macleay, F.L.S., &c.—On the physical structure and geology of Australia, by the Rev. J. E. Tenison-Woods, F.L.S., &c. This paper dealt at length with all the physical features of the Continent, viz. :—its mountain systems, its inland plains, and the portions intervening between the tableland and the sea, and its river-systems. Secondly the author enumerated the formations which had been recognised in Australia from the fundamental granite up to the recent alluvial. Showing that none of the large groups of rocks which are known in other parts of the world are absent from this continent. References were made to the character of the fossils found, and the soils resulting from the rocks.—On a large cretaceous *Mytilus*, from the Barcoo, by the Rev. J. E. Tenison-Woods, F.G.S., &c. This paper was descriptive of a very large fossil *Mytilus* (*M. ingens.* sp. nov.), which was found in some mesozoic strata in Queensland, of probably Oolitic age. The paper also contained a brief reference to the collections of Mesozoic fossils made in Australia.—Notes on the inflorescence and habits of plants indigenous in the immediate neighbourhood of Sydney, by E. Haviland The author gives an account of his observations on the mode of fertilisation of two species of rutaceous plants common in the neighbourhood of Sydney—*Philotheca australis* and *Boronia pinnata*. In the former species the arrangement of the parts of the flower is such as apparently to specially favour self-fertilisation, but a closer observation shows that this is rendered physiologically impossible by the maturing and discharge of the pollen of each flower before the stigma comes to maturity. A similar phenomenon was observed in *B. pinnata*, and the author suggests that the close opposition of the anthers to the stigma in these species until the pollen is almost ripe, may be designed in order to prevent, to some extent, the access of light and heat, and thus retard the maturing of the stigma until the pollen of its own flower has become discharged.—Note on some seaweeds from Port Jackson and adjacent coast, by E. P. Ramsay, F.L.S.—Mr. W. A. Haswell read a note on some points in the anatomy of the pigeons referred to by Dr. Hans Gadow in a recent paper on the anatomy of Pterocles.—Prof. Stephens exhibited a collection of rocks and fossils illustrating the structure of the Western coal-fields, as explained by Mr. Wilkinson in his map of Wallerawang (1877).

BERLIN

Physical Society, December 1.—Prof. Kirchhoff in the chair.—Dr. Hertz described and exhibited an apparatus he had constructed for demonstration of such weak electric currents as change their direction very often, several thousand times in a second. He called attention to the defects of the electro-dynamometers previously employed for the purpose, and showed that the electric heat-effect could most fitly be used in this case. The new dynamometer consists of an extremely thin horizontally stretched silver wire, the extension of which by heat, produced by the alternating currents, is observed. To this end the wire is, at its middle, wound round a vertical cylinder of steel capable of rotation about its axis, by turning of which the wire is stretched. Each extension of the wire through electric heating turns the cylinder the opposite way to his torsion, and its rotation is observed by means of a mirror and telescope. This dynamometer, as Herr Hertz showed, is only applicable when the currents are weak, and the current reversals are very frequent; that is, precisely in cases where other measuring instruments fail.—Prof. Helmholtz then spoke on his thermodynamic investigations of chemical processes, and their relation to the electromotive force of galvanic batteries, and fully explained his views both on the reversibility of chemical processes and the electromotive forces in batteries; also the experimental verification of these views in a "Calomel battery" composed of zinc, chloride of zinc solution, mercurous chloride, and mercury. The results hitherto obtained in these experiments and considerations, were brought by the author before the Berlin Academy of Sciences in July, and he is at present still engaged with the inquiry.

Physiological Society, December 8.—Prof. du Bois Reymond in the chair.—Prof. Munk read a paper upon two investigations which had been carried out in his laboratory. The first of these was by Mr. M. Preusse, on the Tapetum in the retina of some mammals. It appeared from this chiefly anatomical investigation that a tapetum is always present in the eyes of dogs, horses, and cats; and further that this tapetum is of an irregularly triangular shape and that the greater part of it is situated in the outer and upper quadrant of the retinal surface; so that it is specially impinged upon by the rays that enter the eye from beneath; over the median line and the equator of the retina the tapetum extends only a little, and this inwards and under. The point of entrance of the optic nerve always lies to the inside of the tapetum, which attains its greatest height above the nerve-papilla. In the case of all the animals that were examined, the situation of the tapetum corresponds with the region of most distinct vision. Hence is seen the correctness of Mr. Brücke's view that the tapetum acts as a mirror at a plane behind the cones and rods that are sensitive to light, which sends back a second time through the axes of these cones and rods the rays of light that have already passed through them. This arrangement is of particular service to animals when the illumination is feeble, and it explains how the above-mentioned animals can distinctly see objects lying on the ground even when slightly illuminated, and consequently also at night-time. The second investigation on which Prof. Munk reported was that made by Dr. Karlin on the vaso-motor nerves. It is well known that Prof. Goltz has, from experimental evidence, laid down the doctrine that the blood-vessels have ganglion-cells on or in their walls, which cause the blood-vessels to contract, and which are connected with the central organs by means of vaso-motor nerves which generally dilate but also occasionally contract the blood-vessels. The well-established fact that a section of a nerve, *e.g.* of the sciatic nerve, is followed by an expansion of the vessels in its tract was regarded by Prof. Goltz as the result of the action of the vaso-dilator nerves stimulated by the section, and the after occurring contraction of the vessels as the result of the action of the peripheral vaso-motor centres which in course of time attain the preponderance. This doctrine had received support from Prof. Bernstein's experiments, in which a great dilatation of the vessels was observed to occur, on stimulation of the divided sciatic nerve, in extremities in which contraction of the vessels had been induced by a great lowering of the temperature, and consequently a strong dilatation of the vessels was caused by direct electrical stimulation of the nerve. Dr. Karlin repeated the above experiment, and found its results confirmed only when very strong currents were employed; when, however, weak or moderate stimulation was applied, a contraction instead of a dilatation of the vessels took place. Accordingly the dilatation of the vessels on powerful stimulation is to be regarded as due to a paralysis, and the experimental evidence for the existence of vaso-dilator nerves as inconclusive.

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