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THE FUNDAMENTAL DEFINITIONS AND PROPOSITIONS OF GEOMETRY, WITH ESPECIAL REFERENCE TO THE SYLLABUS OF THE ASSOCIATION FOR THE IMPROVEMENT OF GEOMETRICAL TEACHING

I DESIRE to offer some suggestions respecting the form and arrangement of the elementary definitions and propositions of the Euclidian geometry. It has appeared to me that the recent German textbooks upon the subject have made a great improvement upon the older system, as developed in the works of Euclid and Legendre. I have but recently obtained the "Syllabus of the Association for the Improvement of Geometrical Teaching" and compared it with the corresponding parts of a summary of my own, the latter still in an inchoate state.

I now take the liberty of making some remarks on a few points on which I should be greatly pleased to know the views of those interested. In making them, however, no attempt will be made to go below the fundamental conceptions of the subject which are taken for granted in ordinary textbooks. It may be assumed that there is a general agreement that these conceptions are to be taken for granted, and that the only question is respecting their form and arrangement. One general remark may not, however, be out of place. The aim of elementary geometry is to present its definitions and propositions in a perfectly logical arrangement, so that each definition shall be a complete description, and nothing more, and each proposition be founded strictly on definitions and axioms. It may be doubted whether this perfect ideal is attainable. It might be claimed that our elementary conceptions of relations in space have been derived from experience by processes of abstraction and generalisation, in which no logical order was followed, and that it is impossible to arrange them with that perfect unity which logical method aims at. However this may be, it will, I think, be conceded on all sides that all our systems have hitherto been mere approximations to an ideal which no one has actually reached.

In framing a geometrical definition three different objects may be aimed at.

1. To express our fundamental conceptions of the thing defined in the most accurate form possible.

2. To specify those qualities which most completely differentiate the thing defined from all other things.

3. To describe its axiomatic properties, or those which are subsequently used in demonstrating propositions relating to it.

We thus have three tests which we may apply to a definition and which may lead to different judgments of it. In most cases the same definition will be reached which ever object we have in view. The only concept the definitions of which can be separately classed under all three heads is, so far as I have noticed, that of a straight line. The fundamental quality of a straight line as we conceive of it is, I think, that of symmetry, or similarity of properties with respect to space on all sides of it. A line which is throughout its whole length perfectly symmetrical, having no properties on one side which it does not equally possess on all other sides, is a straight line. A curve is concave on one side and convex on another. The definition of Simpson's Euclid that a straight line lies evenly between its extreme points, may be considered as an attempt to formulate this conception of symmetry.

The definition which most completely differentiates a straight line from all others is that of some editions of Euclid and Legendre as the shortest distance between two points. It is to be remarked, however, that neither of these properties is directly made use of in demonstrating the subsequent theorems of geometry. The axiomatic definition of a straight line, if I may be allowed to use the expression, is that of Playfair's Euclid, as being lines which must coincide throughout if they coincide in two points.

Quite similar to that is Definition V. of the Syllabus. This class of definitions, or the axioms in which they are embodied, include the only ones which serve as a basis for the subsequent theorems of geometry.

It is to the definition of plane figures given in the Syllabus that the attention of those interested in this subject is especially asked.

The following are extracts from the Syllabus:—

"Def. VII.—A plane *figure* is a portion of a plane surface inclosed by a line or lines.

"Def. VIII.—A *circle* is a plane figure contained by one line, which is called the *circumference*, &c.

"Def. XXII.—*A plane rectilineal figure* is a portion of a plane surface inclosed by straight lines.

"Def. XXVIII.—*A triangle* is a figure contained by three straight lines."

These definitions agree with those of the old geometry in defining plane figures as inclosed portions of a plane surface. It seems to me that in no part of geometry is greater reform needed than in this.

Figures on a plane surface should, it seems to me, be defined as lines simply, and not as portions of the surface. The following are some of the objections against the old and in favour of the new system of definition:—

1. By Definition VII., as quoted above, an ellipse is a plane figure because it incloses a portion of a plane surface, but a parabola or hyperbola is not. Three straight lines may form a figure, but two cannot. But if we form a figure of three straight lines we must cut off all those portions of each line which lie outside of its intersection with the other two as forming no part of the figure.

2. In the modern synthetic geometry figures are considered in a more general way as formed of lines. A triangle, for instance, is a combination of three indefinite straight lines. To this we may, if we please, add the restriction that no two shall be parallel, and that all three shall not pass through a point. The quadrilateral is a combination of four such indefinite lines, to which again, if necessary, may be applied the restriction that no three shall be parallel or pass through a point; the circle also becomes the line, not the inclosed space. Therefore when the student, whose ideas of such figures are only those of the elementary geometry, passes to the study of the higher geometry, he is obliged to form a new set of conceptions for the same terms; so great a change, for instance, as substituting the conception of three indefinite straight lines for that of a triangular piece of paper. He reads of

the intersection of circles, and must understand that it is something radically different from any intersection of the two round planes which he has been taught to consider as circles.

The same change must be carried into space of three dimensions. Studies of what in the elementary geometry have been termed solids, when made by modern mathematicians, are not studies of solids but surfaces. An ellipsoid in modern mathematics is not a solid but a surface. Of course we cannot reject the conception of an inclosed area, but this area must be regarded as something distinct from the figure itself, just as we regard the perimeter as something different. I do not see that anything but good will result from the change here proposed.

In Definition XI. the idea of a "straight" angle is introduced to express the angle of 180° between two lines emanating from a point in opposite directions. I should like to submit the question whether the term *flat angle* is any better. The converse of straight is bent or crooked, terms which can hardly be applied to an angle. But the converse of flat is sharp or obtuse, terms which can be so applied. Thus, before seeing the syllabus, the term "flat" appeared to me better than "straight." The introduction of this angle must be regarded as one of the greatest improvements in elementary geometry, but it does not seem to have been introduced into the subsequent theorems of the syllabus in which the old designation of two right angles has been retained without essential alteration. Intimately associated with the fundamental definition of angular measure are the theorems relating to right angles and to the impossibility of straight lines having a common segment; the following three propositions are in fact closely connected.

Two straight lines cannot have a common segment.

All right angles are equal to one another.

If a straight line stands upon another straight line it makes the adjacent angles together equal to two right angles.

The treatment of these propositions by Euclid seems extremely unsatisfactory, and the order in which they are given in the syllabus a great improvement.

Euclid takes the equality of all right angles as an axiom and afterwards proves from it that two straight lines cannot have a common segment. But it seems evident that the equality of right angles depends upon and presupposes the impossibility of a common segment. It must first be self-evident that two straight lines cannot have a common segment before it can be evident that all right angles are equal.

The third of the propositions just quoted, as considered both by Euclid and Legendre, seem to me unnecessary and circuitous courses of reasoning carried through solely to avoid the conception of the sum of two right angles being *itself an angle*. This circuit is all the more readily taken from the fact that neither of them has considered it necessary to give a general definition of what shall be meant by the sum of two angles. The syllabus gives this definition and from it alone, without any reasoning whatever, it follows that the sum of the two angles referred to is a flat angle.

As an additional illustration of the simplicity introduced by the consideration of the flat angle we may take Theorem XXVI. of the syllabus, that the interior angles of any polygon, together with four right angles, are equal to twice as many right angles as the figure has sides. In the new notation we would say that the sum of the interior angles of the polygon is equal to a number of flat angles two less than the polygon has sides, an obvious simplification.

With reference to Definition XII. I would suggest the question whether it would not be better to reserve the term "adjacent angles" for the pair of angles which a straight line makes with another at the point of meeting. We might call these supplementary angles, but the term is suggestive not simply of an arrangement of the two angles but of any pair of angles, wherever or however situated, which together make a flat angle. We certainly need some term to correspond with the *Nebenwinkel* of the Germans, and I know of none in our geometry.

In Theorem VI. of the syllabus, which is the same as as Proposition V. of Euclid, namely, "The angles at the base of an isosceles triangle are equal to one another," the syllabus suggests a different demonstration from that of Euclid. The extreme complication of the demonstration given by Euclid is very striking, and it will be interesting to see how it arose. Apparently Euclid wished to avoid the conception of turning a figure over and applying it to itself. But the validity of this turning over is presupposed in the demonstration of the theorem, for without it the equality of two triangles having two sides and the included angle equal would be true only for triangles in which the two sides are similarly situated. This question is of especial interest when we apply it to the corresponding case of two equal solid bodies which are mutually obverted or in other words each of which is represented by the image of the other seen in a looking-glass. Are we entitled to assume that two such bodies are identically equal when it is impossible to bring them into coincidence? The only reason why we cannot bring them into coincidence is that our space is confined to three dimensions. Could we open out a fourth dimension in space the one body could, by simple rotation through 180°, be brought into the form of the other and thus made identically equal to it. A man by turning a properly directed somersault in such space would come back into our natural Euclidian space, turning right side left without the mutual arrangements of the parts of his body, even to the minutest atoms, undergoing any change whatever in their relative positions; and therefore without any change, so far as we could see, in the performance of the vital functions. But as a fourth dimension is necessary to the actual performance of such an obversion, so in plane geometry, the third dimension is necessary to the obversion of a plane figure. The syllabus, and so far as I know all the elementary geometries in English are silent on the validity of this process.

The question whether Theorems X. and XI. that the greater side of every triangle is opposite the greater angle, and the greater angle opposite the greater side, should be regarded as independent and demonstrating in entirely different ways is interesting. Since only one side and one angle can be in the relation of opposition how is it possible that the one theorem should be true without the other? Does not one theorem follow from the other by the rule of identity, and] can they not be

combined into the single theorem that the greater side and the greater angle are opposite each other?

SIMON NEWCOMB

THE SCIENCE OF STATESMANSHIP

POLITICAL science and politics are two very different things; some progress has been made in methodising the facts and inductions of political economy, but politics is still little more than a chaos of party prejudices and personal invective. Yet there is surely no reason why political action, the conduct of the State, should not be guided by scientific method quite as much as the conduct of a scientific exploring expedition such as that which has so recently sailed over the North-East Passage. Prof. Nordenskjöld's feat is one of the finest instances of scientific prediction based on ascertained data that we know of, and we would recommend it to Sir William Harcourt's consideration when he contemplates taking part in another political "agitation." Sir William has succeeded in getting such a firm grasp of the real nature of scientific method, and he applied it so wittily and so well in his recent Birmingham address that we would advise him to follow out this line in real earnest. So thoroughly does he seem to understand the method of scientific research and scientific prediction, and so ably, although only in sport and to banter his opponents, did he expound it, that we think science has lost in him a successful worker. To this loss we could resign ourselves if Sir William would set himself to rescue politics from its present degraded position as a mere theatre for party strife, and to elevate it into something like a science of national life and progress. He must have taken considerable pains to obtain his knowledge of the method and uses of the *Nautical Almanac;* his natural mistake as to its editorship we can overlook. As to the truth of his application of the method of the almanac to the construction of a Conservative Almanac, "after a careful induction from the conduct of Tory government," we have nothing to do here; its ingenuity is amusing. With the following remarks, however, men of all parties cannot but agree:—

"Prediction in politics is not a matter of choice, but of necessity. If public men are not fatalists like the statesmen of our darling Islam, they are bound to foresee and foretell the consequences of their action by which the fortunes of the country are determined. As the predictions prove true or false so will they be judged, for political prophecy, founded upon correct observation and just inference, is nothing else but the science of statesmanship itself."

Here Sir William has struck a vein which might be worked out to the elevation of politics, and with real good to the country. It is, we believe, regarded as an incontrovertible axiom in British politics, that government by party is the surest method of securing the most efficient conduct of public affairs. This point we shall not discuss; but we venture to think that if our political leaders were to give their serious attention to the method indicated above, party differences would be fewer than they are, and party strife less bitter, while the objects supposed to be aimed at by all constitutional governments would be much more effectually and rapidly accomplished.

At present, to judge from the public utterances of our members of parliament and by the results achieved by which ever party may be in power, party government consists mainly in strenuous efforts made by each party either to keep or to obtain place and power; this is accomplished by means of what are called "agitations," the great object of which seems to be to agitate the people into the belief that the agitators are angels from heaven who have the good of the nation disinterestedly at heart, while their opponents are quite the reverse, the only object of the latter being, it is declared, to send the nation to the custody of the person whose emissaries they are.

There are one or two eminent men of science in parliament, but no one of either party ever seems to think of looking at any measure or any line of conduct apart from party bias, and solely as a matter for scientific consideration. It seems enough to damn a measure at once in the eyes of one party, that it originates with their opponents. This is both unscientific and irrational, and can never lead to the best results. The same laws that influence the development of the individual influence the real progress of the nation, and it is only by honest investigation on strictly scientific principles that these laws can be discovered. It is thus that they have been discovered and expounded by Mr. Darwin and his followers in the case of individual organisms, and we would commend to Sir William Harcourt the study of Mr. Darwin's works, if he really desires to arrive at the true principles of scientific statesmanship. One of the great charms of Mr. Darwin's works to the man of science is their perfect candour and fairness. Not only does he adduce all the arguments he can muster in favour of any position or hypotheses he may be considering, but with equal fulness and candour does he treat all, according to his lights, that might be adduced against it, balancing the one series of arguments against another, not in the style of a special pleader, but after the manner of a judge whose sole aim is to discover the truth. Here is a specimen of the method followed by Mr. Darwin, showing his ingenuity in imagining objections to his own theories and thus putting arguments into the mouths of his opponents. We quote from the "Origin of Species" (1860, p. 462):—

"As on the theory of natural selection an interminable number of intermediate forms must have existed, linking together all the species in each group by gradations as fine as our present varieties, it may be asked, Why do we not see these linking forms all around us? Why are not all organic beings blended together in an inextricable chaos? With respect to existing forms, we should remember that we have no right to expect (excepting in rare cases) to discover *directly* connecting links between them, but only between each and some extinct and supplanted form. Even on a wide area, which has during a long period remained continuous, and of which the climate and other conditions of life change insensibly in going from a district occupied by one species into another district occupied by a closely allied species, we have no just right to expect often to find intermediate varieties in the intermediate zone. For we have reason to believe that only a few species are undergoing change at any one period; and all changes are slowly effected. I have also shown that the intermediate varieties which will at first probably exist in the intermediate zones will be liable to be supplanted by the allied forms on either hand; and the latter, from existing in greater numbers, will generally be modified and improved at a quicker rate than the intermediate varieties, which exist in lesser numbers;

so that the intermediate varieties will, in the long run, be supplanted and exterminated.

"On this doctrine of the extermination of an infinitude of connecting links, between the living and extinct inhabitants of the world, and at each successive period between the extinct and still older species, why is not every geological formation charged with such links? Why does not every collection of fossil remains afford plain evidence of the gradation and mutation of the forms of life? We meet with no such evidence, and this is the most obvious and forcible of the many objections which may be urged against my theory. Why, again, do whole groups of allied species appear, though certainly they often falsely appear, to have come in suddenly on the several geological stages? Why do we not find great piles of strata beneath the Silurian system, stored with the remains of the progenitors of the Silurian groups of fossils? For certainly on my theory such strata must somewhere have been deposited at these ancient and utterly unknown epoch in the world's history."

Did we urge Mr. Darwin's method upon the members of our two great political parties, we fear we should only be laughed to scorn. And yet is not such an attitude in any body of men, most of all in those men whose duty it is to discover what is best for the welfare of the State, well calculated to inspire honest and thoughtful men with melancholy? Fancy Mr. Gladstone bringing before an audience during one of his great "agitation" tours, not only all that can be said against any of Lord Beaconsfield's foreign *coups*, but, on the other side, all that could be said in favour of them, and then striking a judicial balance. And would not Lord Beaconsfield be considered as indulging in a huge joke, if, after a Mansion House dinner, he should proceed to treat the conduct of his great opponent after a similar fair and judicial fashion. And yet this would be the true scientific method of arriving at the truth in public affairs, just as it is in the investigations with which physical and natural science deals. And it is really because our parliamentary agitators despise their audiences that they treat them to only one side of a question; and if these audiences were as intelligent as they ought to be, they would not listen to any public agitator who treated them so one-sidedly. By and by let us hope that the nation will be so far advanced that politicians will give and the public will insist on being told all that can be said both for and against any measure. "Agitation," however, is not the best atmosphere in which to carry on scientific work; quite the opposite. And we should advise those of our public men who are really desirous to discover the science of statesmanship, and to guide their public conduct by its principles, to leave the method of agitation alone for a period, and take to calm but rigid scientific research in their own department, and we are sure the results will surprise even themselves. Scientific method is peculiar to no section of phenomena; it is rapidly embracing many departments of research that at one time were thought to be beyond the pale of science; and we venture to think that in no department could it be applied with greater success than in that department which hitherto has been almost entirely under the sway of prejudice and blind party spirit. Sir William Harcourt has clearly shown what can be done in sport; let him and others now try as earnestly whether even greater success would not attend scientific political prediction in earnest.

In the case of individuals, if we know their constitutions and their circumstances, we can to some extent guide their development and influence its direction; we can to some extent help them in the struggle for existence, and enable them to comply with the law of the survival of the fittest. Whether or not these two laws would justify the recent conduct of foreign affairs by the present Government, it is not for us to say. That conduct we know is justified by many on these grounds; at all events, we believe that if scientific statesmanship, and not mere party prejudice, were the guiding principle in the conduct of public affairs, this nation would be more fitted than ever to survive and play the leading part in the affairs of the world.

Scientific retrospection is quite as important as scientific prediction; we must recognise all the causes and their interactions or we may go wrong; but Mr. Bright in his recent sketch of the progress of the country during the past fifty years, altogether ignored what we believe the most important factor—the results of scientific research. Even granting the value of all the political measures to which he referred, where would the country have been at the present day had it not been for the results obtained by the quiet workers in science? Some time ago he gave a great Free Trade speech, in which he dwelt upon the immense benefits which have accrued to the country from the line of policy indicated by that expression. He went on sketching the progress of free trade, and the concomitant progress of the country, as if no other cause could possibly have been at work, and as if such powers as science, railroads, penny posts, improved machinery, increased population, and the like—gave no greater impulse to the development of the nation than an annotated edition of an obscure classic by a still more obscure Oxford don. It is not for us to pronounce on the merits or demerits of free trade or protection, but we venture to think that all that can be said in favour of either the one or the other is small when compared with the services rendered to the country by science during the past fifty years. What about railways, and telegraphs, and the great results of engineering skill, and the application of science to manufactures and agriculture, improvements in navigation, the invaluable practical discoveries of chemistry, and a thousand and one other fruits of scientific research?

Of these the political partisan takes no account; his function, as compared with that of the true worker in science, seems to us pretty much like that of the organ-blower as compared with the organist. We have said that there are one or two really able men of science in parliament; but they are only one or two. Probably in no parliament in Europe is science so sparsely represented, and yet we do not advise our real scientific workers to seek admission into an arena that we fear would be little congenial to them. But is it not high time that all our members of parliament should be really well-educated men, know something about the principles and results of a department which has done so much for the nation and on which its real welfare and progress so largely depends? Sir William Harcourt has shown that there is no reason why this should not be done, and we trust that not only will he follow out the course he has so well begun, and do this not merely for a gibe, but that his example will

stimulate other well-meaning members of parliament to do what they can to qualify themselves to conduct the legislation of the country on broader, more enlightened, and more scientific principles than have ever hitherto been brought into play. Meantime those who have the true welfare of our country at heart will use every means to get education in science introduced into all our schools and colleges without distinction, so that in future years rulers and people will be guided in their public conduct not by party prejudices but by the principles of scientific statesmanship.

NICHOLSON'S PALÆONTOLOGY

A Manual of Palæontology, for the Use of Students. With a General Introduction on the Principles of Palæontology. By H. Alleyne Nicholson, M.D., &c., Professor of Natural History in the University of St. Andrew's. Second Edition. (W. Blackwood and Sons, 1879.)

IT is a great pity that there should be any demand for a Student's Manual of Palæontology. The separation of the study of extinct forms of life from that of recent animals, which is implied in the term Palæontology, and which is unfortunately largely maintained in practical science, is much to be deplored. In nearly all great museums, as in the British Museum, the fossil series of animal remains are preserved and displayed in different parts of the museum from that in which the recent ones repose and are studied and taken care of by a separate staff of officials. The extinct corals, for example, are in the hands of one set of naturalists and the recent corals in the hands of another, the most closely allied or even identical species are widely separated from one another, and considerable labour and trouble are caused to any observer who wishes to bring them together for comparison. There are necessary gaps enough in the various zoological series from the imperfection of the geological record; in museum collections they should be rendered as small as possible.

Prof. Nicholson's book cannot take the place of such a work as Quenstedt's "Petrefactenkunde," which has a proper standpoint as being required by the geologist as a means of identifying fossils. The present work may be described as an attempt to teach students as much as possible about those forms of life which happen to be extinct, by means of the aid of as little knowledge of living forms as possible. The author writes in the introduction: "Palæontology may be considered as the zoology and botany of the past. Regarding it from this, the only true point of view, some knowledge of zoology and botany is essential to the prosecution of the study of palæontology, and such details of these sciences as may be deemed requisite will be introduced in the proper place." *Some* knowledge of zoology and botany is indeed required to make a man a successful palæontologist; the real fact is, that it is only the most skilful and deeply-versed zoologists and botanists who are capable of dealing with the problems of palæontology with any valuable result. Only those most intimately acquainted with living forms are qualified to deal with the fragmentary remains of extinct animals and plants. It would be well, indeed, if the term palæontology were abolished, and with it any pretensions of investigators to treat fossils from a separate standpoint. Botanists are full of complaints of the confusions introduced into their science by the operations of certain palæo-botanists, to use the present author's term, who manufacture genera and species wholesale from impressions of single leaves or even fragments of leaves, and there are plenty of confusions equally detrimental in the nomenclature of extinct animals. It is most illogical to separate the members of the animal and plant series for purposes of study into two groups: that containing those forms which exist at the present epoch, and that embracing those which have lived and mostly become extinct during the vast antecedent period of which record remains. The separation is a purely artificial one, productive of no good, illustrating no general scientific law, coinciding with no natural division of the biological series: and is, further, one especially likely to produce misleading impressions in the minds of students.

Throughout the book the author recurs again and again to the distinction of palæontology as a science from zoology and botany. He writes of palæontology as based on *the kindred sciences* of zoology and botany. "No satisfactory acquaintance with the former can be arrived at without the previous acquisition of some knowledge of the latter." "A few points of these sciences may be noticed as having special bearing on the study of palæontology." Further on, in an account of Prof. Huxley's now abandoned group, the Annuloida, which is retained in the present work, it is mentioned that "The sub-kingdom was proposed by Huxley as a provisional arrangement to include the two groups of the Echinodermata and Scolecida, and the following extraordinary statement follows: *Whether this arrangement be ultimately retained or not matters not at all to the palæontologist, as no member of the Scolecida is known in the fossil condition.* Could any teaching be more pernicious to a student?

After several very good introductory chapters on general geological subjects, Chapter VI. treats of the divisions of the animal kingdom and succession of organic types. The author, after treating of the development theory, concludes by patting the Darwinian theory complacently on the back "as an invaluable, indeed an indispensable, *working hypothesis*," but most unfortunately for the value of his book, he does not make use of the theory as a *working one*, but considers it "preferable to enter upon the study of the actual facts unfettered by præconceptions and unpledged to theories." He accordingly treats of the classification of the animal kingdom in most antiquated style. All animals may be classed under five or six "morphological types," and "no comparison is possible between an animal belonging to one sub-kingdom and one belonging to another, since their distinguishing characters are the results of the modification of two essentially different ground plans."

"We must abandon the idea that it is possible to establish a linear classification of the animal kingdom." But why suggest any such erroneous idea as this latter to the student at all? If only the *working hypothesis* had been adopted, the real meaning of modern scientific

classification, as representing pedigree and being arborescent in structure might have been pointed out. As it is the impression to be gathered by the student must be that the whole classification is disjointed and artificial. The classification given is obsolete and imperfect in many respects. The Sponges are placed amongst the Protozoa notwithstanding all that is now known of their embryonic development. The Tunicata are placed with the Brachiopods and Polyzoa as composing the Molluscoida. The Sirenia are put next to the Cetacea between the Edentata and Ungulata, and the two are treated of in one chapter as if allied to one another. Finally, man, with his venerable but flattering specific title "sapiens," is placed in the old separate order Bimana, apart from the orang and the gorilla, whilst animals so widely different as the rhinoceros and sheep, nevertheless occupy the same order Ungulata. How perverted must be the conception formed by a student of the value of morphological facts, when the results of their study are presented to him in tabular form on so utterly unequal a scale as this.

A book so large as the present necessarily takes a long time in preparation, and consequently, as the author explains in the preface, many recent publications of importance were not available to him for use in the earlier part of it. Hence the "Tabulata" are still retained as a group amongst the corals, although they have been given up as such by the author in his recent monograph on the Palæozoic tabulate corals.

By far the greater part of the book is taken up by the description of invertebrata, and the vertebrata receive comparatively less attention. Prof. Nicholson gives his reasons for not treating the vertebrata with the same fulness as the invertebrata. "The fossil remains of vertebrates are, in many cases, of the highest interest, but they come much less frequently under the notice of the ordinary student than do the remains of the invertebrates." We should have thought that these would be precisely the reasons why these rarities should be described at length, but "no practical study of the fossil vertebrates can be carried on without a *considerable acquaintance* with comparative osteology." Who, then, is the "working palæontologist" for whose benefit, as we are told in the same paragraph, the present treatise is intended? We can understand the value to a *working geologist* of a book which shall enable him to determine with ease the names of fossils, that he may use them in the prosecution of his researches as so many counters; but the present book does not, like Quenstedt's, meet this requirement in any way.

Granting, however, that there are students who require a work of instruction such as the present, the book is not without many merits, and care has been taken to introduce some account of all recent discoveries of importance. The account of the vertebrata contains a great deal of interest, including an account of some of the most interesting of the discoveries of fossil vertebrata in the United States. A good woodcut of the large tooth-bearing diving-bird, *Hesperornis regalis*, is given, taken from Prof. Marsh's restoration of it. It was between five and six feet in height. Figures are also given of the skull of Prof. Marsh's *Dinoceras mirabile*, with huge canines and three pairs of horn-cores, and also of the feet of the same curious form, which is considered by Prof. Marsh as intermediate between the Perissodactyle Ungulates and the Proboscidea.

The elevation of the Platysomid fishes to the rank of a distinct division of Ganoids is adopted by the author, owing to a misunderstanding of certain manuscripts placed at his disposal by Dr. Traquair, who has disclaimed his concurrence in the matter in the *Annals and Magazine of Natural History*. In consequence of Dr. Traquair's letter a slip has been inserted in all but the earliest copies of the book correcting the error.

The book concludes with chapters on palæobotany, which term hardly describes the contents since they are geologically and not botanically arranged. A slight sketch is given of the floras of the successive geological epochs, the characteristic fossil plants being named and figured but without much account of the details of their structure. An antiquated classification of plants is adopted, the Conifers and Cycads being grouped with the dicotyledonous Angiosperms as Exogens or Dicotyledons, whilst the monocotyledonous Angiosperms are separated from the remainder as Exogens.

There is a glossary of terms at the end of the book in which the Greek words look curious as printed in Roman characters, especially as the long vowels are not marked as such in any way. The first word in the list is Abdomen, which is for some unexplained reason derived from the Latin *abdo*, I conceal, instead of given as itself a Latin word of the same sense as that in which it is used in science. It is surely also doubtful whether the word abdomen has anything to do with *abdo*. It is suggested in some dictionaries that it is a corruption of *adipomen*.

The book is sumptuously got up and contains over 700 woodcuts, most of which are very good, many being familiar as taken from D'Orbigny and elsewhere, but many also being new. Good lists of references to monographs are given at the ends of the chapters, and form a very valuable and important feature in the work.

SIZING AND MILDEW IN COTTON GOODS

Sizing and Mildew in Cotton Goods. By G. E. Davis, C. Dreyfus, and P. Holland. (Manchester: Palmer and Howe.)

THE application of a certain kind of science to a certain kind of commerce is rapidly producing a literature of its own. It is not long since that we had occasion to notice a work which treated of the manner in which silks could be "weighted" by chemical means, and the volume now before us is the second of its kind which is concerned with the relations of chemistry and mycology to the manufacture of cotton goods.

In order to explain the *raison d'être* of this book, it may be desirable to premise that in making cotton cloth it is necessary to "size" the longitudinal threads or warp in order that they may be able to withstand the strain in the loom. The size binds the individual fibres together in the thread, and by giving it an even surface, diminishes the fraying action of the reed in its motion to and fro after the passage of the shuttle. "Pure" size consists of a mixture of fermented flour, soft or curd soap, and tallow; or of sago and cocoa-nut oil in water. The yarn is occasionally sized in the hank by hand, but this method is rapidly giving way to the use of machinery, by means of

which the warp is pulled in single threads through the sow-box, or vessel in which the sizing liquor is contained, and is afterwards dried by heated air or by passing round cylinders filled with steam. The amount of size in the so-called "pure" cloths varies from 5 to 7 per cent. In such cloths the quantity of fibre is from 92 to 94 per cent., the remainder being made up of mineral matter derived from the raw cotton. Now as one element in determining the value of cloth is its weight, it happened that at about the time of the "cotton famine" which followed the civil war in America, that certain unscrupulous manufacturers introduced the practice of "heavy-sizing"—that is, in plain terms, of substituting cheap mineral substances for cotton.

Some idea of the extent to which this adulteration is practised may be seen from the following analysis of a heavily-sized warp, published by the authors. It will be noticed that only about one-third of the substance is cotton fibre, the remaining two-thirds being made up of clay, flour, and fats, with certain mineral chlorides.

Cotton Fibre	Fibre	33·18	
	Natural moisture	2·65	35·83
Size	Moisture with size ...	7·81	
	Fats	3·04	
	Starchy matters	16·16	27·01
Mineral ...	Natural ash	1·00	
	China clay	32·07	
	Chloride of magnesium..	3·25	
	,, zinc	0·84	37·16
			100 00

Very large quantities of a variety of cloth known in the Manchester trade as an "eight-and-a-quarter-pound shirting" find their way to India and China. The general character of a very considerable proportion of this substance may be determined from the following numbers:—

	lbs.	oz.	
Warp	2	14	
Weft	1	12	
	4	10	Pure cloth.
	3	9	Size, &c.
Total	8	3	

To the general reader a word or two of explanation concerning the extraordinary complexity of the composition of a piece of modern grey cloth, as revealed by the foregoing analysis, may be desirable. It will be seen that the main weight-giving substance is China clay, which has to be suspended in a sizing liquor of pretty stiff consistency. In order to preserve the clay upon the fibre it is necessary to keep the fabric slightly damp; this is effected by the addition of some highly hygroscopic material to the size, such as the magnesium chloride, which is one of the most deliquescent substances known to the chemist. The constant presence of moisture, however, renders the fabric very liable to mildew, especially if the flour has not been properly fermented before it is incorporated into the sizing liquid; and it is in order to prevent this that some antiseptic is added, usually chloride of zinc.

There is no doubt that in the outset the manufacturers, as a body, set their face against the production of such stuff. Twenty years ago these fabrics had an evil reputation: they were made by tenth-rate manufacturers and sold by tenth-rate agents. But the heat of competition has changed all this. The immense quantities of these goods which found a market in India and China—indeed, they were mainly made for exportation—compelled the great majority of Lancashire manufacturers to respond to the demand for these combinations of China-clay and starch with a modicum of cotton, a demand which is very largely fostered by the numberless middle men who come between the manufacturer and the consumer. The usual result has followed: the very fact that numbers are engaged in it has given the trade an air of respectability. *Quæ fuerunt vitia, mores sunt.* The other day Mr. Consul Gardner reported from Cheefoo that a bad name attaches to Manchester goods among the Chinese, consequent on attempts "to sell glue as cloth," and it is highly amusing to read how the Manchester Chamber of Commerce waxed indignant, and how they requested Lord Salisbury "to prevent the publication of similar statements in the future"! It is rather significant, too, that whenever a book on the subject of cotton-sizing is put forth, it should be thought necessary by the authors to dwell upon the "moral aspects" of the question in entire obliviousness of the salutary caution that to excuse is too frequently to accuse. Some of the arguments in extenuation would be amusing if they were not grotesque, as in the book before us, where we read, on p. 99, that "no one, we suppose, will deny that for coffin linings, &c., a heavily-sized but cheaper cloth is not just as good as a purer but more expensive article. If this be granted, the existence of such a material is certainly a boon." How very grateful the undertakers ought to feel for such a boon!

It is hardly worth while to take up valuable space by noticing the merits or demerits of a book such as this, the object, or at least the tendency, of which is to show the manufacturer how, by the application of certain scientific facts and principles, he may seek to perpetuate a system which, we honestly think, is simply a gigantic fraud. Our authors comment adversely on the assertion of a certain county court judge, in a case which came before him, that the "warp-sizer and manufacturer, in receiving and giving the order for sizing some warp, had entered into a conspiracy to defraud the public," but it seems to us not improbable that the judge might be perfectly right. It is almost certain that such a system will not be perpetuated: people will not sheathe themselves with shirts of China-clay. The time was when Manchester made cottons for the world, but her supremacy is being rapidly undermined; and who shall say that her sins have not contributed to her downfall?

LETTERS TO THE EDITOR

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

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

The Intra-Mercurial Planet Question

I HAVE read, in NATURE, vol. xx. p. 597, your editorial on the above subject. To the language of that portion of it relating to my observations I take most decided exception. You have,

unintentionally of course, done me not a little injustice, owing to a misconception of what I have written, and, strangely enough, you have changed my language, giving it not merely a different, but an opposite, meaning.

I regret that I cannot look at all charitably on your baseless charges that I have "made different statements, and exhibited a degree of hesitancy about it." I had thought that my meaning regarding this question could not possibly be misconstrued, but, perhaps, in going over so much ground in so short a letter, I may not have been so clear on every point as I supposed. My desire to divest the subject of all ambiguity, and to defend my observations, if not my character, from the grave charges you have made, is my only excuse for again appearing before the world. Now, if you will give me a little space in your widely-read journal, I will, as briefly as I can, endeavour to make the subject as plain as written language will allow. If in any person's mind there yet lingers the idea that I have made different and contrary statements, my first effort shall be to set him right. Surmising that in one of your charges (different statements) you refer to the estimated distance of 12′ between the two objects seen by me during the total phase of the eclipse, I answer by emphatically saying that I have never published such a statement. A little explanation, however, is here necessary for clearness of conception. As soon as I saw the two stars I was confronted with half a dozen questions which required immediate answers, for time was precious, viz.: 1. What stars are they? 2. How far and in what direction from the sun? 3. How far apart? 4. Of what magnitudes? 5. In what direction do they point? What star, in the clearest, darkest night, appears to the naked eye as bright as do these? In response to 3, my *instantaneous* impression was about 12′, but, as quickly thinking how wide of the mark I might be in the estimation of so large a distance, I chose to impress it on my mind, knowing that, after arriving at home, I could soon find two stars whose apparent distance would be sensibly the same. This I did, and have several times published to the world the result, viz., that they were a little over half that between Mizar and Alcor, or about 7′ apart. What I wrote in my note-book of the 12′ I discarded immediately, and all the time have said, in language too plain to be misunderstood, that it was of no value at all. Every published statement has been a reiteration of this, and where, I ask, is the excuse for any who have read my letters and reports to misunderstand this? The distance recorded in my note-book was merely for reference, to see how near the truth the guess would come out. I repeat that I have never published that they were 12′ apart, and your charge that I have made "different statements" falls harmlessly to the ground. Have I not adhered with unyielding pertinacity to the facts first published, that they were about 3° south-west of the sun? That they were exactly equal in brightness, and of the fifth magnitude? That the disks were large and red? That they were about 7′ apart? And that they pointed towards the sun's centre? In all I have written I have been as guarded as possible, knowing that the time might come when every word would possess a significance not now anticipated. How, then, with any kind of justice, can I be accused and published to the world as having made different and contradictory statements?

Perhaps you base your charge on the mathematical error made in reducing the estimated distance in arc to that of time, in order to show the near agreement in R.A. between Prof. Watson's star and mine, but does that come under the head of "different statements?" If all numerical errors are to be thus classed, who, without sin, can be found to cast the first stone?

I wish it to be distinctly understood that up to this time I supposed (and the fact was disputed by none) that one of my objects was θ Cancri, and the other Watson's planet (*a*), and I was extremely desirous, while it all was fresh in our minds, to settle the matter, so I wrote to him that I could not harmonise his observations—as published—with my own, though I did not tell him what changes were necessary to attain this result. He replied that after making the necessary corrections, the Dec. comes out + 18° 16′, while his previous statement, made before the corrections were applied, declared it to be but 18°. That 16′ helped matters very much, but still was only half enough, as the following facts will show. The Dec. of θ is 18° 30′ 20″, that of the sun at the time of the eclipse was about the same, and, as my two objects ranged with the sun's centre, my new one (his planet (*a*) as I then supposed) must have had a Dec. almost identical with both, but it is clear that no object with a Dec. of 18° 16′ could range with the sun's centre, or anything like it if one were θ. This is what I meant when, in my reply to Peters, I said, "our difference in Dec. was a source of solicitude to me."

To show that you did not clearly understand the matter you corrected me, inserting in parentheses after Dec. these characters (? R.A.), as though I had made a mistake. No, I made no mistake, but meant just as I said. I had, at that time, but little anxiety about the R.A., supposing that the distance between us was not an irreconcilable one (being ignorant as yet of the error you afterwards pointed out), and this was the way I reasoned. The R.A. of θ was accurately known. I did not, however, know which was θ and which planet (*a*), but Watson wrote me the planet was nearest the sun, though he located it in R.A. 8h. 27m. 35s., which was too far east to agree with my observation. But I, with great reluctance, increased my estimated distance 1′, calling it 8′ instead of 7′, and, reducing this to time, erroneously called it 2m.; while it was really but 32s. This was as far east, or as near to him, as I could go without doing violence to my better judgment. Here arose the question, "Can I not bring Watson nearer to me?" He said: "I consider my observation trustworthy to within 5′ of arc." So I brought him 5′ farther west. Nearer to him I could not go, nor nearer to me could I consistently bring him, as he was certain no error had been made. After all, we were too far apart to harmonise things, and, after much reflection, I made another attempt to shorten the bridge over the chasm between us. I tried to imagine that the planet had just passed its inferior conjunction, and, during the five minutes that elapsed between our observations (mine being the later), it had retrograded a little. This was why I contended that it had just passed its inferior conjunction, and that the evidence adduced from their exceptionally large disks was inadequate to prove that it was approaching superior conjunction, when it would, of course, have a very large gibbous disk.

Up to this time the thought that I had seen anything else than θ and Watson's planet (*a*) had not entered my mind. Being unable to reconcile our difference in R.A., though I then supposed we were not far apart (having as yet no intimation of the above-named error), I turned my attention to the matter of difference of Dec., which I could see no way to reconcile, as it amounted to over 14′ as follows:—

		°	′	″
Dec.	+ Swift	= 18	30	25
,,	+ Watson	= 18	16	00
			14	25

The above Dec., as deduced by me, was published in NATURE, vol. xviii. p. 539, in which I also computed its R.A. to have been (erroneously, as before stated) 8h. 26m. 40s. Commenting on this letter, you pointed out the error of the reduction of the 8′ of arc to time. I instantly saw that 8′ was but 32s., and that we were really wider apart in R.A. than in Dec. Then I said in reply to Dr. Peters, "The scales fell from my eyes, and I was able to see my way clearly through the mystery, viz., that Watson's planet (*a*) and θ Cancri were not the objects seen by me."

Up to this point I have endeavoured to make the subject connected and plain, and if I have not then I despair of ever being able to do so.

I now return to your editorial, which, except what you say of myself, is a fair and candid one. Please allow me to quote a few lines from that part of it where you attempt to quote me in my reply to Peters: "He now writes that the difference in Dec. (? R.A.) shown by his own and Watson's observations had been a source of solicitude, and he could see no way to harmonise them till NATURE pointed out the error," &c. I said nothing of the kind, but something as different as the zenith is from the nadir. You, by inserting the characters in parentheses, make me say that I felt solicitude about the R.A. My concern was for the Dec. as I stated it, that of R.A. being nearly wiped out, as I then—unconscious as yet of the aforesaid mistake—supposed.

But the most curious thing of all is that you should interpret me as saying that Watson's and my own observations were harmonised by your detection and pointing out of the error, when just the opposite was the effect. It *dis*harmonised them, for it showed me that instead of our objects being quite near together in R.A., we were more than a half degree apart. This, coupled with our irreconcilable difference in Dec., caused, as I said, "the scales to fall from my eyes," &c. This matter, which at first sight might appear trivial, is a vital one in my defence,

and I wish to make out a perfect vindication, hoping never again to be obliged to recur to it. If you will refer to my reply to Peters, you will see that I speak of our difference in R.A. and stop, coming to a full pause. I then take up the subject of Dec., and when through with that, make another period. Then I say, "Thus the matter rested until NATURE pointed out the error, &c." Is not your language about as unlike this as can well be? In response to your wish to be able to tell your readers "how this sudden illumination caused the scales to fall from my eyes," I hope the above explanation will prove full and clear to all.

Your second charge, "hesitancy about the matter," is a new one, and so at variance with truth that necessity, even at the expense of being prolix, compels me to refute it, and to show to the world that this charge is as baseless as the other. How long did I hesitate? I answer, from the time of the eclipse until just two minutes after my arrival at home, when, though very weary and ill, and before I was seated, I consulted "Webb's Celestial Objects" to see how far Alcor was from Mizar. Then and only then was I able to fix on a definite distance between θ Cancri and, as I then supposed, the planet Vulcan, viz., about 7′. I left Denver the next morning after the eclipse, coming homeward, both by night and day, as fast as steam could bring me, arriving at home on the P.M. of Saturday, before most of the astronomers had left Denver. I immediately despatched a messenger to the Editor of the *Rochester Sunday Morning Herald*, notifying him of my arrival. I was at once interviewed by him, and a full account was laid before his readers by daylight the next morning. Sunday P.M. I was interviewed by a reporter of the *Rochester Democrat and Chronicle*, which paper, the next morning, contained a long account of my observations, a considerable part of which was published in NATURE. As soon as possible I wrote the facts to the Astronomer-Royal, to the *Observatory*, to Admiral Mouchez, and made out my report to Prof. Colbert, of Chicago (the chief of the party to which I belonged), which, with those of the other members, was published in pamphlet form, also a more extended one to Admiral Rodgers, not yet published. Very little hesitancy in this I think.

I left Denver with Professors Colbert and Hough. On the way Prof. Hough asked me several questions regarding the distance between the two stars. I told him I was unable to give their distance in arc, neither could I think of two stars whose apparent distance was the same. I also said to him that the nearest approach to a resemblance which I could then recall were α^1 and α^2 Capricorni, but, not having observed them with such an object in view, would not say that they were sensibly the same. After they had left me—changing to another road—and before my arrival at Kansas City, and before night of the day of starting, the thought came suddenly to my mind that their distance apart was about equal to a little more than half that between Mizar and Alcor, whatever that might be, which could not be ascertained until my arrival at home

Since the eclipse I have made many observations of θ Cancri and regions adjacent, to see if my judgment would allow me to modify in any particular my observations as made and published. I have even gone to a part of this city where the streets run parallel with and at right angles to the meridian, as they did at our camp, in Denver, and then wait until an imaginary sun some 30′ west of δ Cancri had the same altitude and azimuth as had the real sun during totality. And, while I am not inclined to make any changes whatever, I will say that it cannot be denied that, as regards the distance and direction from the sun, they can only be considered as rough guesses, though this does not militate in the least against the existence of the new objects. That they are new I know, for they are not there now. I have never made a more valid observation, nor one more free from doubt regarding the genuineness of the objects seen, which, in my opinion, were circumsolar bodies, unquestionably intra-Mercurial planets. The view of them was as beautiful as it was unexpected, and it was with great reluctance that I could break away from the captivating scene. It must be borne in mind that my telescope was filled with a flood of light, with not an object for reference visible, and therefore, when I ran upon these two round red disks, equally bright, and so near together, it is not surprising that they made an impression upon my mind that never will be effaced.

The great field for future astronomical discovery will, without doubt, be the sun and his immediate surroundings. Let no man's prejudice deter him from taking part in such prospective discoveries, for the field promises rich rewards.

Though I have said above that I am not inclined to modify my published estimations, yet I am willing to say as follows:—If I were compelled to change the brightness of the two stars one magnitude, and say whether they were of the fourth or sixth, I should answer, the former. If I were compelled to change their distance from the sun half a degree, and say whether they were $2\frac{1}{2}°$ or $3\frac{1}{2}°$, I should say the latter. Again, if I were compelled to change their direction from the sun, and say a little farther south or north, I should unhesitatingly say the latter, or, as I said in my report to the Naval Observatory, south of west, instead of south-west. And, finally, were I obliged to change their distance apart, and declare whether they were 6′ or 8′, I should, without a moment's hesitation, say the former, or about the distance between α^1 and α^2 Capricorni.

LEWIS SWIFT

Rochester, N.Y., December 10, 1879

The Transverse Propagation of Light

IN NATURE, vol. xxi. p. 256, appeared a paper by Mr. Tolver Preston, on which I wish to make a few remarks.

The author does not make himself very clear as to what he supposes the effect of the vibrating molecules of gross matter on the ether atoms to be. From what I can gather, the effect on a small plane receiving the light from an illuminated "point" would be of the following nature:—When the molecule of gross matter was not vibrating, there would be a more or less shaded spot on the plane, but if the molecule vibrated, then this shaded spot would also vibrate in the same time, which would be possible, since during one vibration of the molecule an extremely large number of ether atoms would impinge on it, and therefore, a large number at each portion of its vibration. In what follows I shall suppose that this is the manner in which the light is supposed to be propagated.

1. The atoms are very small; the free paths are very long. In order that the acceleration of the sun on all the planets must be inversely proportional to the squares of their distances, this mean path must be comparable with the radius of Neptune's orbit; and in order that the light of the stars may be visible, it must be comparable with the distance of the furthest visible star. Again, since, as Mr. Preston says, the automatic adjustment to equality of direction is "of such a rigid character, that if the atoms were imagined to be disturbed or made to move in the most chaotic manner, they would, when left to themselves, instantly correct the irregularity," it follows that the time of describing the mean free path must be very much smaller than the "instantly" small time in which they "correct the irregularity." Their velocity, therefore, must be enormous. They must move to the farthest visible star in a very small fraction of a second. That they have a very large velocity also follows from the smallness of the atoms and the magnitude of gravitation. Now the velocity of light on Mr. Preston's theory must be the velocity with which the atoms move, a velocity which, as has been shown, must be enormously greater than 200,000 miles a second.

2. The above supposes the velocity of all atoms the same, which would not be true. If they varied in the same way as in a gas composed of atoms which do not influence one another, then at a distance from the illuminated point, after a few vibrations of the gross molecule, the shaded spot would not vibrate, but would become an elongated shaded spot without motion, and there would be no light all.

3. The data of the theory are definite, and it therefore ought to be capable of explaining the laws of refraction and reflection, let alone those of diffraction. This it is incapable of doing; for the light that gets through must be carried by atoms which pass through without striking any of the molecules of gross matter; they must therefore pass through without change of direction or velocity, and therefore cannot be deflected.

These are three reasons, each of which by itself condemns the ingenious explanation offered by Mr. Preston.

W. M. HICKS

St. John's College, Cambridge, January 16

Mountain Ranges

IT is to be regretted that Mr. Trelawney W. Saunders should make confusion worse confounded by noticing imaginary discrepancies based upon a mistaken assumption of a natural agreement. In his paper "On the Mountains of the Northern and Western Frontier of India," published in NATURE, vol. xxi. p.

96, he takes geologists to task for not making their descriptions to fit in with his delineation of purely superficial features. He reproaches the authors of the "Manual of the Geology of India" with adopting an "antiquated theory" which had been disposed of by his demonstration of a second line of peaks in the Himalayan range. The omission to account for such apparent neglect of recent discovery was solely due to the perceptions of its almost irrelevancy to the matter in hand. The old familiar feature for which Mr. Saunders claims such geographical importance (which the writers were not concerned to dispute) happens to be of quite incidental significance in the mountain-structure, and much more in accordance with "the antiquated theory" than with the independent position Mr. Saunders would assign to it. Also, the fact that the great gneissic axis of the Himalayan range divides into several minor axes west of the Sutlej, and that these disappear under fossiliferous formations before reaching the Indus, will probably be held by geologists as sufficient reason for considering this ground as the natural termination of the range. On the other hand, the fact that there should be a continuous watershed between these terminal ridges and the contiguous ridges of a confluent system of disturbance, will be admitted by geologists as sufficient for a combined hydrographical delineation of the two systems, as proposed by Mr. Saunders. The points of view of the pure geographer and of the geologist are at present so wide apart that it is irrational to represent them as conflicting.

H. B. MEDLICOTT

Calcutta, December 31, 1879

Ice Filaments

THE phenomenon alluded to by the Duke of Argyll in NATURE, vol. xxi. p. 274, is not at all of unfrequent occurrence. I remember having been struck by the beauty of these ice-filaments on dead branches in Epping Forest many years ago, and some friends of mine observed some beautiful specimens of such branches in Surrey some few weeks since. The explanation which I have been inclined to give is the following:—During the moist weather preceding the frosts, the dead branches on the ground become sodden with water; the interstices between the cells of the dead ligneous fibre get saturated by capillarity, and the branches become water-logged. Now if a certain amount of dry weather intervenes between the moist period and the frost, this absorbed water would have time to partially evaporate and leave the branches more or less dry. But if the frost *immediately* follows the moist period—as pointed out by the Duke of Argyll—there is no time for the drying of the branches, and the interstitial water becomes frozen *in situ*. Under these circumstances the expansive force of the ice would cause it to flow out from every available pore by virtue of its viscosity, and such I take it is the origin of the filaments observed. Those portions of the branches which are protected by bark are sheathed by the latter in such a manner that the ice is prevented from oozing outwards; but my friends who have recently observed the phenomenon inform me that where the bark was partly separated from the wood beneath it so as to leave a small intermediate space, this space was likewise filled with filamentous ice.

All physicists are familiar with the experiment of submitting fragments of ice to great pressure in a steel mould with an opening in it. The ice becomes consolidated by regelation, and flows out of the opening in a continuous thread. The state of affairs in frozen water-logged branches could thus be imitated by having a steel mould sufficiently strong to bear the pressure, completely filled with water and perforated by capillary holes, and then freezing the contents. The ice would, under these circumstances, flow out of the capillary holes in the filamentous form observed, and if a metal band were firmly fastened round the mould so as to sheath a certain zone of the capillary holes, no ice could appear in this zone, which would thus represent the portions of the branches protected by bark.

From the point of view of this explanation, which I venture to submit for the judgment of physicists, the Duke is hardly correct in speaking of this filamentous form of ice as an "ice-crystal."

R. MELDOLA

21, John Street, Bedford Row, W.C., January 23

THE filamentous form of ice-crystal, described by the Duke of Argyll as occurring upon rotten wood when a frost sets in suddenly after moisture, is by no means uncommon also upon chalk and other porous kinds of stone. It appears to arise from the water with which the body is soaked being extruded by the expansion due to cold when near its freezing-point, and becoming solidified as it passes the surface of the substance. It is, as it were, spun out of the pores of the rotten wood or porous stone. This explanation accounts for the fact, noticed by his Grace, that this form of crystal is not found upon those parts of a decayed branch upon which the bark is unbroken.

Harlton, Cambridge, January 23

O. FISHER

WHILE residing upon the South Downs I observed, during hard frosts, that prisms of ice exuded from small pieces of chalk, and having their sections identical with the piece of chalk. It is clear that the prism was formed by the moisture passing through the chalk by capillary attraction. May not this explain the formation of the filaments described by the Duke of Argyll?

H. KING

The Kangaroo

I NOTICE in NATURE, vol. xx. p. 511, in a lecture on "Tails," the following remarks in reference to kangaroos:—

"These creatures make use of their tails not only sometimes to carry grass, and to a certain extent in their jumps," &c. Permit me to state that the former statement is perfectly erroneous and the latter one is correct only in a very modified degree. Kangaroos *cannot* use their tails to carry grass, and never attempt it, and the use of their tails in jumping is confined to balancing the body, and whatever leverage may be exerted in the swaying of it when in motion. The tail *never* touches the ground in going. Twenty years' observation in three colonies is my authority for saying so.

ALFRED MORRIS

Sydney, N.S.W., December 30, 1879

Chinese Geese

IT may interest some who read Mr. Darwin's note on this variety, to know that there are—or were only a few months ago—a rather large number of hybrids, of apparently all grades, at the Bristol Zoological Gardens. When I was there in September there was quite a respectable flock, pure Chinese being among them.

I have not unfrequently found both the pure variety and hybrids in the country, and have usually found that the people regarded them merely as a variety. The differences mentioned by Mr. Darwin seem scarcely so great as those presented by the Polish fowl—which also, by the way, seems almost to have been regarded as a species by some naturalists of good repute.

LEWIS WRIGHT

The Molecular Velocity of Gases

IN NATURE, vol. xxi. p. 201, which reached me only recently, I find a letter of your correspondent "R", to whom I am much obliged for having pointed out to me an error into which I had fallen, in common with many others. I may quote, *e.g.*, the exhaustive work of Rühlmann,[1] where, in the chapter on the history of the molecular theory, Joule is only alluded to, and immediately afterwards the theory of Krönig is given *in extenso*, without any hint that it is practically identical with that given by Joule in 1848. Having read "R's" letter, I immediately procured the original article of Joule, and I am now ready to admit that Joule's article contains all that is essential to Krönig's method of computing the velocity of gas molecules. It is true, the formula itself as an algebraical expression is not found there, but the calculations given are to all purposes equivalent to the formula.

It is scarcely necessary to add that this makes no difference at all in reference to the contents of my letter in NATURE, vol. xxi. p. 176, referring, as it does, only to the historical footnote.

L. HAJNIŠ

Prague, Spálená ulice, 2 nové, January 20

Suicide of the Scorpion

SINCE writing mine of the 12th inst. I have, I believe, discovered in Byron's "Giaour" the scientific (?) flight of fancy upon which Dr. R. F. Hutchinson based his *central glowing*

[1] "Handbuch der mechanischen Wärmetheorie."

inference in his last letter (NATURE, vol. xxi. p. 226). Here you have it:—

"The Mind, that broods o'er guilty woes,
Is like the scorpion girt by fire;
In circle narrowing as it glows,
The flames around their captive close,
Till inly search'd by thousand throes,
And maddening in her ire,
One sad and sole relief she knows:
The sting she nourish'd for her foes,
Whose venom never yet was vain,
Gives but one pang, and cures all pain,
And darts into her desperate brain."

I hope to tax your patience no further on scorpion *felo de se*.

Prov. de Jaen, Linares, Spain, January 17 F. GILLMAN

Meteor

A MAGNIFICENT meteor was seen here last Monday evening (19th inst.) at 6.8 P.M. The meteor when first observed had an elevation of about 30° above the horizon and was travelling due west. It appeared to me to be at least four times the size of Jupiter and much more brilliant, the colour being bright blue. It seemed to be moving comparatively slowly and was in sight for some two or three seconds. When still about 15° above the horizon it burst, sending forth a number of different coloured sparks, in fact strikingly reminding one of the bursting of a sky-rocket. No report could be heard after the explosion. I may add that the night was very fine and the moon bright, and that a number of small meteors were seen at the same time.

West Calder, N.B., January 21 J. S. THOMSON

ON HALLEY'S MOUNT

"Hoc primum ab homine Anglo invertum fuisse non inficiabitur æqua posteritas."

IN Mrs. Gill's account of her voyage to Ascension,[1] she relates how her husband (since appointed astronomer at Cape Town) visited Halley's Mount, a prominent spur on the northern declivity from Diana's Peak, the central culminating point of the Island of St. Helena. Here, on a small plate u, the sight of a few roughly-squared blocks of tufa cannot fail to inspire the beholder with deep interest, for these stones, now overgrown with wild-pepper and blackberry brambles, are all that remain to mark the site of a celebrated astronomical station.

The neglected state of these ruinous foundations,

"In which there was obscurity and fame,
The glory and the nothing of a name,"

contrasts in a marked manner with the "exquisite neatness" (as Mrs. Gill terms it) which distinguishes the cœnotaph of Napoleon[2] in the so-called "Vale of the Tomb" several hundred feet beneath.

Here it was that Edmund Halley 200 years ago established his observatory, and first constructed his "Catalogus Stellarum Australium;" here he observed the transit of Mercury, and wrote his method of obtaining the sun's parallax by the forthcoming transits of Venus, and here made the first[3] magnetical observations in the southern hemisphere.

On the eve of Mr. Gill's astronomical experiment at Ascension, then a matter of uncertain expectancy, now happily a successful *fait accompli*, no wonder is it that a sincere sympathy with the aspirations of his predecessor determined him to some day find the means and opportunity to raise a memorial on the spot.

To astronomical students the apotheosis of the great Halley is immortally celebrated by the comet which bears his name; but to the "*profanum vulgus*" the mention of Dr. Halley conveys no conception of his genius nor of the practical scientific benefits he bequeathed to the English nation. It was Delambre who, speaking of Halley's "Synopsis Astronomicæ Cometæ," said (*Ast. Siècle*, xviii. p. 310): "Voilà bien, depuis Kepler, ce qui on a fait de plus grand, de plus beau, de plus neuf en astronomie."

It is a fact hardly yet appreciated either in England or America, that Dr. Edmund Halley is second only to Isaac Newton, whose friend and contemporary he was (Newton's "Principia" was first printed in 1686-7 at Halley's expense), and that it is to this close contemporaneity alone that the bright light of Halley's star has suffered diminution of lustre from the brilliant rays of his world-renowned neighbouring luminary.

No biographer has yet appeared to write the life of this great man, nor does any public monument yet adequately represent the national estimation which is so richly deserved by the second most illustrious of Anglo-Saxon philosophers. The first of these two reproaches is, we believe, on the eve of being wiped away; for we learn that Prof. Pritchard[1] of the Oxford University, to whom (as holding the Chair of Astronomy denied to Halley by Stillingfleet) pertains the honour of compiling so valuable a biography, is preparing for the press a full account of the long life-work of the venerable astronomer.

It is to remove the second of these wants that we now would advocate, through the columns of NATURE, the erection of a fitting memorial to our illustrious countryman on the spot which is indissolubly connected with his name, as the scene of his famous achievement.

The onerous duties of the astronomer at Capetown have prevented his doing more than suggesting the idea of a monument to Halley and the most appropriate site; it now remains for us with more leisure at home to forward the idea, and do our utmost to carry out his well-intentioned scheme; nor need we fear that it will be lost sight of and fall to the ground, now that it has been brought forward to the notice of our scientific societies. This recognition of the claim of Halley to his proper place on the roll of English scientific worthies, although somewhat tardy, need not therefore be the less hearty and thorough now that it takes place. It is now some seven or eight years since the Tuscans expended nearly forty thousand pounds in a memorial to their "Divinus Galilæus," at Florence;[2] and in 1874 the preparations for observing the transit of Venus recalled to our minds the hitherto obscure memory of the long-forgotten Jeremiah Horrocks. Surely we need not wait for the advent of the next transit in 1882 to remind the present generation what they owe to the St. Helena observer of 1677. Have we not therefore established the fact that it is desirable to erect a memorial to Halley on the ancient site of his observatory in St. Helena?

Receiving in anticipation an affirmative reply from our readers in answer to the question above, we may now approach the next stage of our subject by inquiry as to the form which such a memorial should take; and the fact is that it matters very little in reality whether tablet or bust; whether column, pyramid, or statue be chosen, so long as it is not too ornate.[3] The simplest and most

[1] See NATURE, vol. xix. p. 240. "Six Months in Ascension. An Unscientific Account of a Scientific Expedition." By Mrs. Gill. (Murray, 1878.)

[2] Darwin says: "After the volumes of eloquence which have poured forth on this subject, it is dangerous even to mention the tomb. A modern traveller, in twelve lines, burdens the poor little island with the following titles: it is a grave, tomb, pyramid, cemetery, sepulchre, catacomb, sarcophagus, minaret, and mausoleum!" ("A Naturalist's Voyage," p. 486.) Darwin's lodgings at Hutt's Gate were within a stone's throw of Halley's observatory, of which fact he appears to have been unaware; and, similarly, neither Napoleon nor any of his staff appear to have remarked the scientific associations of Halley's Mount during the six years they were resident at Longwood; a circumstance the more curious, as Napoleon always patronised science, perhaps less for its own sake than from motives of policy.

[3] In 1667 Halley found the variation of the compass to be 40′ E., it is now 24° W.

[1] See *Monthly Notices*, Royal Astronomical Society, December, 1875, p. 51. Large materials for a life of Dr. Halley were found among the papers of the late Prof. Rigaud, which will be edited by Prof. Charles Pritchard, M.A.

[2] "Tuscan Memorial to Galileo," by G. F. Rodwell (NATURE, vol. viii. p. 328, August, 1873)

[3] The sketch of one design has been shown us, consisting of a pyramid whose four sides are inclined at an angle of 70° with the base standing on a podium, which is dodecagonal surrounded with seats. The faces of the pyramid face the cardinal points. On the north face is Ursa Major, and on

severe design alone will suit the locality, which we will presently describe, and may safely be left to the discretion of a committee of taste by a general meeting of the subscribers to the memorial as only a small sum of money need be expended on this object; but we would ask if this alone will appease the manes of Edmund Halley? We must give further explanations.

Within sight of Halley's Mount are *two* disestablished observatories. One, the most important, is that on Ladder Hill, with this inscription over the doorway: "HÆC SPECULA ASTRONOMICA Condita fuit A.D. MDCCCVII." This was Johnson's observatory, broken up when the Imperial Government took the island from the East India Company in 1834. It is now used as a mess-room for the officers, R.A. and R.E., at Ladder Hill.[1] The other is at Longwood, and was established in 1840 by Sabine as a magnetic and meteorological observatory; this station was broken up in 1845. In front of it on Deadwood Plain is the base-line measured by Lefroy, 2,986·3 yards in length.

The re-establishment of these valuable observing stations would indeed be a lasting memorial such as Halley would approve. Of the fitness of the first-named station as an astronomical observatory, we need only judge from the actual work accomplished there by Johnson and by Gill's appreciation of its position and capabilities. Of the second it will be manifest to all meteorologists, what an advantage such an establishment in the heart of the south-east trades would be to science; whilst Halley's magnetical researches could be renewed in an island where the atmospherical electricity is so seldom disturbed that lightning conductors are never fitted to the powder magazines, and where distant thunder is heard seldom more than once in a generation.[2]

A few more words may not be out of place to describe Halley's Mount. Nearly in the centre of the island it commands from its elevation of 2,400 feet, the whole of the northern portion of St. Helena. Four miles looking due north is the northern extremity of the island called Sugar-loaf, and four miles to the right is Dry-gut Bay and Stone-top, whilst the same distance to the left or west, is Bennett's Point. Throughout the whole of this northern semicircle, the view is bounded only by the sea horizon. Behind us the crateral ridge just under 3,000 feet hides the southern coast, which is distant only three miles in Sandy Bay. But although this ridge hides the view, it forms a fine background and shelter against the southern winds. Above Halley's Mount the mountain-tops are covered with indigenous vegetation, shrubby Campanulaceæ, Scævoleæ, mosses, lycopods, and arborescent Dicksonias, and the peculiar composite trees with cauliflower-like blossoms, much the same as when Halley was here two centuries ago; but beneath us, how changed. As Sir Joseph Hooker observed in a lecture on "Insular Floras," at the Nottingham meeting of the British Association in 1866, in reference to St. Helena:—"When discovered about 360 years ago, it was entirely covered with forests, the trees drooping over the tremendous precipices that overhang the sea. Now all is changed, fully five-sixths of the island are utterly barren, and by far the greater part of the vegetation which exists, whether herbs, shrubs, or trees, consists of introduced European, American, African, and Australian plants." On Halley's Mount the indigenous and exotic plants meet on equal terms, a fit vegetation to surround a cosmopolitan relic.

In the present day the most conspicuous features in the landscape of St. Helena, as viewed from the highlands, are the sombre plantations of pinaster (only introduced in 1787), which contrast strongly with the willow-leaved acacias of New South Wales; whilst on all sides are ever wider extending acres of *Phormium tenax*, grown for the sake of its economical fibre, and whose seeds afford capital fare to the numerous Chinese pheasants which inhabit the covers on the sides of Halley's Mount.

the south Crux. On the east the inferior planets, and on the west the comet. On the twelve seats are the signs of the Zodiac (has any one remarked that the conventional signs of the Zodiac have become a recognised ornamental pattern on the jewellery made by the natives on the west coast of Africa?) and the names of astronomical workers in the southern hemisphere.

[1] See "Six Months at Ascension," p. 26. David Gill's feelings at viewing this degraded observatory, remind us of Halley's disappointment on reaching Greenwich, on his appointment as King's Astronomer, 1720, and finding that the executors of Flamsteed had removed all the instruments. See Whewell's "Inductive Science," vol. ii. Compare also the desolation of Uraniburg.

[2] We have not been able to ascertain in which observatory Capt. Foster's pendulum experiments were carried on between 1828-1831, but we presume in Johnson's observatory; nor are we sure where Maskelyne's station was.

THE U.S. WEATHER MAPS

WE are again enabled, through the courtesy of Gen. Myer, of Washington, to present our readers with two Weather Maps of the War Department of the United States, which graphically present the mean pressure and temperature for the whole of the Northern Hemisphere of the earth for April, 1878, and the tracks of the centres of storms for the same month. As these maps are constructed from the observations of all the stations reporting to the Army Signal Service, they must be held as very accurately representing the meteorology of the period, and they may serve to show the extraordinary energy with which this well-directed meteorological system is conducted and turned to account in the interests of the public and of science.

The outstanding characteristic of the weather of 1878 was its extraordinarily high temperature to the east of the Rocky Mountains, chiefly in the upper valleys of the Missouri and Mississippi, and the Lakes region, rising in the latter to nearly 11°·0 above the mean of the month. April is one of the months in which the western prairies receive their annual maximum of rain, but during April, 1878, this maximum rose greatly above its normal amount, the rainfall of the basins of the Mississippi and its affluents, with the exception of Ohio, being generously large. In Minnesota the fall was nearly four inches in excess of the average. The region of absolutely heaviest rainfall covered a broad track extending from St. Louis, Mo., to Florida.

These characteristics of the distribution of the temperature and rainfall were impressively felt in the singular distribution of atmospheric pressure, which everywhere was under the average of April, but most pronouncedly so to the west and north-west of the regions of the extreme excess of temperature and rainfall. The deficiency at Omaha amounted to fully two-tenths of an inch, an unusual deficiency for that region and season.

Turning now to the map of the tracks of the centres of the storms of April, 1878, we observe that most of them group together and lingered longest in this very region of low pressure, and that immediately to the *east and south-east* lay those regions where temperatures ranged so unusually high, rain fell so copiously, and thunderstorms played so strikingly brilliant a *rôle* among the weather phenomena of the month.

These tracks of the different storm centres admirably illustrate some of the more prominent types of the States' storms. Storm No. I. is seen to branch into two shortly after it began its advance on the States, the one passing northwards and dying out after one day's course, near the Cumberland River, whilst the other pursued a north-easterly course toward Newfoundland. No. II. originated to the east of Pike's Peak, and after a two days' course to north-north-east, was lost sight of in Canada for want of the observations necessary to trace its course over that part of the Dominion. Whilst this storm had its centre over Minnesota, a deep barometric trough ran southward into the Gulf of Mexico—a feature of American storms of no unfrequent occurrence—and the rain area extended eastward over the Lakes, the middle, and South Atlantic States, with frequent heavy thunderstorms, accompanied with hail. Storms IV. and V. illustrate the coalescence

of two storms; storms V. and VI. advanced from the Pacific, crossed California and the Rocky Mountains, and thence swept eastward over the States; and storm VIII. began its course near Chicago, ran out south-eastward to near Cape Hatteras, and then recurved in the direction of Niagara, where it died out after having traced a course nearly elliptical. It is to an exhaustive treatment of a tolerably large number of instances of these different types of storms, that we must look for the key of the mystery of the genesis, progress, and termination of the cyclone which comprehends within itself by far the major portion of all weather changes. Towards this great and perhaps not far distant result, nowhere is any meteorological system making contributions so large and so effective as is Gen. Myer through the munificent liberality of the United States Government.

DIFFUSION OF COPPER IN THE ANIMAL KINGDOM

THE fact of the normal presence of minute quantities of copper in various members of the animal kingdom has been noticed by several chemists within the past twenty-five years. Kingzett states that he has invariably found it to be a constituent of the human brain, while Odling and Dupré, and Bergeron and Hôté have determined analytically the average amount of copper present in the liver and kidneys of human beings and domestic animals. In the latter case the average percentage of copper found was about 35 millionths. Some two years since Cloez examined the blood of a deer, and found it to contain 6 millionths of copper. The most interesting instance of the occurrence of copper in the animal creation is, however, that communicated by Prof. Church to the Royal Society in 1869. At this time he was engaged in the investigation of a peculiar, soluble, red colouring matter present in the wings of the Turaco, a bird from the West Coast of Africa. A thorough study of this pigment showed it to contain 5·8 per cent. of copper, and Prof. Church established for it the formula $C_{50}H_{50}O_{19}NCu$. Led to seek the source of this strange factor in the animal economy of the Turaco, he succeeded in detecting copper in the fruit of the *Musa sapientum*, which forms the chief article of the bird's diet.

To these few isolated cases of the normal assimilation of copper in the animal kingdom, Dr. M. Giunti, in the last fascicule of the *Gazetta Chimica Italiana* (vol. ix. p.541), adds a number of interesting and diversified instances.

His attention was first directed to the subject accidentally by finding over one-third of 1 per cent. of copper in the guano deposits from bats occurring in certain Italian caves. This led to an analytical examination of the bat, the results of which showed that about four ten-thousandths of the weight of the ashes of this animal consist of cupric oxide. Still bent upon finding a more ultimate source for the metal, Giunti has subjected to analysis quantities of the insects which form the food of the bat, and in all cases he has found copper present in greater or less amount. The quantity would seem to vary in the different orders, families, and species. Aquatic insects contain less than those found on land, and the Coleoptera appear to yield the highest percentage. Thus the ashes of *Anomola vitis* contain 0·1 per cent. of cupric oxide, and those of *Blatta orientalis* 0·826 per cent. High as this percentage seems, the amount of copper in an individual insect is infinitesimal, being, in the case of *Anomala vitis*, less than four-millionths of a gramme. Copper was also detected amongst other Coleoptera (such as *Cetonie, Cerambix, Ateucus sacer, Leurus striatus*, and notably the lava of *Srillotalpa*); amongst Diptera (*Mosca domestica*), Lepidoptera (*Vanessa cardui, Piaris sinapis, Limenites camilla*, &c.), and Hymenoptera (*Æschena maculatissima, Libellula depressa, Calabroni*, &c.).

Giunti has next sought to ascertain whether other insectivorous animals besides the bat are wont to assimilate the copper present in their insect prey. This was found to be the case with all members of this class subjected to examination, such as snakes, lizards, urchins, &c. The ashes of the latter contain from one to two ten-thousandths of copper, while the ashes of lizards contain over fifteen thousandths. In their case most of the copper is to be found in the skin of the animal.

Giunti's experiments have likewise been extended amongst the invertebrates. Various varieties of spiders; of myriapods, such as *Julus terrestris;* of isopods, such as *Armadillidium vulgare;* and of snails, have all given affirmative responses to his tests. Amongst these, *Julus terrestris* contains the largest amount of copper, its ashes showing a percentage of 0·18.

The investigations of the Italian chemist in this novel branch of physiological chemistry are still being continued, and it is to be hoped that more extended observations will inform us of the exact nature of the *rôle* played by cupric compounds in the animal economy.

T. H. NORTON

NOTES

NEXT week we publish an extra number entirely devoted to an account of the life and work of M. Dumas, the eminent French chemist, and one of the greatest of living Scientific Worthies. Dr. Hofmann, of Berlin, has been good enough to devote a great deal of time and research to this paper, and has treated the important subject in such detail that, owing to the pressure on our space at present, it is impossible for us to find room for this long article in the ordinary way, and we are therefore compelled to devote to it an extra number. We are sure our subscribers will give us their willing approval and support in an emergency so very special, and all will doubtless be glad to have this sketch of an eminent French chemist by so eminent a German *confrère*.

A PAPER has been circulated by the Perpetual Secretary of the Paris Academy giving notice that M. Maindron has been officially commissioned to collect under their authority the archives of the Academy, in a locality belonging to the Institute. Persons possessing documents available for that purpose are requested in the name of science kindly to send them. A fair example has been recently given by M. Bornet, whose liberality has been publicly acknowledged. M. Etienne Charavay, the expert in autographs, has recovered on behalf of the Institute a number of documents which had belonged to the Academy.

THE Society for the Promotion of Hellenic Studies, which was inaugurated in June last, held its second general meeting on Thursday, January 22, at 7, Adelphi Terrace, Mr. C. T. Newton in the chair, when the rules drawn up by the Committee were adopted, the Bishop of Durham elected President, and other officers settled as follows:—Vice-Presidents: Lord Morley, Mr. Justice Bowen, the Dean of St. Paul's, M. Gannadius, Mr. Newton, Mr. E. Maunde Thompson, the Master of Trinity College, Cambridge, Prof. Colvin, Rev. H. F. Tozer, Prof. Sayce, Prof. Jebb, and Prof. T. K. Ingram. Council: The Bishop of Lincoln, the Dean of Westminster, the Dean of Christchurch, the Rector of Lincoln College, Oxford, Sir John Lubbock (Treasurer), Sir Charles Dilke, Professors Bryce, Hort, Kennedy, Mahaffy, B. Price, H. J. S. Smith, Tyrrell, Messrs. A. J. Balfour, M.P., Oscar Browning, J. Bywater, W. W. Capes, H. O. Coxe, T. Chenery, E. A. Freeman, Percy Gardner, George Macmillan (Hon. Sec.), Ernest Myers, D. B. Monro, J. Cotter Morison, H. F. Pelham, F. C. Penrose, Walter Perry, J. A. Symonds, and Oscar Wilde. The objects of the Society, as stated in the outset of the Rules, are:—1. To advance the study of Greek language, literature, and art,

and to illustrate the history of the Greek race in the ancient, Byzantine, and Neo-Hellenic periods, by the publication of memoirs and unedited documents or monuments in a journal to be issued periodically. 2. To collect drawings, fac-similes, transcripts, plans, and photographs of Greek inscriptions, MSS., works of art, ancient sites and remains, and with this view to invite travellers to communicate to the Society notes or sketches of archæological and topographical interest. 3. To organise means by which members of the Society may have increased facilities for visiting ancient sites and pursuing archæological researches in countries which, at any time, have been the sites of Hellenic civilisation.

AMONGST the prizes offered by the Istituto Reale Veneto di Scienze e Lettere at Venice we mention the following:—(1) 1,500 lire (about 58*l.*) "for a detailed description of the determinations hitherto made of the mechanical equivalent of the heat unit, investigation of causes, &c.; (2) 3,000 lire (116*l.*) "for a representation of the advantages which the application of physics has brought to medical science, and to clinical medicine in particular;" (3) 3,000 lire "for a summary of the recent investigations in theoretical hydrodynamics, followed by a representation of the true and essential progress made in this part of scientific mechanics;" (4) 3,000 lire "for a description of the most recent hypotheses in physical science concerning the phenomena of light, heat, electricity, and magnetism, followed by an indication of the changes which scientific language would have to undergo in order to be in accordance with the best founded theories, this indication to be illustrated by some examples describing some of the principal phenomena." The competition for the first and fourth of these prize-themes ends on March 31 next, that for the second and third on March 31, 1881. For further details we must refer our readers to the Institution itself.

ON his passage through Rome, Dr. Gerhard Rohlfs was received in special audience by the King of Italy, who personally decorated the great traveller with the Commander Cross of the Italian Order of the Crown.

THE Royal Academy of Sciences at Turin has awarded the Bressa prize for the four years 1875 to 1878, to Mr. Charles Darwin.

WE had occasion some time ago to call attention to the excellent scientific work which is being carried on at the Carlsberg Laboratory, Copenhagen. This laboratory of research, it will be remembered, was founded and endowed by Mr. J. C. Jacobsen with the intention of aiding, as far as possible, in placing upon a secure scientific basis the technical processes of brewing and malting. We have now before us a Report of the work carried out during the past year. This is published under the title of "Meddelelser fra Carlsberg Laboratoriet" by the committee of management appointed by the Royal Danish Academy of Sciences. The original report is in Danish and is accompanied by a very full *résumé* in French. We append the titles of the principal papers embodied in the Report:—"Contributions à la Connaissance des Organismes qui peuvent se trouver dans la Bière et le Moût de Bière et y vivre," par E. Chr. Hansen. "Sur l'Influence que l'Introduction de l'Air atmosphérique dans le Moût qui fermente exerce sur la Fermentation," par E. Chr. Hansen. "Recherches sur les Ferments producteurs de Sucre," par J. Kjeldahl. (1) Recherches sur la Diastase; (2) Recherches sur la Ptyaline (Diastase de la Salive).

WITH regard to distinguishing artificial from natural butter, M. Donny remarks, in a recent note to the Belgian Academy, that the two behave very differently when heated between 150 and 160 degrees in a capsule or test-tube. At this temperature artificial butter produces very little froth, but the mass undergoes a sort of irregular boiling, accompanied by violent jerks which tend to project some of the butter out of the vessel. The mass grows brown, but this is by reason of the caseous matter separating out in clots on the walls; the fatty portion of the sample sensibly retains its natural colour. Natural butter, on the other hand, heated to 150° or 160° produces abundant froth, the jerks are much less pronounced, and the mass grows brown but in a different way. A good part of the brown colouring matter remains in suspension in the butter, so that the whole mass has a characteristic brown aspect similar to that of the sauce called *au beurre noir*. All natural butters behave thus, and it is strange, M. Donny says, that this simple method of distinguishing natural from artificial butter has not been indicated before.

A BODY of Russian *savans* is expected to go next spring into the Slavonic Balkan provinces to study their geology and ethnographically examine the palæographic architectural remains. The expenses of this expedition are to be defrayed by the Russian Geographical Society and a Slavonic committee.

THE death is announced at New Braunfels, in Texas, of Ferdinand Lindheimer, a German botanist, long settled in Texas, for the botany of which he did much by the valuable collections he made.

SEVERAL shocks of earthquake were felt at Havana on the night of January 22. On Sunday last two slight shocks were felt at Carlsruhe.

A MUNICH correspondent describes an interesting anatomical model recently constructed by Prof. Rüdinger of that city. The model represents a whole human body, life-size, which can be taken to pieces in eight different ways. The sixteen section planes thus obtained show most minutely all anatomical details. The model was executed, under the learned professor's direction, by Messrs. Zeiller.

THE *Gazette de Lausanne* of January 20 publishes a very interesting letter by Dr. Forel, on the probability of the Lake of Geneva being frozen during this winter. After having made several measurements on January 15, Dr. Forel proved that the temperature of water throughout the lake (at a certain distance from the shores) was on that day equal to 5°·2 Celsius. Now comparing this figure with the temperature of water measured at various depths on October 23, 1879, he concludes that the water of the lake has lost during eighty-five days no less than thirty calorific units for each square centimetre of its surface, and that it must lose twenty-four units more to reach the temperature of maximum density (4° Celsius), when a superficial freezing might become possible. The laws of freezing are but imperfectly known; but applying to the Lake of Geneva the results of measurements he has made during December last on the frozen Lake Morat, Dr. Forel concludes that the waters of the former lake must lose eight calorific units more to lower the temperature of the water at the surface to the freezing-point. Thus the waters of Lake Leman must lose altogether thirty-two calorific units per square centimetre of surface before any freezing would become possible. The lake having lost but thirty units from October 23 to January 15, we ought to experience a period of cold of the same intensity as that which was experienced during the last three months, for the freezing of the lake. But, according to the computations of Prof. Plantamour, it would be highly improbable that the cold December of 1879 should be followed by a January as cold as that of 1830. Thus, it is highly improbable that the Lake of Geneva will freeze during this year, but it is possible that the "Little Lake" (*i.e.*, its south-western part) might freeze in January. January, however, is near an end, and we have not yet heard of the lake being frozen.

THE ice on the Loire continues to occupy the French engineers. The works are proceeding actively but not very

favourably; more frosty weather having prevailed the water freezes behind the boats of the men trying to open a channel in the ice-barrier. Immense disasters are anticipated from the thaw if some means are not found to work more effectually. It is stated that the block was formed principally in consequence of the situation of the bridge of Saumur, which some competent engineers proposed to demolish many years ago as creating a danger on the occasion of inundations. The proposal was renewed during the present crisis without having met with any success.

THE Canal Saint Martin, which is used so largely for provisions of Paris, has also been entirely frozen, and the blocks of ice not having melted, as in the Seine, the Director of the City Works is busy in disencumbering it as much as possible. The difficulty is not so much in cutting the ice as in sending it into the Seine by the flood gates. Although having a length of only a few kilometres, the Canal St. Martin has so many locks, that the problem of freeing it is one of the most difficult than can be imagined.

THIS week the Commission of the Municipal Council of Paris will deliberate upon the desirability of continuing the experiments on electric lighting in the Avenue de l'Opéra. Since the article by M. de Fonvielle was written, the Siemens brothers have exhibited their lamps on one of the largest confectionery shops on the Boulevard Montmartre. It works very well, and creates some sensation in Paris.

AT the last meeting of the St. Petersburg Gardening Society, Prof. Beketoff made an interesting communication on the discovery in the government of Ekaterinoslav, in a wild state, of vine-plants and of the Hungarian oak (*Quercus cervis*). Both are probably degraded plants, affording remarkable specimens of natural transformism.

AMONG the numerous bibliographical indexes which have lately appeared in Russia, we notice the "Bibliography of works in Finance, Industry, and Trade in Russia, from 1714 to 1870," by M. Karataeff, which contains a complete systematic list of more than 6,000 books, papers, and newspaper notices on these subjects. The work has just appeared at St. Petersburg.

WE notice in the last number of the *Journal* of the Russian Chemical and Physical Society, the sixth part of the memoir by Prof. Menshutkin, on the influence of isomerism of acids on the formation of compound ethers. As seen from numerous measurements published by the author, the isomerism of acids is of great influence on the absolute and relative rate of etherisation, the primary acids being etherised in from 72 to 120 hours, whilst no less than 336 hours are necessary for the complete etherisation of several tertiary acids. Besides the rate of etherisation decreases also with the increase of the molecular weight. The same journal contains a paper by MM. Beilstein, and Courbatoff on chloranilines and chlornitranilines, and the minutes of the meetings of the Society.

THE new French cable for America has been placed at the disposal of the public for correspondence. It goes direct from Brest to St. Pierre, and from St. Pierre to Massachusetts, where it is connected with the American Telegraphic Union. A new cable will be laid from Brest to Penzance by the *Faraday* steamer, in the beginning of February, and afterwards from Penzance to St. Pierre. This second cable will be used for English telegrams.

IT is stated that a valuable bed of anthracite has been prospected at Ching-mên-chow, near Ichang on the Upper Yangtsze-kiang, and that it is already being worked. The coal district is said to extend for seventy-five square miles, and to contain ten beds of coal, one of which, at Wo-tsze-kow, is estimated to contain 1,200,000 tons, and lying only 100 feet below the surface.

THE Cracow newspaper *Wiek* states that the Cracow Academy proposes to convoke a general congress of historians.

THE Forty-sixth Annual Report of the York School Natural History Society is on the whole favourable; good work has been done in the geological section especially.

THE annual meeting of the Yorkshire Naturalists' Union was held at Huddersfield on Saturday week, Dr. H. C. Sorby, the president, occupying the chair. There are now twenty-six societies in the Union; Prof. Williamson, of Manchester, was chosen as Dr. Sorby's successor in the presidency. The latter gave his annual address in the evening on "The Structure and Origin of Limestones."

WE have received a report of a very successful scientific exhibition which has been opened for a few days by the enterprising Dundee Naturalists' Society. We notice from the programme of the Society, that besides lectures by eminent men of science, a number of papers of a thoroughly scientific character, will be read by members of the society during the present session.

A BANK, commonly called Hafner, in the Lake of Zurich, and situated at a distance of a few thousand feet from the Mansion House Promenade, is now being minutely investigated by order of the town authorities. It appears that remains of a prehistoric pile dwelling are coming to light at this spot, consisting of a quantity of coarse and fine clay vessels, coals, a few bronze implements, &c. The piles upon which the old colony rested are particularly numerous.

THE additions to the Zoological Society's Gardens during the past week include a Chinese Rhesus Monkey (*Macacus lasiotus*) from Shanghai, presented by Messrs. John Morris and A. H. Brown; two Blue-eyed Cockatoos (*Cacatua opthalmica*) from the Duke of York's Island, presented by the Rev. Geo. Brown, C.M.Z.S.; two Martinican Doves (*Zenaida martinicana*) from Grenada, W.I., presented by Capt. H. King; a Kittiwake Gull (*Rissa tridactyla*), European, presented by Mr. W. H. Cope, F.Z.S.; a Common Barn Owl (*Strix flammea*), European, presented by Mr. G. D. Edwards; a Jaguar (*Felis onça*) from South America, four Common Peafowls (*Pavo cristata*) from India, two Knots (*Tringa canutus*), four Widgeon (*Mareca penelope*), a Wild Duck (*Anas boschas*), two Scaup Ducks (*Fuligula marila*), European, purchased.

OUR ASTRONOMICAL COLUMN

PERIODICAL VARIATION IN THE BRIGHTNESS OF NEBULÆ.—In 1877, in a communication to the Royal Astronomical Society, Prof. Winnecke drew attention to the nebula H. II. 278, remarking that it appeared to exhibit not only a variability in its light, but, which he considered much more remarkable and difficult of explanation, that *periodical* fluctuations of brightness seemed to take place. A short time since he briefly pointed out a second case of similar character, in the nebula H. I. 20; in the last number of the *Astronomische Nachrichten* he returns to the subject, and collecting the descriptions of the latter nebula, presents very strong evidence of the variability of its light and indications that it may prove periodical.

H. I. 20 is No. 882 *h*, and No. 2405 of the General Catalogue: its position for 1880 is in R.A. 11h. 18m. 13s., N.P.D. 77° 59'·6, or it precedes B.A.C. 3882 by 34'5s., and is 5' south of the star. A star 12m. follows at 2'·8s., 2'·1 to the north. Sir W. Herschel described it as "very bright" on March 15, 1785. Forty-five years afterwards his son found it "extremely faint," and remarked at the time: "This nebula must have changed greatly, if it ever belonged really to the 1st class." On April 4, 1831, he again found it faint. The next record of its appearance was made by Boguslawski, during his preparation of Hour XI. of the Star-charts of the Berlin Academy, when it appears

to have been bright enough to be well seen in the comparatively small telescope used in the formation of the chart (aperture 3·8 inches); this would be at the epoch 1840 ±. On March 7, 1856, Winnecke found it pretty bright with the Berlin refractor. D'Arrest, on February 19, 1863, noted a considerable diminution of brightness: "Hodie aperte non supra tertiam classem," and he adds: "Locum hæc nebula non mutat, an lucem?" On April 10, 1878, it had again brightened, Winnecke recording: "Bei hellem Mond, deutlich gesehen, gewiss I. Classe." On March 21, 1879, he considered it "wohl nicht I., aber gut II. Classe." This nebula is of the elongated class, the direction of elongation not very far from the parallel; the longest diameter about 1½′. It is evidently well deserving of continuous observation.

Prof. Julius Schmidt directed attention in 1862 to another very suspicious case in the same quarter of the heavens. The object to which he refers in his communication to the *Astronomische Nachrichten* appears to be H. IV. 4, though he does not mention the identity. Sir W. Herschel, observing on February 22, 1874, describes it as "extremely faint, small, like a star with a very faint brush s.p.; 240 shows the star." It will be remembered that Sir W. Herschel's fourth class included "stars with burs, with milky chevelure, with short rays, remarkable shapes, &c." Sir John Herschel's description on April 13, 1828, does not differ from his father's; he calls it a "star 13·14 m., with a faint, small, nebulous brush." In the General Catalogue, where it is No. 2403, it is noted "very faint, small: attached to a star 13 m." Prof. Schmidt commences his note upon the probable variability of this object by remarking that it is found upon Chart No. 6 of the Bonn Durchmusterung, and must have been seen in the zone-telescope, a Fraunhofer comet-seeker of three inches aperture and two feet focus; it is No. 2436 at p. 24 in vol. iii. of the Bonn Observations. At the date of his communication (1862, March 29) he says: "This nebula is at the limit of visibility for the Athens refractor." He determined the position of the nebula and of two small neighbouring stars by reference to Weisse No. 315, with the following results for 1855·0:—

	h.	m.	s.		°	′	″	
Nebula R.A.	11	16	22·6	Decl.	−0	18	36	Light of nucleus = 13 m.
x ...	11	16	28·1	,,	−0	21	59	12·13
y ...	11	16	42·5	,,	−0	20	34	11·12

The Bonn position reduced to the same epoch gives R.A. 11h. 16m. 28·8s., Decl. −0° 21′·8, agreeing almost precisely with Schmidt's small star *x*. There may be a suspicion, therefore, that the place of greatest condensation of the nebulosity changes, as would appear to be the case with the first variable nebula in Taurus, discovered by Mr. Hind in 1852, according to M. Otto Struve's observations at Pulkowa. These objects require, and certainly merit, very close observation with adequate instruments.

TOTAL SOLAR ECLIPSES IN THE NEXT DECADE.—The report of the observation of an intra-Mercurial planet, during the total eclipse of the sun on the 11th inst., from one of the higher mountains in California (which, however, at the time we write, has not received the confirmation that might have been expected), naturally directs attention to the similar opportunities for observation of such a body that are approaching, and we may briefly particularise the circumstances under which the total eclipses of the sun, within the next ten years, will take place. The first is the eclipse of 1882, May 17, where the central line passes over Egypt, not far from Luxor, near Teheran, and so across Asia to Shanghai; the greatest duration of totality is 1m. 48s., but at the most accessible stations will not exceed 1m. 15s.; maps exhibiting the general features of this eclipse are already published in the *Nautical Almanac* and the *American Ephemeris*. Then follows the eclipse of 1883, May 6, in which the course of the central line is wholly on the Pacific Ocean, avoiding apparently, with the exception of the Marquesas, the inhabited islands. From the Admiralty chart of this group, it seems that the total phase may be observable at Chanel Island, where it will commence about 0h. 42m. local time, continuing 2m. 52s. The eclipse of 1885, September 9, may be well observed in New Zealand, where the sun will have risen to an altitude of fifteen or sixteen degrees, the duration of totality on the central line in the longitude of Wellington being 1m. 54s. Next follows the great eclipse of 1886, August 29, a recurrence of that of 1868, August 17, which was observed in India. Unfortunately in this case we have again an ocean track for the belt of totality, except near the beginning and ending of its course; at the southern extremity of the Island of Grenada the sun will be hidden for 3m. 15s., while at an altitude of about 20°; but in about 14° 13′ west of Greenwich, and latitude 2° 58′ N., where the sun is centrally eclipsed on the meridian, totality will continue for nearly 6m. 30s., and it may be expected that efforts will be made to secure in this part of the Atlantic, at least such observations as bear upon the existence of an intra-Mercurial planet or planets; when the central line reaches the African coast the duration of total phase will have diminished to about 4m. 45s., in 12° S. latitude. The next eclipse is that of 1887, August 19, which it was supposed for a long time would be total in this country, the central line, however, does not reach England; commencing in Central Germany, or in 11° 39′ east of Greenwich, and 51° 38′ N., it passes by Berlin and Moscow, to a point in 102° 15′ E., and 53° 46′ N., where the sun will be totally eclipsed on the meridian, and thence to 173° 47′ E. and 24° 32′ N., where the central phase passes off the earth; at Berlin, where the sun will only just be clear of the eastern horizon, totality continues 1m. 41s., and in the longitude of Moscow, to the north of the city, 2m. 30s., with the sun at an altitude of 17°; on the shores of Lake Baikal, where he will be near the meridian, the duration of totality is increased to 3m. 38s. The last total eclipse of the decade to which this note applies will take place on December 22, 1889; it may be observed at Bridgetown, Barbadoes, where the sun at an altitude of about 6° will be hidden for 1m. 48s.; at a point on the Angola coast in about 10° S, totality will continue 3m. 34s., the central eclipse passes off the earth in 60° 55′ E. and 6° 53′ N.

BIOLOGICAL NOTES

BEES EATING ENTRAPPED MOTHS.—Mr. Packard, jun., writing in the January number of the *American Naturalist*, says that a flowering stalk of an asclepiad (*Physianthus* [*Aranja*] *albens*) was brought to him last September, with the bodies of several moths (*Plusia precationis*) hanging dead from the flowers, being caught by their tongues or maxillæ. "These moths had, in endeavouring to reach the pollen-pockets of the flowers, been caught as if in a vice by one of the opposing edges of the five sets of hard, horny contrivances covering the pollinia." A very short time afterwards the Rev. L. Thompson, of North Woburn, Mass., a careful observer, sent Mr. Packard the following details of the behaviour of bees (*Apis mellifica*) also frequenting the flowers of the same asclepiad:—"My attention was attracted by two or three bees buzzing immediately around as many entrapped moths that were alive and struggling to get away. Every moment or two a bee suddenly and furiously darted upon a prisoner and seemed to me to sting it, despite its desperate efforts to escape. This onset was generally instantaneous, but was repeated again and again; and after a moth became still and apparently lifeless the bee settled upon and, if my eyes did not greatly deceive me, began to devour it." Mr. Thompson previously noticed tongues of the same species of moth caught in the flowers, the bodies to which they belonged having disappeared. At the time he fancied these were probably eaten by birds, but on further examination he came to the conclusion that the bees had really feasted on animal food, as well as upon the nectar of the surrounding flowers. Specimens of these bees being captured, the species was determined by Mr. Packard. On this fact being communicated to Mr. Darwin, he wrote that he "never heard of bees being in any way carnivorous, and the fact is to me incredible. Is it possible that the bees opened the bodies of the Plusia to suck the nectar contained in their stomachs? Such a degree of reason would require confirmation, and would be very wonderful." Hermann Müller wrote "that his brother Fritz in South Brazil has observed that honey-bees (species doubtful) licked eagerly the juice dropping from pieces of meat which had been suspended in the open air to dry; but he thinks nothing has been published on the carnivorous habits of bees." The well-known apiarian, Prof. A. J. Cook, however, reminds Mr. Packard "that honey-bee workers within the hive, on killing off the drones, tear them in pieces with their mandibles rather than sting them, and that he has seen them thus kill a humble-bee that had entered the hive." Huber, if we mistake not, also tells us that under certain circumstances the common hive-bee will devour the eggs laid by the queen bee.

NEW MOSASAUROID REPTILES.—The Mosasauroid Reptiles are so rare in Europe that the famous type specimen described

by Cuvier still remains the most perfect yet discovered there. This was the specimen said to have been given up to the French army on the capture of Maestricht, and which is now in the Paris Museum. So much was thought about it that the story goes that the French gunners had orders not to point their artillery to that portion of the town where it was known to be. In America Prof. O. C. Marsh tells us, the group attained a marvellous development, and was represented by very many genera and species belonging to even diverse families. In a paper in the current number (January) of the *American Journal of Science* he gives some new characters of the group, based on the examination of an enormous collection in the museum of Yale College, which is calculated to contain the remains of not less than 1,400 distinct individuals. In not a few of these the skeleton is nearly if not quite complete, so that every part of its structure can be determined with almost absolute certainty. Already from this immense storehouse has Prof. Marsh made out various important details of the anatomy of the group. In the present paper he communicates several others which had escaped other observers. Several specimens, one of which is figured, prove the presence of a sternum which is of the true lacertilian type. The entire pectoral arch and paddles in several genera are described; the general structure of the paddles is Cetacean in type; hyoid bones have been found. In some genera the orbit was protected by a ring of osseous plates, composed of but a single row of plates overlapping; the transverse bone of Cuvier (ecteopterygoid, Owen) is present in several of the genera. The accuracy of Cuvier's determination of the pterygoid bones can no longer be called in question; Cope errs in calling them palatines. All these newly-discovered characters and facts indicate a true lacertilian alliance, and a new sub-order of lizards should be formed, to be called Mosasauria.

NEW ENGLAND ISOPODS.—In the *Proceedings* of the United States National Museum (November 5, 1879) Oscar Harger briefly describes the marine isopods collected by the United States Commission of Fish and Fisheries. Fuller descriptions with figures of most of the species are promised later. As new species are described *Janira spinosa*, from Banquereau, and *Leptochelia rapax*, from Aunisquam. There are forty-three species enumerated, of which eleven are to be found on the coasts of Europe.

THE FOSSIL HORSES OF CONSTANTINE.—Veterinary Surgeon P. H. Thomas has quite recently published an interesting account of the remains of some fossil horses found in the neighbourhood of Constantine, in Algeria. It will be remembered that the environs of Constantine are traversed by large and deep valleys, on the flanks of which, as far as an elevation seldom exceeding 600 metres, the stripes of a fluvial-lacustrine pliocene formation lie stratified. These, at their base, are characterised by the presence of a chalky marl, and towards their summits by gritty conglomerates, pudding-stones, and sand; the fluvial lacustrine deposits contain a somewhat transition fauna-composed of some of the larger vertebrates, amongst which two species of horse have been found, one an Hipparion and one very near to, if not identical with, the *Equus stenonis* (Gaudry), of the pliocene of Europe. In the bottom of these valleys, at the base of the steep banks of the larger rivers, turfy deposits are found, appertaining in all probability to a recent quaternary period in which a fauna appears—which, though showing some affinities to the previously-mentioned fauna, is more clearly connected with that actually existing. Here are to be found remains of a horse (*Equus caballus*) differing by only a few secondary characters from the actually living African horse; an ass of small dimensions, presenting in its dentition some characters calling to mind the genus Hipparion, which genus had, however, disappeared since the preceding geological period. In the grey marl which immediately lie over the alluvial turf, and which appear to be very recent, there will be found in the lowest strata the remains of horses, horned cattle, and molluscs, differing in no way from those of the present day. In a middle stratum remains of flint weapons have been found (at about 2·50 m. from the surface of the soil), while at about 1 metre below this surface, vestiges of the Roman occupation will be met with.

PHYSICAL NOTES

MEASUREMENTS of the movements of glaciers have hitherto been directed either to approximate determination of the yearly or daily mean velocity, or to showing that the motion of glaciers resembles that of liquids. Some new measurements by Herr Koch and Fr. Klocke (*Wied. Ann.*, No. 12) have been limited to ascertaining the motion of a point of the surface in a vertical plane parallel to the direction of length of the glacier, with a view to finding the real nature of the glacier's progress, whether continuous and in the same direction or not. Two scales were placed, one vertical, the other horizontal, being attached to a post, fixed half a metre deep in the ice, and having a cone of ice and *débris* formed round it. This was on the west side of the Morteratsch glacier, about 1½ km. from its principal extremity. The observations were made in August and September, the scales being watched by day only, through a fixed telescope with cross-wires. The number of scale parts passing the cross gave the direct and horizontal components of the motion. Another similar post with scales was set up near, and in the field of vision. The observations proved that the motion of the glacier is by no means uniform, for one and the same point may move now upwards, now downwards, towards the mountain, or towards the valley. Further, two points of the surface, about 50 to 60 metres separate from each other, may, at the same time, move in different, and even in opposite directions.

THE behaviour of membranes in sounding columns of air has been recently investigated by Herr Kohlrausch (*Wied. Ann.*, No. 12), and with the following results (which sufficiently indicate the line of research):—1. Open membranes (freely in contact with the air on both sides) vibrate in the ventral segments of stationary waves, and come to rest in the nodes; covered membranes (shut off from the external air on one side) vibrate in the nodes and come to rest in the ventral segments. 2. A fine open membrane stretched over a ring is a *very sensitive* means of determining the position of the nodes in stationary waves. 3. If a solid body be brought between two nodes of the stationary vibrations of a pipe, the half-wave between these two nodes contracts, while the others are lengthened, and the pipe gives a tone corresponding to the longer half-waves, consequently a deeper one.

FROM a comparison of the temperature co-efficients of fluidity and galvanic conductivity for a number of substances (*Wied. Ann.*, No. 12), Herr Grotrian finds that with increasing concentration of a solution, both coefficients vary in the same sense. In solutions of NH_4Cl, KCl, KBr, and KI, the galvanic conductivity increases nearly in proportion to the percentage proportion. The fluidity, on the other hand, varies but little with the concentration.

A SLIGHT improvement has been introduced into the Bunsen grease-spot photometer by Herr Toepler (*Wied. Ann.*, No. 12), rendering the observations much less dependent on the position of the observer (the angle between his line of sight and the paper screen). The grease spot is done away with, and the thickness of paper is reduced instead, to give a spot. Between two very thin moderately transparent sheets of parchment paper, having a small circular aperture, is placed a sheet of ordinary strong paper.

DR. BAUMGARTNER has recently made, in Prof. Pfaundler's laboratory (*Wied. Ann.*, No. 12), a series of determinations of the specific heat of water by a method of mixtures, in which *boiling* water was poured directly into the cold water of the calorimeter. The specific heat at 100° (that at 0° = 1) was found 1·0307 (as against 1·0130 by Regnault; 1·0220 Regnault, according to Bosscha's calculations, 1·0302 v. Münchhausen and Wüllner, 1·0720 Heinrichsen, 1·1220 Jamin and Amaury, 1·1255 Marie Stamo).

THE telephone has been found by Herr Niemöller (*Wied. Ann.*) capable of determining very quickly and accurately the resistance of liquids. It is substituted for the galvanometer in a galvanic bridge, and an induction current is used; then, if the resistances compared are a large liquid resistance on the one hand, and a Siemens's resistance-box on the other, so that the electro-dynamic constants of the branches are very small; if, further, a German-silver or platinum wire be used as measuring wire, it is found that in the position where the galvanometer shows no deflection, the tone in the telephone has a well-marked minimum of intensity. Supposing the liquid resistance has 2,000 units, a variation of it, even four units, reveals itself in a displacement of the minimum position.

FOR study of liquid waves Signor Bazzi lately used (N. Cim. (3) 6, p. 98) a trough 6 m. long, 10 ctm. deep, and 5 ctm. wide. In one end of it dipped a wooden parallelepiped, which could

be moved up or down in guides, and served to produce waves. A movable apparatus indicated on a cylinder the movements of the surface at any point; the moment of immersion was also indicated. The following results were arrived at:—1. If the body be drawn out and a wave of depression produced, a whole series of other waves follows this, which are of gradually decreasing height. 2. Both the primary and the secondary waves are, from a certain distance from the origin onwards, propagated with uniform velocity, which, for the same depth, is independent of the mode of the immersion. The first primary wave has the greatest velocity; it coincides with that resulting from Lagrange's calculations. The velocity of the others decreases from wave to wave, so that their length increases proportionally to the distance from the origin. 3. The depth of the first wave is proportional to the volume brought out of the position of equilibrium; and it decreases inversely as the square root of the distance from the origin (this corresponds to Boussinesq's development). 4. The profile of each secondary wave is a sinusoid, but that of the primary is much more complicated. These results are in contradiction to nearly all analytical results on wave motion. The author is prosecuting his inquiry further.

In an interesting memoir presented to the Belgian Academy, on the influence of the form of masses on their attraction, M. Lagrange arrives at the following theorem, which he considers as fundamental for the mechanical theory of crystallisation: A mass of any form, at a distance from its centre of inertia, acts with maximum, mean, and minimum energies in three rectangular directions, and these directions coincide respectively with the three axes of maximum, mean, and minimum inertia of the mass; the attraction diminishing the more rapidly the less the mass in question. M. Lagrange offers some preliminary considerations on the structure of bodies, and one curious consequence of his formulæ is that the molecules of a body are not always distributed symmetrically with regard to the three rectangular directions, owing to the influence of certain secondary axes of attraction, which is combined with that of the principal axes of inertia. The principal modes of crystallisation of bodies seem to M. van der Mensbrugghe (who reports on the memoir), in perfect harmony with the classification of molecular groups, (1) according to their principal axes of inertia, (2) according to their secondary axes of attraction. M. Lagrange promises, in an early work, a complete solution of the problems of crystallisation of bodies.

M. Thollon has recently observed, by the aid of his spectroscope of high dispersive power, a solar protuberance whose height equalled one-sixteenth of the diameter of the sun, or about 55,000 miles.

Herr Edelmann describes, in Carl's *Repertorium*, a novel quadrant electrometer in which the needle, instead of being a flat plate, consists of two quadrants cut vertically from a cylinder. This swings concentrically within another cylinder slit into four quadrants, which replace the usual pairs of flat quadrantal plates. The needle and its attached mirror are supported by a bifilar suspension, and the charge is given to the needle by connecting the cup of concentrated sulphuric acid, into which it dips, with the pole of a Zamboni pile. This latter arrangement is simpler than the usual replenisher and gauge of the well-known Thomson electrometers, but cannot be anything like as reliable.

Herr Böttger describes a process for steeling copper plates by electrolysis. 100 parts of ferrous-ammonia sulphate, together with 50 parts of sal-ammoniac, are dissolved in 500 parts of pure water, a few drops of sulphuric acid being added to acidulate the solution. The copper plate connected to the negative pole of a battery of two or three Bunsen elements, an iron plate of equal size being employed as an anode. The solution is maintained at from 60° to 80°. The deposit of iron is of a hard steel-like quality, and is very rapidly formed.

Prof. Graham Bell communicated a notice of "Some Experiments relating to Binaural Audition" to the recent meeting of the American Association for the Advancement of Science. The paper, which contains some extremely valuable observations, will be published *in extenso* in the *American Journal of Otology*.

GEOGRAPHICAL NOTES

A Reuter's telegram from Halifax, Nova Scotia, states that arrangements are in progress there for a new American Arctic Exploring Expedition, under the leadership of Dr. Emil Bessels, the scientific member of Capt. Hall's *Polaris* Expedition.

In opening the proceedings of the Geographical Society on Monday evening, Lord Houghton read a letter from Sir Bartle Frere, in which he spoke in the highest terms of Dr. Emil Holub as the most competent traveller he had met for a long time, and in which he also expressed the opinion that, with the exception of a very small portion, the Valley of the Zambesi was well suited for Europeans in regard to climatic conditions. After an amusing sketch of his early experiences in South Africa, and a brief account of his two preparatory journeys, Dr. Holub delivered an address, describing vividly and in considerable detail his main journey, which occupied twenty-one months, from the Diamond Fields to the upper waters of the Zambesi. Among other matters, he thus explained how the River Zooga flows at one time to the east and at another to the west. When the Shallow Lake Ngami is filled up by the streams falling into it from the west, its waters pass through the Zooga to the salt lakes on the east, but when these streams do not pour in such an amount of water, the level of the lake becomes very low, and the Zooga, often largely increased in volume from the overflowing salt lakes, sends its waters into Lake Ngami. This solution of a curious phenomenon agrees, we believe, with the conclusion arrived at by Major Serpa Pinto. Dr. Holub dwelt for some time on the Marutse Empire, which he considered to be some 400 miles long and 450 broad, and the languages and customs of which he had ample opportunities for studying from his prolonged stay at Shesheke. When examining the country to the north of this place, Dr. Holub was unfortunately prostrated by severe illness, which compelled him to give up all further explorations in this interesting region. He made his return journey through the western Makalaka region of the Matabele country, about which he gave many particulars. Dr. Holub exhibited a very carefully drawn chart which he had made of part of the course of the Zambesi, and gave some information respecting his various collections. These include ethnographical objects, a large number of skins of birds and animals, fishes, insects, reptiles, &c., besides numerous botanical specimens. Dr. Holub hopes that before long he may have an opportunity of exhibiting his collections in London.

We have received the first number of the new *Zeitschrift für wissenschaftliche Geographie*, edited by Herr J. I. Kettler, of Lahr, in Baden, assisted by an imposing staff of German geographers. We expected great things from this new journal, judging from the prospectus to which we referred some weeks ago; but we confess this first number disappoints us. Fifteen pages are devoted to a discussion of the first landing-point of Columbus, by Dr. R. Pietschmann, surely a great waste of space in a journal that professes to devote itself to scientific geography. The editor takes up seven pages with an article on the position of Brunswick; the old story of Severstoff's Ferghona expedition is related, and Dr. O. Krummel reproduces his discussion of the mean depths of the ocean, which has gone the round of the journals long ago. Behm's *Jahrbuch* for 1879, now out of date almost, is reviewed, and some old letters of Humboldt's are given, interesting only on the writer's account. An elaborate series of small charts are the only maps given, illustrating the paper on Columbus's landing-point. We trust the succeeding numbers will be both more scientific and more novel, else the new journal can scarcely justify its existence.

Last week the French expedition commissioned to explore the Sahara in connection with the proposed railway left Paris for Marseilles, whence it will sail for Algeria. The expedition will devote its attention mainly to the country south of Wargla, which is too imperfectly known at present to enable a decision to be come to as to the precise route which the railway ought to take. The expedition is under the command of Lieut.-Col. Flatters, who is accompanied by an efficient scientific staff of engineers and others. They will be accompanied by an escort of trustworthy frontier Arabs. At the last meeting of the Paris Society of Commercial Geography, M. Masqueray, the Saharan explorer, gave some interesting information concerning the land of Adrar, in the Western Sahara. This he derived from three pilgrims on their way to Mecca, who had been plundered in the desert, and supplied with funds by the French Government in Algiers to continue their pilgrimage. On their return they have promised to conduct the French explorer to their country. Adrar, or Aderer, presents two or three of the chief aspects of the Sahara, which is by no means the universal desert at one time

supposed. In the south-west are long bands of sand, not exceeding eight days' march in width. Adrar-Temar, the country of the travellers, is placed like a long and narrow island between two of these bands of sand. It is an almost level region, slightly elevated above the sands, which tend to encroach upon its borders. Intermittent streams are found in the country, and there are numerous towns or large villages, containing a considerable population. The three pilgrims represent their country as covered with gum-acacias, and ostriches greatly abound. The most important commercial fact in connection with Adrar is the existence at Ijil of an immense deposit of rock salt, which, as we advance towards the country of the negroes, becomes the most valuable article of trade. Tichu (? Tishit), some days' journey to the south-east of Ijil, is the principal market for the trade in salt, for which slaves are the principal exchange.

HERR CLEMENS DENHARDT, who has just returned to Germany from an exploring tour in Eastern Central Africa, has received a grant of 500 marks (20*l.*) from the Gesellschaft für Erdkunde, at Berlin, to defray the cost of publishing his notes of travel.

M. GRANDIDIER, the explorer of Madagascar, has been appointed president of the governing body (Section Centrale) of the Paris Geographical Society for 1879. Admiral La Roncière Le Nourry has been continued president of the Society. The Geographical Society of Paris is preparing to hold a reception when Prof. Nordenskjöld arrives in France; but the first step will be taken by the Society of Marseilles, the city at which Nordenskjöld will land from Naples, according to all probability.

WE learn from the last number of the *Isvestia* of the Russian Geographical Society that the expedition of M. Pyevtsoff to Mongolia was very successful. M. Pyevtsoff, after having stayed seven days at Koukou-khoto, started for Kalgan (in the south-east part of the Gobi steppe) where he remained for two months, studying the trade of China with Mongolia. Thence the expedition went to Urga, and from Urga to Ulassoutai, following thus a route which never was before explored. From Ulassoutai M. Pyevtsoff turned west to the Chuyra river, which was reached at Kosh-agach; this route was quite unknown until now. On the whole thousand miles' distance between Urga and Kosh-agach the expedition made a survey, and M. Pyevtsoff determined the latitudes and longitudes of twelve points. On the whole the expedition has made, on its way from Khobdo to Kalgan and thence to Kosh-agach, no less than 2,700 miles of surveys, and determined astronomically the position of twenty-six points, all longitudes being determined as well by chronometers as by the occultations of stars. Barometrical measurements were made during the whole journey, and very rich zoological, botanical, and mineralogical collections were obtained.

THE St. Petersburg Geographical Society has received news from Col. Prjvalsky, *viâ* Pekin. The intrepid traveller has safely arrived at Zaidam, on the Tibetian frontier, after having crossed the hitherto unknown country from Hami *viâ* Shatsheu to Zaidam. From the latter place he will proceed to the interior of Tibet. News has also been received from the chief of the so-called Samara Expedition, referring to the readiness of the Chiwinz tribe to restore the old course of the Amu Darya by destroying the dykes on the lower part of the river. The expedition sent out by the Russian Government Office for Communications, under Col. Gluchowski, and charged with the investigation of the lower course of the Amu Darya, with a view to rendering it navigable in future, also begins to show signs of activity.

THE "Karl Stangen'sche Reisebureau," at Berlin, will publish a description of its first journey round the world (1878-79) early in March, this description to serve as a guide for future journeys and intending tourists.

THE EFFECTS OF UNINTERRUPTED SUNLIGHT ON PLANTS

PROF. SCHÜBELER of Christiania, who for nearly thirty years has been engaged in observing the influence exerted by differences of climate on vegetation, has published the result of his observations in recent numbers of our Norwegian namesake, *Naturen*. The first of the series of his observations, which he has given in detail, refer to winter-wheat, and were undertaken with the special view of noting what effect the almost unbroken sunlight of the short Scandinavian summers had on plants raised from foreign seed. The experiments were made with samples of grain from Bessarabia and Ohio, and in both cases it was found that the original colour of the grain gradually acquired each year a richer and darker colour—the difference being perceptible even in the first year's crop—until it finally assumed the yellow-brown tint of other home-grown Norwegian winter-wheats. Similar results were obtained with maize, different kinds of garden and field peas and beans, and certain other garden plants, as celery, parsley, &c. In no case has Dr. Schübeler found that an imported plant, capable of being cultivated in Norway, loses in intensity of colour after continued cultivation; while in regard to many of the common garden flowers of Central Europe, he believes it may be asserted with certainty, that after their acclimatisation in Norway, they acquire an increase of size, as well as an augmentation of colour. These altered conditions are more forcibly manifested the further north we go, within the limits of capacity of vegetation for different plants. Thus it has been observed by Prof. Wahlberg of Stockholm, that *Epilobium angustifolium*, *Lychnis sylvestris*, *Geranium sylvaticum*, and many other plants common to Lapmark and the more southern districts of Sweden, attain in the former a size and brilliancy of tint unknown in the latter. The change in the case of *Veronica serpyllifolia* and *Trientalis europæa* is remarkable; the former changing as it goes further north from a pale to a dark blue, and the latter from white to rose-pink. It is noteworthy that a tinge of red is a common characteristic of the vegetation of the Scandinavian Fjælds; this being observable alike in blue, yellow, green, and white colours.

Colour is not, however, the only property affected by the unbroken continuance of daylight in the summers of Scandinavia, for according to Dr. Schübeler, the aroma of all wild and cultivated fruits, capable of cultivation in the northern lands, is much greater than that of the same fruits when grown in more southern countries. This is especially observable in regard to strawberries, cherries, and the various kinds of wild marsh and wood berries. In corroboration of this, Prof. Flückiger of Strassburg has found that the Norwegian juniper yields a much larger amount of essential oil than can be obtained from the shrub when grown in Central Europe. This excess of aroma in northern plants and fruits co-exists with an inferior degree of sweetness; thus the common golden-drop plum, and the green-gage of Christiania, or Throndhjem, although large, well-coloured, and rich in aroma, are so deficient in sweetness as to seem unripe to those who have eaten these fruits in France, or Southern Germany.

Dr. Edmond Göze, who has long been resident at Coimbra, informs Dr. Schübeler, that his observations on the fruits of Portugal enable him to corroborate that observer's opinion in regard to the different conditions on which aroma and sweetness respectively depend. The strawberries grown in large numbers near Coimbra are, he says, of great size, extremely sweet, but almost wholly deficient in aroma and flavour. The same remark refers to the Portuguese wines, when compared with the highly flavoured yields of the Rhenish and other northern vineyards; and a consideration of these varying conditions leads him to accept as an established fact, that light bears the same relation to aroma, as heat does to sweetness.

This increase of aroma, or intensification of flavour, due to the uninterrupted action of the sun's light, has the effect of making some of our most savoury garden plants almost uneatable in Scandinavia. Thus Dr. Schübeler has found that common white stick-celery, which had been grown near Christiania with careful attention to the methods followed in England, and which in outward appearance could not be distinguished from plants brought direct from Covent Garden Market, had a sharp unpleasant taste, when compared with the milder and more agreeably flavoured English plants. The same result was observed in garlick, shalots, and onions, and although it must be admitted that as the expressions of mere individual taste, the writer's conclusions in regard to this point are open to doubt, it should at the same time be borne in mind that they are based on practical observations and experiments, continued for nearly thirty years, and confirmed by the concurrent testimony of several of his colleagues, who, like himself, were desirous of deducing practical results from the acclimatisation of plants in Norway. From this point of view, some of Dr. Schübeler's observations are especially interesting, and in the present low condition of Norwegian industrial development, their practical

application would be highly important. Thus, he shows that while linseed oil is obtained in Holland, Germany and Middle Russia in the proportion of about 3 or 4 p. c. of the weight of the plants from which it is extracted, the yield from uncultivated plants in Norway varies from 4 to 5, or 5 to 8 per cent. Again his experiments of the yield of the essential oil of lavender, have convinced him, that plants grown in Christiania or Throndhjem, when compared to those grown near Merton, which have hitherto been regarded as the first in the world, greatly excel the latter in aroma, and he considers that the cultivation of this plant could be carried on with undoubted success on the coast-lands of Norway.

While Dr. Schübeler has no hesitation in maintaining that light engenders aroma, as heat engenders sweetness, he has not been able to determine to what extent the vegetable alkaloids are affected by either. In connection with his own observations, he reports some curious particulars in regard to the action of continuous light in the polar regions, which he has obtained from intelligent residents, who had undertaken to conduct certain experiments under his direction. Thus it was found both at Alten in West Finmark, and at Stamsund in the Lafodens, that plants of *Acacia lophantha* never contracted their leaves during the two months, or longer, that the sun remained above the horizon. An experiment was made at Alten to shade one half of the crown of an acacia during the night, and the result was that in about twenty minutes' time, the protected leaves began to contract, and remained closed until the plant was again wholly exposed to the midnight-sun, when after a time the leaves began slowly to unfold. At Stamsund it was observed that whenever the acacias were placed on the north side of a house, which was partially screened by a neighbouring Fjæld, the leaflets turned upwards, without however wholly closing, and the same thing was noticed in rainy weather. The leaves of *Mimosa pudica* contracted in the lightest and clearest nights, and remained folded back for some hours.

Without entering further into the details of Dr. Schübeler's numerous experiments, we may summarise their results as follows:—

1. The grain of wheat, that has been grown in low lying lands, may be propagated with success on the high Fjælds, and will reach maturity earlier at such elevations, even although at a lower mean temperature. Such grain, after having been raised for several years at the highest elevation which admits of its cultivation, is found when transferred to its original locality to ripen earlier than the other crops which had not been moved. The same result is noticeable in grain that has been transported from a southern to a more northern locality, and *vice versâ*.

2. Seeds imported from a southern locality, when sown within the limits compatible with their cultivation, increase in size and weight, and these same seeds, when removed from a more northern locality to their original southern home, gradually diminish to their former dimensions. A similar change is observable in the leaves and blossoms of various kinds of trees and other plants. Further, it is found that plants raised from seed, ripened in a northern locality, are hardier, as well as larger than those grown in the south, and are better able to resist excessive cold.

3. The further north we go—within certain fixed limits—the more energetic is the development of the pigment in flowers, leaves and seeds. Similarly, the aroma, or flavour of various plants or fruits, is augmented in intensity the further north they are carried within the limits of their capacity for cultivation, conversely, the quantity of saccharine matter diminishes in proportion as the plant is carried further northward.

MYTHOLOGIC PHILOSOPHY[1]

1. *THE GENESIS OF PHILOSOPHY.*—The wonders of the course of nature have ever challenged attention. In savagery, in barbarism, and in civilisation alike, the mind of man has sought the explanation of things. The movements of the heavenly bodies, the change of seasons, the succession of night and day, the powers of the air, majestic mountains, ever-flowing rivers, perennial springs, the flight of birds, the gliding of serpents, the growth of trees, the blooming of flowers, the forms of storm-carved rocks, the mysteries of life and death, the institutions of society—many are the things to be explained.

The yearning to know is universal. *How* and *why* are ever-lasting interrogatories profoundly instinct in humanity. In the evolution of the human mind the instinct of cosmic interrogation follows hard upon the instinct of self-preservation.

In all the operations of nature man's weal and woe is involved. A cold wave sweeps from the north, rivers and lakes are frozen, forests are buried under snows, and the fierce winds almost congeal the life fluids of man himself, and man's sources of supply under the rocks of water. At another time the heavens are as brass, and the clouds come and go with mockery of unfulfilled promises of rain, the fierce midsummer sun pours its beams upon the sands, and scorching blasts heated in the furnace of the desert sear the vegetation, and the fruits, which in more congenial seasons are subsistence and luxury, shrivel before the eyes of famishing men. A river rages and destroys the adjacent valley with its flood. A mountain bursts forth with its rivers of hell, the land is buried, and the people are swept away. Lightning shivers a tree and rends a skull.

The silent, unseen powers of nature, too, are at work bringing pain or joy, health or sickness, life or death to mankind. In like manner, man's welfare is involved in all the institutions of society.

How and *why* are the questions asked about all these things—questions springing from the deepest instinct of self-preservation.

In all stages of savage, barbaric, and civilised inquiry, every question has found an answer, every *how* has had its *thus*, every *why* its *because*. The sum of the answers to the questions raised by any people constitute its philosophy; hence all people have had philosophies consisting of their accepted explanation of things. Such a philosophy must necessarily result from the primary instincts developed in man in the early progress of his differentiation from the beast. This I postulate; if demonstration is necessary, demonstration is at hand.

Not only has every people a philosophy, but every stage of culture is characterised by its stage of philosophy. Philosophy has been unfolded with the evolution of the human understanding. The history of philosophy is the history of human opinions from the earlier to the later days—from the lower to the higher culture. In the production of a philosophy phenomena must be discerned, phenomena must be discriminated, phenomena must be classified. Discernment, discrimination, and classification are the processes by which a philosophy is developed. In studying the philosphy of a people at any stage of culture, to understand what such a people entertain as the sum of their knowledge, it is necessary that we should understand what phenomena they saw, heard, felt—discerned; what discriminations they made, and what resemblances they seized upon as a basis for the classification on which their explanations rested. A philosophy will be higher in the scale, nearer the truth, as the discernment is wider, the discriminations nicer, and the classification better.

The sense of the savage is dull compared with the sense of the civilised man. There is a myth current in civilisation to the effect that the barbarian has highly-developed perceptive faculties. It has no more foundation than the myth of the wisdom of the owl. A savage sees but few sights, hears but few sounds, tastes but few flavours, smells but few odours, his whole sensuous life is narrow and blunt, and his facts, that are made up of the combination of sensuous impressions, are few.

In comparison the civilised man has his vision extended away toward the infinitesimal and away toward the infinite; his perception of sound is multiplied to the comprehension of rapturous symphonies; his perception of taste is increased to the enjoyment of delicious viands; his perception of smell is developed to the appreciation of most exquisite perfumes; and the facts that are made up of his combination of sensuous impressions are multiplied beyond enumeration. The stages of discernment, from the lowest savage to the highest civilised man, constitute a series, the end of which is far from the beginning.

If the discernment of the savage is little, his discrimination is less. All his sensuous perceptions are confused, but the confusion of confusion is that universal habit of savagery—the confusion of the objective with the subjective, so that the savage sees, hears, tastes, smells, feels the imaginings of his own mind. Subjectively determined sensuous processes are diseases in civilisation, but normal functional methods in savagery.

The savage philosopher classifies by obvious resemblances—analogic characters. The civilised philosopher classifies by essential affinities—homologic characteristics; and the progress of philosophy is marked by changes from analogic categories to homologic categories.

[1] From Vice-Presidential Address of Prof. J. W. Powell, of Washington, Vice-President Section B, American Association for the Advancement of Science, Saratoga Meeting, August, 1879.

2. *Two Grand Stages of Philosophy.*—There are two grand stages of philosophy—the mythologic and the scientific. In the first, all phenomena are explained by analogies derived from subjective human experiences; in the latter, phenomena are explained as orderly successions of events.

In sublime egotism man first interprets the cosmos as an extension of himself; he classifies the phenomena of the outer world by their analogies with subjective phenomena; his measure of distance is his own pace, his measure of time his own sleep, for he says, "It is a thousand paces to the great rock," or, "It is a thousand sleeps to the great feast." Noises are voices, powers are hands, movements are made afoot. By subjective examination discovering in himself will and design, and by inductive reason discovering will and design in his fellow-men and in animals, he extends the induction to all the cosmos, and there discovers in all things will and design. All phenomena are supposed to be the acts of some one, and that some one having will and purpose.

In mythologic philosophy, the phenomena of the outer physical world are supposed to be the acts of living, willing, designing personages. The simple are compared with and explained by the complex. In scientific philosophy, phenomena are supposed to be children of antecedent phenomena, and so far as science goes with its explanation, they are thus interpreted. Man with the subjective phenomena gathered about him is studied from an objective point of view, and the phenomena of subjective life are relegated to the categories established in the classification of the phenomena of the outer world; thus the complex is studied by resolving it into its simple constituents. Some examples of the philosophic methods belonging to widely separated grades of culture may serve to make my statements clearer.

Wind.—The Ute philosopher discerns that men and animals breathe. He recognises vaguely the phenomena of the wind and discovers its resemblance to breath, and explains the winds by relegating them to the class of breathings.

He declares that there is a monster beast in the north that breathes the winter winds, and another in the south, and another in the east, and another in the west. The facts relating to winds are but partially discerned; the philosopher has not yet discovered that there is an earth surrounding atmosphere. He fails also in making the proper discriminations.

His relegation of the winds to the class of breathings is analogic, but not homologic. The basis of his philosophy is personality, and hence he has four wind gods.

The philosopher of the ancient Northland discovered that he could cool his brow with a fan, or kindle a flame, or sweep away the dust with the wafted air. The winds also cooled his brow, the winds also swept away the dust, and kindled the fire into a great conflagration, and when the wind blew he said, "Somebody is fanning the waters of the fiord," or "Somebody is fanning the evergreen forests," and he relegated the winds to the class of fannings, and he said, "The god Hraesvelger, clothed with eagle plumes, is spreading his wings for flight, and the winds rise from under them."

The early Greek philosopher discovered that air may be imprisoned in vessels or move in the ventilation of caves, and he recognised wind as something more than breath, something more than fanning, something that can be gathered up and scattered abroad, and so when the winds blew he said, "The sacks have been untied," or, "The caves have been opened."

The philosopher of civilisation has discovered that breath, the fan wafted breeze, the air confined in vessels, the air moving in ventilation, that these are all parts of the great body of air which surrounds the earth, all in motion, swung by the revolving earth, heated at the tropics, cooled at the poles, and thus turned into counter currents and again deflected by a thousand geographic features, so that the winds sweep across valleys, eddy among mountain crags, or waft the spray from the crested billows of the sea, all in obedience to cosmic laws.

The facts discerned are many, the discriminations made are nice, and the classifications based on true homologies, and we have the science of meteorology, which exhibits an orderly succession of events even in the fickle winds.

Sun and Moon.—The Ute philosopher declares the sun to be a living personage, and explains his passage across the heavens along an appointed way by giving an account of a fierce personal conflict between Ta-vi, the sun-god, and Ta-wats, one of the supreme gods of his mythology.

In that long ago, the time to which all mythology refers, the sun roamed the earth at will. When he came too near with his fierce heat the people were scorched, and when he hid away in his cave for a long time, too idle to come forth, the night was long and the earth cold. Once upon a time Ta-wats, the hare-god, was sitting with his family by the camp fire in the solemn woods anxiously waiting for the return of Ta vi, the wayward sun-god. Wearied with long watching the hare-god fell asleep, and the sun-god came so near that he scorched the naked shoulder of Ta-wats. Foreseeing the vengeance which would be thus provoked, he fled back to his cave beneath the earth. Ta-wats awoke in great anger, and speedily determined to go and fight the sun-god.

After a long journey of many adventures the hare-god came to the brink of the earth, and there watched long and patiently, till at last the sun-god coming out, he shot an arrow at his face, but the fierce heat consumed the arrow ere it had finished its intended course; then another arrow was sped, but that also was consumed, and another, and still another, till only one remained in his quiver, but this was the magical arrow that had never failed its mark. Ta-wats, holding it in his hand, lifted the barb to his eye, and baptised it in a divine tear; then the arrow was sped and struck the sun-god full in the face, and the sun was shivered into a thousand fragments, which fell to the earth, causing a general conflagration.

Then Ta-wats, the hare-god, fled before the destruction he had wrought, and as he fled, the burning earth consumed his feet, consumed his legs, consumed his body, consumed his hands and his arms; all were consumed but the head alone, which bowled across valleys and over mountains, fleeing destruction from the burning earth, until at last, swollen with heat, the eyes of the god burst and the tears gushed forth in a flood, which spread over the earth and extinguished the fire.

The sun-god was now conquered, and he appeared before a council of the gods to await sentence. In that long council was established the days and the nights, the seasons and the years, with the length thereof, and the sun was condemned to travel across the firmament by the same trail day after day till the end of time.

In the same philosophy we learn that in that ancient time a council of the gods was held to consider the propriety of making a moon, and at last the task was given to Whip-poor-will, a god of the night, and a frog yielded himself a willing sacrifice for this purpose, and the Whip-poor-will, by incantations and other magical means, transformed the frog into the new moon.

The truth of this origin of the moon is made evident to our very senses, for do we not see the frog riding the moon at night? And the moon is cold, because the frog from which it was made was cold.

The philosopher of Oraibi tells us that, when the people ascended by means of the magical tree which constituted the ladder from the lower world to this, they found the firmament—the ceiling of this world low down upon the earth—the floor of this world. Machito, one of their gods, raised the firmament on his shoulders to where it is now seen. Still the world was dark, as there was no sun, no moon, and no stars. So the people murmured because of the darkness and the cold. Machito said, "Bring me seven maidens," and they brought him seven maidens; and he said, "Bring me seven baskets of cotton bolls," and they brought him seven baskets of cotton bolls; and he taught the seven maidens to weave a magical fabric from the cotton, and when they had finished it he held it aloft, and the breeze carried it away toward the firmament, and in the twinkling of an eye it was transformed into a beautiful full-orbed moon, and the same breeze caught the remnants of flocculent cotton which the maidens had scattered during their work, and carried them aloft, and they were transformed into bright stars. But still it was cold, and the people murmured again, and Machito said, "Bring me seven buffalo robes," and they brought him seven buffalo robes, and from the densely matted hair of the robes he wove another wonderful fabric, which the storm carried away into the sky, and it was transformed into the full-orbed sun.

Then Machito appointed times and seasons and ways for the heavenly bodies, and the gods of the firmament have obeyed the injunctions of Machito from the day of their creation to the present.

The Norse philosopher tells us that Night and Day each has a horse and a car, and they drive successively one after the other around the world in twenty-four hours. Night rides first with her steed, named Dew-hair, and every morning as he ends his course he bedews the earth with foam from his bit. The steed

driven by Day is Shining-hair. All the sky and earth glisten with the light of his name. Jarnved, the great iron-wood forest lying to the east of Midgard, is the abode of a race of witches. One monster witch is the mother of many sons in the form of wolves, two of which are Skol and Hate. Skol is the wolf that would devour the maiden, Sun, and she daily flies from the maw of the terrible beast, and the moon-man flies from the wolf Hate.

The philosopher of Samos tells us that the earth is surrounded by hollow crystalline spheres set one within another, and all revolving at different rates from east to west about the earth, and that the sun is set in one of these spheres, and the moon in another.

The philosopher of civilisation tells us that the sun is an incandescent globe, one of the millions afloat in space. About this globe the planets revolve, and the sun and planets and moons were formed from nebulous matter by the gradual segregration of their particles, controlled by the laws of gravity, motion, and affinity. The sun, travelling by an appointed way across the heavens, with the never-ending succession of day and night, and the ever-recurring train of seasons, is one of the subjects of every philosophy. Among all peoples, in all times, there is an explanation of these phenomena, but in the lowest stage, away down in savagery, how few the facts discerned, how vague the discriminations made, how superficial the resemblances by which the phenomena are classified!

In this stage of culture, all the daily and monthly and yearly phenomena, which come as the direct result of the movements of the heavenly bodies, are interpreted as the doings of some one, some good acts. In civilisation, the philosopher presents us the science of astronomy, with all its accumulated facts of magnitude, and weights, and orbits, and distances, and velocities, with all the nice discriminations of absolute, relative, and apparent motions, and all these facts he is endeavouring to classify in homologic categories, and the evolutions and revolutions of the heavenly bodies are explained as an orderly succession of events.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Exactly 102 names are in the Cambridge Mathematical Tripos list this year (including three Ægrotant honours). The signification of this is not quite apparent, but lower in the list will be found two whose degree is allowed, but who are not to count it as an honour's degree. These men did well enough in the part of the examination they took to deserve a "poll," and not an honour's degree. Trinity has passed more than a score, St. John's 14, several colleges eight; but Jesus, Sidney, and Magdalene, as usual, have few mathematicians. Christ's has picked up well, having no fewer than ten in mathematical honours; Trinity Hall gets in only two, and Downing has one representative.

Prof. Humphry announces that his lectures on Anatomy and Physiology (the Muscular and Circulatory Systems) will be resumed on February 3, while his classes for the second M.B. and for the Natural Sciences Tripos recommence on Friday, February 6. Mr. Wherry (recently elected surgeon to Addenbrooke's Hospital) began a class in osteology on January 21, continuing on Mondays, Wednesdays, and Fridays at 1 P.M. Dr. Paget's lectures on the Principles and Practice of Physic begin on Monday, February 2.

SCIENTIFIC SERIALS

The Quarterly Journal of Microscopical Science, January.—H. M. Ward, on the embryo-sac and development of *Gymnadenia conopsea*, pl. 1-3.—Fred. Elfving, studies on the pollen bodies of the angiosperms, pl. 4.—F. O. Bower, on the development of the conceptacle in the Fucaceæ, pl. 5.—Dr. Cunningham, on certain effects of starvation on vegetable and animal tissues.—J. E. Bloomfield, on the development of spermatozoa; part 1, Lumbricus, pl. 6, 7.—F. M. Balfour, on the spinal nerves of Amphioxus.—G. A. Hansen, the bacillus of leprosy, pl. 8.—Notes and Memoranda.—*Proceedings* of Dublin Microscopical Club, April, 1879, to October, 1879.

THE *American Naturalist*, vol. xiii. No. 12., December, 1879.—George H. Perkins, archæology of the Champlain Valley.—G. de Mortillet, the origin of the domestic animals.—F. Brendel, historical sketch of the science of botany in North America from 1635 to 1840.—E. D. Cope, on the extinct American rhinoceroses and their allies.—Recent Literature; General Notes; Scientific News.

Vol. 14, No. 1, January.—Henry J. Rice, observations on the habits, structure, and development of *Amphioxus lanceolatus*.—Elliot Coues, sketch of North American ornithology in 1879.—F. Brendel, historical sketch of the science of botany in North America from 1840 to 1858.—The Editor, notes on the present position of affairs in the Philadelphia Academy.—Recent Literature; General Notes; Scientific News.

Proceedings of the Academy of Natural Sciences, Philadelphia, 1879. Part 2, April to October.—Thos. Meehan, on hybrid fuchsias; on special fecundity in plants; do snakes swallow their young? on *Lonas inodora*; on sex in *Castanea americana*; Variations in *Thuja* and *Retinospora*.—Rev. H. C. M'Cook, the adoption of an ant-queen; mode of depositing ant-eggs; on the marriage flights of *Lasius flavus* and *Myrmica lobricornis*; pairing of *Linyphia marginata*; on mound-making ants; notes on *Tetramorium cæspitum*; on *Myrmecocystus mexicanus*.—John A. Ryder: on a new Pauropod and its larva (*Eurypauropus spinosus*); on a new Chirocephalus, *C. holmanii*; on honey glands on Catalpa leaves; description of *Streptocephalus sealii*, sp. nov.—Dr. Chapman, on Amphiuma; placenta of *Macacus cynomolgus*.—Dr. Dercum: the lateral sensory apparatus of fishes.—Dr. Leidy: on rhizopods in Sphagnum; fossil foot-tracks of the anthracite coal-measures; explosion of a diamond; on *Orgyia*; on some coast animals of New Jersey; on *Cristatella idæ*; on *Amœba blattæ*.—E. Potts: on the supposed sensitive characters of the glands of the Asclepiadaceæ.—E. Goldsmith, on amber containing fossil insects.—Angelo Heilprin, on some new eocene fossils from the Claiborne marine formation of Alabama, plate 13.

Revue des Sciences Naturelles, 2nd ser., tome 1, No. 3, December 15, 1879.—L. Tillier, contributions to a memoir on the geographical distribution of marine fish (conclusion).—A. de Saint-Simon, anatomical notes on some species of Pomatias.—Ph. Thomas, note on some species of horses found fossil in the neighbourhood of Constantine.—M. Leymerie, a sketch of the Pyrenees of the department of Aude.—Scientific Review, containing notices of French works on zoology, botany, and geology, published in 1879.—Bulletin.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, November 27, 1879.—"On certain Definite Integrals," No. 6. By W. H. L. Russell, F.R.S.

January 6.—"On certain Definite Integrals," No. 7. By W. H. L. Russell, F.R.S.

"On a Possible Mode of Detecting a Motion of the Solar System through the Luminiferous Ether." By the late Prof. J. Clerk Maxwell. In a letter to Mr. D. P. Todd, Director of the *Nautical Almanac* Office, Washington, U.S. Communicated by Prof. Stokes, Sec. R.S.

Mr. Todd has been so good as to communicate to me a copy of the subjoined letter, and has kindly permitted me to make any use of it.

As the notice referred to by Maxwell in the *Encyclopædia Britannica* is very brief, being confined to a single sentence, and as the subject is one of great interest, I have thought it best to communicate the letter to the Royal Society.

From the researches of Mr. Huggins on the radial component of the relative velocity of our sun and certain stars, the coefficient of the inequality which we might expect as not unlikely, would be only something comparable with half a second of time. This, no doubt, would be a very delicate matter to determine. Still, for anything we know *à priori* to the contrary, the motion might be very much greater than what would correspond to this; and the idea has a value of its own, irrespective of the possibility of actually making the determination.

In his letter to me Mr. Todd remarks, "I regard the communication as one of extraordinary importance, although (as you will notice if you have access to the reply which I made) it is likely to be a long time before we shall have tables of the satellites of Jupiter sufficiently accurate to put the matter to a practical test."

I have not thought it expedient to delay the publication of the letter on the chance that something bearing on the subject might be found among Maxwell's papers.

(Copy.)

Cavendish Laboratory,
Cambridge,
19th March, 1879

SIR,

I have received with much pleasure the tables of the satellites of Jupiter which you have been so kind as to send me, and I am encouraged by your interest in the Jovial system to ask you if you have made any special study of the apparent retardation of the eclipses as affected by the geocentric position of Jupiter.

I am told that observations of this kind have been somewhat put out of fashion by other methods of determining quantities related to the velocity of light, but they afford the *only* method, so far as I know, of getting any estimate of the direction and magnitude of the velocity of the sun with respect to the luminiferous medium. Even if we were sure of the theory of aberration, we can only get differences of position of stars, and in the terrestrial methods of determining the velocity of light, the light comes back along the same path again, so that the velocity of the earth with respect to the ether would alter the time of the double passage by a quantity depending on the square of the ratio of the earth's velocity to that of light, and this is quite too small to be observed.

But if J E is the distance of Jupiter from the earth, and l the geocentric longitude, and if l' is the longitude and λ the latitude of the direction in which the sun is moving through ether with velocity v, and if V is the velocity of light and t the time of transit from J to E,

$$J E = [V - v \cos \lambda \cos (l - l')] t.$$

By a comparison of the values of t when Jupiter is in different signs of the zodiac, it would be possible to determine l' and $v \cos \lambda$.

I do not see how to determine λ, unless we had a planet with an orbit very much inclined to the ecliptic. It may be noticed that whereas the determination of V, the velocity of light, by this method depends on the differences of J E, that is, on the diameter of the earth's orbit, the determination of $v \cos \lambda$ depends on J E itself, a much larger quantity.

But no method can be made available without good tables of the motion of the satellites, and as I am not an astronomer, I do not know whether, in comparing the observations with the tables of Damoiseau, any attempt has been made to consider the term in $v \cos \lambda$.

I have, therefore, taken the liberty of writing to you, as the matter is beyond the reach of any one who has not made a special study of the satellites.

In the article E [ether] in the ninth edition of the "Encyclopædia Britannica," I have collected all the facts I know about the relative motion of the ether and the bodies which move in it, and have shown that nothing can be inferred about this relative motion from any phenomena hitherto observed, except the eclipses, &c., of the satellites of a planet, the more distant the better.

If you know of any work done in this direction, either by yourself or others, I should esteem it a favour to be told of it.

Believe me,
Yours faithfully,
(Signed) J. CLERK MAXWELL

D. P. Todd, Esq.

Linnean Society, January 15.—Prof. Allman, president, in the chair.—Mr. A. J. Hewett exhibited and made remarks on a common web on community of cocoons, and of the moths (genus *Anaphe?*) escaped therefrom, said to have been got at Old Calabar.—Mr. Baker brought under notice a monstrous form of Thistle (*Carduus crispus*) obtained by the Rev. J. A. Preston in Wiltshire. In this specimen the capitula were abnormally numerous, and aggregated in secondary heads as in *Echinops*.—A Moa's tibia and tarsus (*Dinornis maximus*) dug up four feet from the surface at Omaru, N.Z., were shown on behalf of Mr. Jas. Forsyth.—A paper was read on the birds and mammals introduced into New Zealand, by Mr. H. M. Brewer. The author refers to Dr. Buller's Avifauna of New Zealand as not written too soon, for the rapid disappearance of many highly interesting forms is to be deplored. Finches and other small birds introduced are preyed on by the New Zealand Owl, but nevertheless quite a long list of British songsters, game birds, and others have been successfully established. Pheasants in some districts abound; and it is observed that when the tremor of an earthquake occurs the cock pheasants set up a continuous crow, either of defiance or fear (?). Partridges thrive best on the south island. Red deer are now seen in herds on the hills near Nelson. Hares have increased too rapidly, and the female in New Zealand has become more prolific, giving birth to six or seven young at a time. Kangaroos and various other mammals have likewise been imported, but unfortunately facts mentioned point out that the acclimatisation of some of them is not altogether an unmitigated blessing to the farmer colonist.—Then followed a memoir by Mr. J. G. Baker "Synopsis of the Aloineæ and Yuccoideæ." To these two tribes belong all the shrubby arborescent tribes of the capsular Liliaceæ. Aloes belong entirely to the Old World; out of a total of 200 species 170 being concentrated at the Cape of Good Hope, the remainder in the highlands of Tropical Africa. Of the Yuccoideæ there are about fifty species altogether, and nearly all are natives of Mexico and the Southern United States. The yuccas fruit rarely under cultivation, the large white pendulous flowers being in the wild plant fertilised by a moth of the genus *Pronuba*. *Herreria*, belonging to temperate South America, is a shrubby climber with the habit of *Smilax* and *Dioscorea*.—Messrs. J. Poland, J. Darell Stephens, and Prof. Allen Thomson were elected Fellows, and T. Jeffery Parker, an Associate of the Society.

Zoological Society, January 6.—Prof. Flower, F.R.S., president, in the chair.—Prof. Newton, F.R.S., V.P., exhibited, on behalf of Mr. G. B. Corbin, a specimen of *Acanthyllis* sive *Chætura caudacuta*—the Needle-tailed Swift—shot near Ringwood, in Hampshire, in July, 1879, remarking that it was the second example of this Siberian species which had been obtained in England.—Mr. John Henry Steel, F.Z.S., read a series of preliminary notes on the individual variations observed in the osteological and myological structure of the Domestic Ass (*Equus asinus*).—A communication was read from Mr. E. W. White, C.M.Z.S., containing notes on the distribution and habits of *Chlamyphorus truncatus*, from observations made by the author during a recent excursion into the western provinces of the Argentine Republic, undertaken for the purpose of obtaining a better knowledge of this animal.—Dr. John Mulvany, R.N., read a paper on a case which seemed to him to indicate the moulting of the horny beak in a Penguin of the genus *Eudyptes*.—Mr. O. Thomas, F.Z.S., read the description of a new species of *Mus*, obtained from the Island of Ovalau, Fiji, by Baron A. von Hügel, and proposed to be called *Mus huegeli*, after its discoverer.—A communication was read from Mr. R. G. Wardlaw Ramsay, F.Z.S., containing a report on a collection of birds made by Mr. Bock, a naturalist employed by the late Lord Tweeddale, in the neighbourhood of Padang. Three species were described as new, and proposed to be called *Dicrurus sumatranus*, *Turdinus marmoratus*, and *Myiophoneus castaneus*.—Dr. Günther, F.R.S., read a description of two new species of Antelopes, of the genus *Neotragus*, *N. kirki*, from Eastern Africa, and *N. molaris*, from Damara-land.

GENEVA

Society of Physics and Natural History, May 1, 1879.—M. Charles Soret details his experiments for investigating the mode of distribution of salts in solutions, the constituents of which are subjected to different temperatures. The attempts made upon azotate of potash and chloride of sodium led him to the discovery that there is a greater concentration in the cold part than in the warm.

June 5.—Prof. Schiff discusses the comparative properties of the nerves of sense and those of motion. He demonstrates on a curarised frog, the persistence of sensibility after the animal has lost all capacity for movement under the action of the poison. He observes, at the same time, that the persistence is only relative, and that the sensibility presently disappears, after an interval varying in duration according to the temperature. If that temperature is low (3° or 4° C., for example) the frog may live for eighteen days.—MM. L. Soret and E. Sarasin have determined the principal elements of the magnesium spectrum, by measuring the refraction indices of quartz for its principal lines, and by the existence of numerous photographs.—M. G. Lunel describes a new species of Trygonide belonging to the genus Pteroplatea, brought from Rio Janeiro.—M. R. Pictet reports his investigations to solve the problem—What form must be given to a definite surface that it may maintain its equilibrium in the air with the minimum of mechanical work? His experiments were made with kites having a dynamometer of great

sensibility attached to their strings. His conclusion was that, with reference to the work done, a given surface would the more easily support a fixed weight in proportion as the surface presented its shorter dimension in the direction of the wind, and its longer dimension perpendicularly to that direction.—M. A. De Candolle announces the publication of the last part of the fourth volume of M. Boissier's "Flora Orientalis," completing the description of Dicotyledons.

July 8.—MM. Micheli gives an abstract of his monographic investigations of the families Alismaceæ, Butomaceæ, and Juncagineæ.—M. E. Ador has studied with MM. Friedel and Crafts the action of chloride of methyl on benzine, in presence of chloride of aluminium.—M. Forel has detected in the oscillalations of the surface of Lake Geneva a movement which he terms "seiches dicrotes," consisting in a redoubling of the oscillation in two series of oscillations which mutually interfere, being of unequal duration.—Prof. Colladon observed an upward current of air round the Pissevache Waterfall, which is surrounded by a layer from 30 to 40 cm. in thickness, filled with very small drops of water. This phenomenon, due to the air-suction of the fall, might serve to explain the atmospheric currents accompanying the formation of hail.

August 7.—Prof. Schiff relates his researches on the action exercised upon hysterical subjects by the contact of metals and electric currents.—M. R. Pictet saw on Mount Jura, during a storm that broke forth on the 5th, a bluish light produced over a forest, resembling St. Elmo's fire. It disappeared and reappeared three times, under the influence of successive violent thunder-claps.

September 4.—M. Soret believes the "seiches dicrotes" observed by M. Forel can be explained by the superposition of two oscillations, one "uninodal," lasting seventy-two minutes, the other "binodal," lasting a little less than half that time.

October 2.—Dr. Marcet shows his instruments for collecting and analysing the air emitted from the lungs.—M. C. de Candolle has ascertained the prolonged action of low temperatures on the germinative power of various kinds of grain.

November 6.—MM. L. Soret and Rilliet have investigated the absorption of the ultra-violet rays of the spectrum by certain organic substances—the azotates of ethyl, isobutyle, and amyl, ammonia, &c.—M. R. Pictet presents a barometer intended to measure vapour tensions. It is composed of a vertical glass tube, wide at the top and very narrow towards the bottom, which bends at a right angle to be prolonged into a long horizontal tube. The lower level of the mercury is therefore constant, and as its volume does not vary, the variations of the higher level in the large tube are reproduced on a much enlarged scale in the narrow horizontal tube from which the readings are taken.—From a comparison of a series of eighty years' meteorological observations made at Geneva, Prof. Wartman has observed that the odds are remarkable in favour of August 15 being a stormy day.—Prof. Brun shows a fragment of fulgurite found on Mount Jura in chalky soil, a circumstance of very rare occurrence. Its surface is covered with small vitrefied globules which can only be explained by the fusion of the chalky matter under the influence of the lightning.

PARIS

Academy of Sciences, January 19.—M. Edm. Becquerel in the chair.—M. Daubrée presented the fifth volume of his "Traité de Méchanique."—The following papers were read:—On some applications of elliptic functions, by M. Hermite.—On the heat of formation of hydrate of chloral, by M. Berthelot. He offers experimental proof that gaseous chloral and water-vapour combine together with liberation of heat, and without change of state. The two are introduced from the boiling liquids into a small glass globe (with thermometer and drawing-off tube), one through a straight, the other (chloral vapour) through a spiral tube, and these parts are inclosed in a stoppered piece of glass tube, through which a steam current from the same source as the other circulates, and which also holds a thermometer. Throughout the experiment, after the vapours met, the thermometer in the globe showed a higher temperature than that of the inclosure, and the temperature was about 1° above that of boiling water, during twenty-five minutes. Negative results may be got, if the relative proportions be not regulated.—Note on hydrate of chloral, by M. Wurtz. With similar conditions of experiment, and the chloral previously boiled to expel hydrochloric acid, he had not found the least rise of temperature.—Note on the utility of concentric curved plates to alternately charge siphons by means of an oscillating liquid column, by M. De Caligny.—Simplification of American audiphone apparatus for the deaf and dumb, by M. Colladon. A simple disk of a particular kind of pasteboard, which is compact, homogeneous, elastic, and tenacious, is substituted for the hardened caoutchouc, no cords being required to fix the tension. The part applied to the teeth is coated with a substance to resist moisture. Musical sounds, and words uttered near, were understood by deaf mutes who tried the instrument.—The General Inspector of Navigation communicated figures regarding the daily height of the Seine in 1879, at the Pont Royal and the Pont de la Tournelle. The highest water at the former was 6·21 m. on January 9, the lowest 1·67 m. on October 10, 15, and 17; the mean, 2·72 m.—A letter was read, suggesting to saw into pieces the bank of ice on the Loire, near Saumur. Admiral Paris gave details of an attempt made in Russia in 1855 to liberate ships from ice by means of saws. He thought the method very useful where there is a current to carry off the ice; the ice being sawn in long strips across the current, which break up *en route*.—On a class of linear differential equations, by M. Picard.—Experimental and clinical researches on anæsthesia produced by lesions of the cerebral convolutions, by M. Tripier. Sensibility may be more or less diminished by lesions of the fronto-parietal region, which has been thought only a motor zone.—On the plants which serve as base for various curares, by M. Planchon. Four distinct regions are centres of preparation for curare, and for each a principal plant can be indicated. (The regions are English and Upper French Guiana, that of the Upper Amazon, and that of the Rio Negro.)—On the linear and lacunar confluents of the connective tissue of the cornea, by M. Renaut.—On the parturition of the common porpoise (*Phocæna communis*), by M. Jourdain.—Influence of climates on the maturation of corn, by M. Balland. This relates to observations at Orleansville, in Algeria. The mean monthly temperatures in 1877–78–79 are given; they range from 7°·8 to 32°·6. It is calculated that wheat, to reach its full evolution, must have received 2498° of heat in 1877-78, and 2432° in 1878-79, which is near the number (2365°) obtained by M. Hervé Mangon for Normandy; but the time required at Orleansville was 180 days, as against 266 in the other case.—Remarks on the use of Smithson's pile for detection of mercury, especially in mineral waters, by M. Lefort. Arsenic may, with it, be confounded with mercury. The easy reduction of oxygenated acids of arsenic by metals, under influence of the weakest electric current, is made evident.—Light, cover, and humus, studied in their influence on the vegetation of trees in forests, by M. Gurnaud.

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