THURSDAY, SEPTEMBER 5, 1889.

SIR WILLIAM THOMSON'S POPULAR LECTURES.

Popular Lectures and Addresses. By Sir William Thomson, LL.D., F.R.S., F.R.S.E., &c. In Three Volumes. Vol. I. Constitution of Matter. With Illustrations. ("Nature Series.") Pp. xi. + 460. (London: Macmillan and Co., 1889)

TO review a book by the leader of British physical science in the ordinary sense of reviewing would be absurd. To attempt an estimate of merit and demerit or to offer a superficial criticism might easily become an impertinence.

The object of a review in such a case as this is mainly to give persons who have not concentrated their attention on physics some idea of the nature of the book, so as to enable them to form a judgment how far it is suitable and accessible to them.

For, inasmuch as the greater part of what is published by the author of this book is stiff reading for trained physicists and mathematicians, and inasmuch also as the subjects of which he treats even in popular lectures are usually extremely abstruse, and such as require, if they are to be accurately stated at all, a very carefully-selected form of words and a rather involved construction of sentences, the idea may easily grow that anything by Sir William Thomson is mainly unintelligible. And unintelligible it probably is to the general public in their after-dinner arm-chairs. Unintelligible it quite possibly was to a large percentage of the audience in their after-dinner seats at the Royal Institution, though the personality of the man and the magnetism of his enthusiasm could hardly fail to enchain the attention of the most cynical or casual hearer.

In the printed book this personal charm is fainter; it is not absent—to those who have ever heard him the manner in which the illustrations are brought forward, the very tone of voice with which the sentences were delivered, are continually suggesting themselves—but it is fainter; and it becomes a question how far these lectures, which are undoubtedly scientific, are really popular, *i.e.* are really adapted to intelligent persons interested generally in the subject but who make no claim to be specialists in it. To answer this question I will run through the contents in such order as may be convenient.

About the middle of the book there is a quite popular and easy essay on the sense-organs of man, including that most important and fundamental sense—the sense of muscular exertion—without which it is doubtful if we should be conscious of an external world at all, insisting on a distinct sense of heat, and lumping together taste and smell; an essay in which certain much-needed clarifying statements are made to counteract some confusions introduced by more than one very popular book, as, for instance, the familiar difficulty about the relationship between light and radiant heat.

Next comes a lecture on the wave theory of light, delivered in America, giving to anyone who has attended a course of lectures or read some popular treatise on light a very good general notion of what is meant by the elastic-solid theory of the ether, and of the way in which the difficulties introduced by supposing light to consist of ordinary mechanical transverse vibrations of an elastic medium have to be met.

Then follows a perfectly beautiful series of discourses or articles on the age of the sun's heat, which, looked at from the point of view of the general reader, perhaps form the gem of the whole.

The cool collected way in which a possible and more or less probable way of forming our sun is gone into, with every detail clear-cut and closely reasoned out, forms a study than which nothing more instructive, more suggestive, and more wildly interesting is likely to be accessible with equal ease to the imaginative reader.

A sustained power of attention, a period free from interruption, and a power of forming vivid conceptions, are all that is needed for a comparatively uninstructed reader to receive some of the most splendid cosmical speculations of our time. He may not know exactly why when the two earth-like bodies start to rush together it is stated that they will meet in six months, or that the collision will last half an hour, and perhaps he may find some difficulty in picturing the equatorial zone or disk and the axial rod between which forms the mass will subsequently oscillate till it settles down into a globular and white-hot sun; but he may rest assured that none of these statements nor any such numerical statements met with in this book are random ones; they are all the result of exact mechanical knowledge and arithmetic, and whether they be precisely true or not, they are, at all events, righter than anything else he is likely to come across.

Irrespective of that on which stress is laid in the title, viz. the *age* of the sun's heat, we have in these essays a popular and very clear exposition of the solution of that long-standing puzzle—the means by which solar heat is maintained.

With lumps of matter of ordinary size (*i.e.* not incomparably greater or less than the human body) gravitation is a force altogether insignificant in comparison with chemical affinity, and accordingly while the combustion of a lump of coal transforms great quantities of energy, the force of gravitative attraction between two such lumps or between one lump and the oxygen it can combine with is so minute as to require a Cavendish and a Boys to demonstrate its existence to an audience. But with lumps of matter of sizes such as are found in the depths of space the case is quite otherwise. Between them gravitative attraction is furiously greater than any known kind of chemical affinity; and the work such masses can do in falling together, nay even the work one lump can do in slowly contracting upon itself, is sufficient to maintain radiation at the sun's prodigious rate.

Take a large enough mass of gas (*i.e.* of detached atoms), let its parts gravitate together continually, and you have a sun—a sun, moreover, obeying simple mechanical laws, and with a life-period, in its molten and therefore uncrusted and radiative state, of roughly calculable length.

It may be worth while parenthetically to remark that, whereas the chemical (or electrical) attraction between two atoms at any distance exceeds their gravitative attraction at the same distance more than a thousand million billion billion times, the atoms being regarded as spheres oppositely electrified each to about the potential of a volt, the gravitative attraction between two worlds the size and density of our earth exceeds their electrical attraction when likewise oppositely charged each to a volt in just about the same ratio. The ratio of the forces depends, in fact, on the fourth power of the linear dimensions of the bodies concerned—other things being fixed. For a couple of small bullets the two forces would be approximately equal.

Again, if every atom be regarded as separately charged, and able to combine with each other, we get the maximum possible energy of combustion, which may be put down as at the most 20,000 therms per gramme. The heat of formation of our moon by combustion is on this estimate very comparable to that developed by the falling together of its materials from infinity under gravitation. But whereas the energy of combustion is simply proportional to the masses concerned, the energy of gravitation is proportional to the product, *i.e.* to the second power of the masses; and so we find that when a body is as big as the sun the gravitative energy of its mere earthquake subsidence as it shrinks is enormously greater than that which could be afforded by the combustion of an equal mass. So also it is shown to be greater than could be caused by any reasonably permissible hail of meteorites from infinity: meaning by "reasonably permissible," such a hail as would not introduce planetary perturbations of a conspicuously non-existent amount.

Returning now to the beginning of the volume, we find an altogether admirable, but rather stiff discourse on capillarity. How it can help being stiff when it enters into problems usually treated by the higher mathematics, and hitherto reserved for specialists, I do not know. It is a serious mathematical essay done into ordinary language. The diagrams of the precise shape of liquid surfaces are beautiful, and such as are nowhere else to be found. To a reader who will concentrate his thought upon this discourse, it will gradually become luminously clear, but perhaps the conscientious person who always reads books from cover to cover, may run the risk of being choked off by the accident of its coming first.

Appended to it are three notes, one on the "tears of strong wine," as explained by Prof. James Thomson; one on the author's remarkable and beautiful discovery of the reasons why mist globules cannot form without a nucleus, why big rain-drops form at the expense of little ones, and why put-away clothes get damp; and lastly, a note on the sufficiency of Newtonian gravitation to explain cohesion. This latter is a highly ingenious piece of special pleading. It is so easy to prove that gravitation will *not* explain cohesion, on any of the commonly current mental ideas of what atoms are like; but here, by assuming a sufficiently violent concentration of substance in certain regions, and sufficient absence of all substance from other regions of an atom, it is shown that cohesion *may* be explained by gravitation. At least, it can be seen that different atoms can cling to each other, but it is not so clear how the various parts of the atoms themselves hang together. No-how, it seems to me, unless they are exaggeratedly fibrous structures, and unless the ends of the fibres of one atom cling on to the next, and thus build up a body like a cobweb. Nothing but cobweb can cohere by gravitation, so it seems to me (perhaps wrongly, of course); and although one has gradually learnt that no hypothesis concerning reality is *a priori* absurd or unlikely, yet this does not feel, nor indeed is it intended, as anything final or satisfactory.

Then comes a long lecture on electrical units of measurement, wherein the foundations of the conventional "absolute" systems of electrical measurement are explained and illustrated by showing how by means of electrical observations the fundamental standards of length, mass, and time might, if lost, be conceivably recovered. The subject is rather technical, and scarcely of sufficient general interest to repay the unelectrical reader, though there are here, as everywhere, numerous suggestive remarks. One might, perhaps, suggest that the distinction between the conventional and the essential is not always sufficiently borne in mind and enforced.

The lecture on the size of atoms is intensely interesting to everybody. Physicists know by how many different lines of argument a limit of smallness for the space occupied by an atom can be fixed, or an actual estimate of the number of molecules in a given lump of matter can be made. A number of these methods suggested by the author are here stated, and, with many illustrations, explained. But, besides this, there are instructive mechanical models or images illustrating Prof. Stokes's theory of phosphorescence, Cauchy's theory of dispersion, and the polarization of light by small particles.

The remaining subjects dealt with in this volume—elasticity regarded as a mode of motion, and a kinetic theory of matter—are closely related to each other, are wholly the author's own, and are among the most brilliant speculations of the century. But a small inkling of the great field thus opened up is given here—enough, however to afford to the reader some glimpse of the possibilities of development lying in this direction.

Such are the contents of the volume before us, and a more comprehensive collection of scientific addresses has seldom been published. They do not, of course, really represent Sir William Thomson at his best: neither they nor any other intelligible production of his is able to convey to the general reader an adequate notion of the magnitude of his solid work, or of the grounds for the veneration with which his contemporaries regard him.

Such as they are, however, every physicist will be glad to read these papers again in this handy form, and every intelligent and educated man who feels an interest in the strong thought of physical science during this eventful century will do well to make a serious effort to grasp at least the main outlines of the profound studies shadowed forth in this small volume. OLIVER J. LODGE.

THE MATHEMATICAL THEORY OF POLITICAL ECONOMY.

Éléments d'Économie Politique Pure. Par Léon Walras. (Lausanne: F. Rouge, 1889.)

THE appearance of a new and enlarged edition affords us a wished-for opportunity of calling attention to this original work. Its author is one of the favoured few to whom belongs the honour of having made a discovery in political economy. The title of Ricardo to the theory of rent is not better than the title of Prof. Walras to a theory more comprehensive than that of rent. It is a claim founded on originality rather than priority. Prof. Walras is the last of a small band of original thinkers who, in the latter half of this century, have independently excogitated the cardinal article in the doctrine of value. They have contemplated in different aspects the same fundamental conception: that value in exchange is neither simply identical with, nor wholly different from, value in use, but corresponds to the utility of the last, the least useful, portion of the commodities exchanged. "Nutzlichkeit des letzten Mengentheilchens," "Degree of Final Utility," "Grenznutzen," and "Rareté"—in different tongues and various terminology they proclaim the one essential truth which will be for ever associated with the names of Gossen, Jevons, Menger, and Walras.

This chronological, and, as it happens, alphabetical, arrangement is not identical with the order of merit. In that order we should place nearest together the names which are first and last in the series above written. Gossen appears to have been a mere specialist with few valuable ideas beyond the one which has made him immortal. Prof. Walras's light is more diffused. Yet it is true that we find in him rather *multum* than *multa;* that his principal achievement is the copious exposition of the one fundamental theorem to which we have referred. His next most important contribution to the stock of economic ideas relates to the function of the *entrepreneur*. Prof. Walras is one of the first who correctly conceived the *entrepreneur* as buying agencies of production (use of land, labour, and capital), and selling finished products in four markets, which thus become interdependent. His criticisms of the English school on this head are often valuable. Of the *entrepreneur's* funds, not pre-determined in the sense which some have imagined to any particular form of outlay, he well says:—

> "Il serait aussi impossible de distinguer ce fonds de roulement du travail du fonds de roulement de la rente foncière, ou du fonds de roulement du profit, que de distinguer dans un bassin à trois robinets l'eau destinée à s'écouler par un robinet de celle destinée a s'écouler par les deux autres."

But surely he goes too far in the way of abstraction when he insists that the ideal *entrepreneur* should be regarded as "making neither gain nor loss":—

> "Pour ce qui est de la part du profit constituant le bénéfice de l'entrepreneur l'école anglaise ne sort pas qu'elle est alcatoire, qu'elle dépend des circonstances exceptionelles, et non pas normales, et que, théoriquement, elle doit être négligée."

Perhaps his views on this and other points would have been more exact if he had considered the part which the "disutility" of labour—to use Jevons's phrase—plays as a factor of economic equilibrium, instead of confining his attention to "final utility." Another theory to which we ought to call attention is contained in the lesson on capitalization, which is added in the new edition. If the price of capital is determined by competition, it follows from the general theory of supply and demand that the maximum utility of all the parties concerned is realized in the same sense as in other markets. What is more than this in the newly-added theory has baffled us.

In the case just noticed and others, the argument is probably rendered obscure, or at least unattractive, by the use of symbols in excess of the modest requirements of elementary mathematical reasoning. The exuberance of algebraic foliage, rather than the fruit of economic truth, is the outcome of science thus cultivated. It is remarkable that the neatness which characterizes Prof. Walras's literary style, should not be reflected in his mathematical compositions. As an algebraist he has not attended to the maxim, *Il ne faut pas épuiser les choses.* We shall justify our criticism by referring to the chapters or "lessons," in which it is attempted to analyze what is called the "*tâtonnement*" of the market. The writer gives us three courses of this analysis. He diffuses over some thirty-five pages an idea which might have been adequately presented in a few paragraphs. For it is, after all, not a very good idea. What the author professes to demonstrate is the course which the higgling of the market takes—the path, as it were, by which the economic system works down to equilibrium. Now, as Jevons points out, the equations of exchange are of a statical, not a dynamical, character. They define a position of equilibrium, but they afford no information as to the path by which that point is reached. Prof. Walras's laboured lessons indicate *a* way, not *the* way, of descent to equilibrium. This is not the only topic with respect to which the laboriousness of the investigation is out of proportion to its importance.

Agreeing, therefore, in the main with Prof. Walras in his plea for the use of mathematical reasoning in economics, we fear that he may have prejudiced the cause by his advocacy. The excessive elaboration of his reasoning, compared with the simplicity of his conclusions, is calculated to excite suspicion. Moreover, he traduces the mathematical method when he applies it in such a manner as to justify the popular prejudice against abstract reasoning. He is surely *ultra crepidam*, he goes beyond the little hard matter with which the craft of the mathematician is concerned, when he offers opinions on the living organism of the industrial body, and the complexion of practical problems. His scheme of dosing the circulation by a nicely calculated injection of supplementary currency reminds us of the tailors in Swift's Laputa, who went through laborious mathematical computations in order to determine the measurements of a suit of clothes, which after all fitted very ill. When Prof. Walras offers us "the solution of the Anglo-Indian monetary problem," we think of Fluellen in the heat of the battle discoursing about the "discipline of the wars." There is a discipline adapted to the schools, and which it is profitable to have studied, but which has no direct bearing upon action.

A minor ground of complaint is formed by the extreme severity of our author's criticism, especially those which relate to the English school. We cannot think that Mill's oversights deserve the "*horribili flagello*" which is administered. To dismiss in a few lines "comme nul et non avenu" so much of that philosopher's reasoning appears to us rather slashing. But we are sensible that in condemning the unceremonious treatment of great men, we are laying down a law which applies to our own criticism of Prof. Walras. We shall therefore forbear to reduce our initial encomium by invidious reservations. When all that could be made are summed and subtracted, there would still remain to Prof. Walras the undoubted glory of an original discovery. He may say of that, as Napoleon of his victories, "*Il y a là du solide que la dent de l'envie ne peut ronger.*" F. Y. E.

MUSICAL INSTRUMENTS AND THEIR HOMES.

Musical Instruments and their Homes. By Mary E. Brown and W. Adams Brown. (New York: Dodd, Mead, and Co., 1888.)

THIS work should prove very useful to all who are interested in music and musical instruments. Primarily it professes to be a catalogue of the collection of musical instruments made by Mrs. J. Crosby Brown, of New York; but its value has been greatly augmented by a series of essays on the music and musical instruments of Oriental and savage races. The "catalogue" portion is well illustrated with clever pen-and-ink sketches, which give, for the most part, an excellent idea of the instruments, though they do not exhibit a great amount of detail. A brief description with dimensions, and, where possible, the native name, accompanies each sketch. The catalogue is divided into geographical sections, and at the end of each is added an essay treating of the music of the country from an historical and theoretical point of view, with a general account of the native instruments. Though these essays contain little that is new or original, they nevertheless form extremely useful compilations from a large amount of scattered literature; the references are full, and the list of authorities is a very representative one.

The sections devoted to China and India are of special interest, as dealing with regions which were the birthplaces of so many of the instruments in use amongst ourselves, changed though these be from their original forms. There seems little doubt, for example, that we owe the harmonium to China, and that instruments played upon with a bow had their original home in India, whither, too, we must refer the original use of "sympathetic strings."

Musical instruments, like all other products of man's handiwork, are subject to the laws of evolution, and each arrived at its present state by gradual stages of improvement. If the genealogies could be all followed back to the earliest stages, all instruments could be referred to such simple original forms as, for example, hollow or solid logs, reeds, or hunters' bows. With the rapid disappearance of the more primitive native instruments, the difficulty of tracing the history of music backwards by means of primitive "survivals" increases year by year. Every effort should be made to collect and place on record these simple forms, as from these we greatly derive our ideas of the "dawn" of music. The magnificent work by Hipkins and Gibb furnishes us with beautiful illustrations of *beautiful* instruments, but does not deal with the humbler kinds. The illustrations in the present work, therefore, are especially valuable, as the primitive instruments receive equal attention with the more elaborate. A very common error has crept into the pages of this otherwise excellent work—in the terminology. Nothing is more distinct than instruments of the "oboe" type and those of the "clarionet" type are from each other. These, though somewhat similar in general aspect, belong to different classes—the "double-reed" and the "single-reed" classes respectively; and any relationship must date back to the time when they each probably took their origin from a section of corn-stalk, the one form being sounded through the *pinched* end of the stalk, and the other through a *slit* cut in the end, and forming a *vibrating* or *beating* tongue. We find, however, in several passages a confusion of these terms. Thus, the Corean, Greek, and Spanish "clarionets," so called, are evidently "oboes," with double reeds for mouth-pieces. Similarly, the "pandeiro" of Madeira, not having a tense membrane, cannot be a "tambourine," however much it looks like one. The "mogugyo," or "wooden fish," of Corea and China, is called a "drum" in one passage (p. 80). But a "drum," too, *must* have a tense membrane, and an instrument ceases to be one if lacking this addition. The "mogugyo" is really far more closely allied to the "bell" series, though there is no general term which expresses this class of wooden instrument. Such mistakes are, doubtless, mere slips, but they are apt to be misleading.

In the description of savage music it is stated (p. 240) that the Mincopies have no musical instruments. This is not quite true, as they have one, though a simple one, and consisting merely of a hard-wood board, of special shape, which is used for sounding a rhythmical time for dancing. It is used only as a musical instrument, and so illustrates a step in advance of the Australian, who taps with a stick upon his "casting-board" for the same purpose, without employing a separate instrument.

It is to be hoped that other collectors will follow the excellent example of the authors of this work, and publish illustrated catalogues of their collections. We can hardly expect many such beautifully produced "catalogues," but the scientific spirit and easy style of this book might well be a model for others.

OUR BOOK SHELF.

Heat. By H. G. Madan, M.A., F.C.S. (London: Rivingtons, 1889.)

THIS is an elementary treatise of exceptional merit, combining thoroughly practical work with sound theoretical conclusions. The course of instruction which it comprises has been found suitable by the author, in his capacity as instructor at Eton College, for boys who already have some acquaintance with physiography and elementary dynamics. Mathematical expressions are accordingly used as little as possible, and, when used at all, they are fully explained in ordinary language.

A very large number of experiments—many of them new—are described, and we have the author's assurance that they are all capable of successful performance with moderate skill and care. It is rightly observed that experiments which do not always succeed, even with the greatest care, are altogether unsuitable for young students, as they invariably tend to make them lose confidence in the science.

Particular attention is given throughout to the application of the general laws of heat to the arts and manufactures, and to the phenomena of every-day life. The subject of ventilation, for example, is very fully discussed and illustrated by experiments. There is also a beautiful experiment illustrating the intermittent action of geysers (p. 195). Perhaps the most important application of the laws of heat, however, is the steam-engine; and most of the various forms, including locomotive and marine engines, are described. Even the gas-engine is briefly referred to.

There are no less than 138 excellent diagrams distributed throughout the text, most of which have been specially prepared for the book.

British Rainfall, 1888. By G. J. Symons, F.R.S. (London: E. Stanford, 1889.)

THIS work is a general summary and epitome of a year's work, and contains a Report upon the progress of rainfall investigations.

The volume is divided into three parts: the first deals with the measurement of snow, experimental gauges, the Camden Square evaporation experiments, and concludes with a list of the staff of observers, showing that the staff is still on the increase, although very slowly, the chief increase being in England.

The second part treats of the rainfall and meteorology for the year, as reported from the various observing stations. One of the heaviest short-period rains recorded is that which fell on March 24 at Chepstow, Shirenewton Hall; it lasted two minutes, and in that time the ground was covered 2 inches deep with snow, the flakes being 3¾ inches in diameter, and only ¼ inch thick, 6 inches of this snow yielding 1 inch of water, so that, if the snow had lasted one hour, it would have reached an average depth of 5 feet.

Maps and tables indicate the monthly rainfall for the year, the greatest fall being at "The Stye," in Cumberland (175·40 inches), the least at Skegness, in Lincolnshire (17·50 inches).

Lastly, Part III. consists of general tables of the total rainfall at the 2500 places of observation.

Putting together all the above facts, we find that during the year there was much dry weather, although few droughts; there were hours and days of excessive rain, months with amounts of rain almost without precedent. Yet, on the whole, we get a result not at all remarkable, but decidedly below the average.

Rainfall observers will find in this book a collection of most interesting tables, maps, and articles upon the various branches of the work; and as the new decade begins with January 1 next year, we hope that the staff of observers will number many of our readers among them.

Ancient Art of the Province of Chiriqui. By W. H. Holmes. (Washington: Government Printing Office, 1888.)

THIS is an extract from the sixth Annual Report of the U.S. Bureau of Ethnology, and will be read with interest by all students of American antiquities. Chiriqui occupies a part of the Isthmus of Panama, and at the present time is inhabited chiefly by Indians and natives of mixed blood. Many ancient cemeteries have been discovered along the Pacific slope of the district, and explorers have found in them a great quantity of more or less valuable objects of art. These objects Mr. Holmes has classified, and in the present monograph he carefully describes the characteristics of typical specimens. He first deals with the graves and their human remains, then passes on to consider, in order, objects in stone, objects in metal, and objects in clay. His descriptions are concise and lucid, and their value is greatly increased by a large number of excellent illustrations. Mr. Holmes is careful to point out that there is no valid reason for assigning a very high antiquity to the works of art found in Chiriqui. The tribes by whom the graves were made may, he thinks, have been in possession of the country, or parts of it, at the time of the conquest. Their pottery appears to indicate that they were more closely related with the ancient Costa Rican peoples than with those of continental South America; but in their burial customs, in the lack of enduring houses and temples, and in their use of gold, they were, as Mr. Holmes shows, like the ancient peoples of middle and southern New Granada.

An Elementary Treatise on Dynamics. By Benjamin Williamson, F.R.S., and Francis A. Tarleton, LL.D. Second Edition. (London: Longmans, Green, and Co., 1889.)

THIS work has been thoroughly revised, and a considerable alteration has been made in the order of its arrangement. The first half of the book treats of the dynamics of a particle, while the latter part deals with kinematics and kinetics of rigid bodies.

Many portions of the subject have been developed, and in some cases rewritten, especially that on generalized co-ordinates in connection with Legrange's and Hamilton's methods; the general theory of oscillations is exhibited in a new form.

The work has been arranged from the most elementary conceptions, so that anyone acquainted with the conditions of equilibrium, and with the notation of the calculus, may commence the treatise without studying any other book on the subject.

LETTERS TO THE EDITOR.

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

On some Effects of Lightning.

DURING the thunderstorms of the 6th and 7th of June last, some twenty trees and buildings were struck within a 5-mile radius from Cranleigh. I have examined most of the trees struck, and have found a remarkable similarity in the effects, which are of two kinds: the first, by far the most common effect, is simply a score out of the bark up the trunk of the tree, out along one limb, and then by perhaps two or three smaller branches to the outer twigs; the other effect is the shattering of the tree, which occurs, as Mr. Griffith remarks in NATURE of August 15 (p. 366), when the lightning course leaves the outer surface of the tree and enters between the bark and the wood at the junction of some main branch with either the stem or with some other branch, when the shattering would probably occur from some obstruction at the junction, or from there being water in a cavity or in a collection of dead leaves in the fork.

I imagine that in general the course of the electricity is outside of the bark, following one or more lines of moisture or running water down the tree; when this conductor becomes insufficient a discharge takes place, and the stream of water is converted into steam so violently as to destroy the bark instantly along the line of strain. If the sap within be also converted into steam by communication through a knot-hole or by a flaw, the bark is blown off altogether.

If the tension be very great indeed, and especially if the air round the tree be dry, the sap may be violently exploded, and the trunk splintered and shattered as if by dynamite.

Most of the trees in this neighbourhood were struck while it was raining; but one tree, a Scotch fir, occupying a prominent position on the side of a hill, was struck before any rain fell. This tree divided out into two arms nearly in line with the stem; one arm was thrown to the ground, the other remained up for a few hours, and was then blown down by the wind, falling in the opposite direction to the first arm. At the junction there was a great deal of turpentine which was thoroughly blackened. The trunk below the arms was shivered, and the bark thrown out to a distance very similarly to the case related by Mr. Griffith. One curious feature in the present instance was that the roots of the tree could be traced to a considerable distance by the earth above them being thrown up as over a mole barrow.

Most of the trees struck here have been oak; but there were also two poplars, four elms, a chestnut, and the fir above mentioned. It is said that beech-trees are never struck: probably the smooth close-fitting bark makes a better conductor than the rough bark of the oak. J. P. MACLEAR.

Cranleigh, August 26.

Nose-Blackening as a Preventive of Snow-Blindness.

IN vol. xxxviii. of NATURE there were several interesting letters on this subject. Will you allow me to suggest a possible explanation?

For some years past I have interested myself in the choroidal circulation, and my observations have led me to believe that when light is absorbed by the choroidal pigment the blood-supply at that spot is increased. If the light is intense, this increase soon has the effect of blurring the image, and if at the same time the light is intense and the exposure to it prolonged, the sensitiveness of the retina may suffer for some time after from the same cause, *i.e.* an abnormally large blood-supply in the choroid.

In the course of the blood-vessels there is just such a connection between the retina and the skin as the nose-blackening preventive requires. My suggestion is that the blackening of the skin increases its demand for blood in some way, perhaps by its increase of temperature, and that thus a larger supply is drawn along the main branches of the ophthalmic artery, the naso-frontalis, the supra-orbitalis, and the lacrymalis, diminishing the quantity which finds its way into the small and almost independent system of the choroid. In this way nose-blackening would save the retina from being oppressed and injured in the way mentioned.

I may mention that if anyone after walking for an hour or so in the snow covers his eyes so as to exclude all external light, he will find his eyes filled with a very bright retinal light, and also if he is at all accustomed to see the blood corpuscles moving in his field of vision he will see them at such a time very distinctly and in great numbers by looking at the sky.

The theory which my observations have led me to form, that there is a very intimate connection between the retinal light and the circulation in the choroid is almost necessarily crippled by the fact that it rests largely upon subjective phenomena which are misleading, are not demonstrable, and depending on the constitution of the subject are not readily confirmed. Until, therefore, I can hear of another worker in the same field whose observations agree in the main with my own, I do not feel prepared to publish them. HENRY BERNARD.

Jena, August.

A Method of Mounting Dried Plants.

AN example of a very useful and expeditious method of mounting herbarium specimens adopted here is inclosed as worthy of attention. Short strips of lead, used in packing tea, are passed through slits in the paper on each side of the part of the plant to be fastened, and the ends then bent flat out on the back of the sheet. The many advantages of using this, or some other pliable metal, in certain cases, are very obvious. Has this method been hitherto suggested? JOHN WILSON.

University, St. Andrews.

COLOUR-BLINDNESS AND DEFECTIVE FAR-SIGHT AMONG THE SEAMEN OF THE MERCANTILE MARINE.

IN the House of Commons, recently, attention was called to this subject by Dr. Farquharson, who, in stating that he would take an early opportunity of discussing it next session, intimated that the efficiency of the Board of Trade regulations on this matter was open to grave suspicion. On making inquiry, we find his doubts are only too well founded. When, in the year 1852, the carrying of red and green side-lights by sailing-vessels was made compulsory, the subject of colour-blindness had not awakened the attention of practical observers. Had the fact that from 3 to 4 per cent. of the whole male population are colour-blind then been known, it is possible that some mode other than by showing red and green lights would have been devised to indicate the positions of vessels at sea at night. As there is generally but a hazy conception of what is meant by the term colour-blind, we will briefly indicate its exact significance.

When, in 1794, the distinguished chemist, Dalton, published a description of his sense of colour, the scientific world were surprised to find that there existed individuals whose perception of colour differed in a remarkable way from that of their fellow-man. To have said that an individual possessed the sense of sight was tantamount to saying that he possessed the sense of colour, the latter being considered an integral part of the former; but Dalton's report clearly showed that the two senses were separate and distinct, and that, while an individual might have a perfect appreciation of form, he might also be quite unable to perceive any distinction between two or three or more distinct and different colours. Further investigation showed that there were a few people who could discern no colour at all, every object appearing as black or white, or as shades of black and white (grey). This is total colour-blindness, and is very rare. The usual form, and that which we allude to when we speak of a colour-blind, is that in which the individual can distinguish the colours blue and yellow, but can see no difference between the colours red, green, and brown; and from the fact that one of these individuals, if given a vivid scarlet skein of wool, will select to match with it green skeins and brown skeins, it follows that he must see green and scarlet as he sees brown. Now, there being between 3 and 4 per cent.[1] of the whole male population afflicted with this variety, it follows that a very large section of the community are by nature disqualified for all those positions in which the correct interpretation of coloured lights is essential to safety. Clear as this fact must be, it was not until Dr. George Wilson, of Edinburgh, in the year 1855, published his admirable work, entitled "Researches on Colour-blindness," that public attention was invited to the subject. He showed with the greatest clearness how the safety of a vessel lay in the hands of men—"look-outs," officers, and pilots—who might be colour-blind, but were unconscious of this defect, or afraid to confess it; and he came to the definite conclusion, as the colour-blind were in a minority in the community, therefore, those destined to deal with signals should be selected solely from the majority whose vision was normal, and he earnestly urged upon those in authority the necessity of excluding colour-blind men from the sea profession.

One sentence was prophetic, that in which he says "the appalling yearly list of lost vessels which appears in our Wreck Returns awakens the suspicion that more than one of these fatal disasters may have resulted from the mistaken colour of a lighthouse beacon or harbour lamp, which on a strange coast, and with the accompaniments of a snow-storm or a thick fog, has been wrongly deciphered by a colour-blind pilot."[2] And if true of the pre-steamship days when vessels carolled along at the rate of a few miles an hour, what is to be said of the present time, when our "greyhounds" of the ocean flash along at the rate of twenty miles an hour, day and night? The regulation red and green lights of a steamer are

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[1] Holmgren examined 32,165 men: 1019 colour-blind—3·168 per cent. Joy Jeffries examined 10,387 men: 431 colour-blind—4·149 per cent. London Committee examined 14,846 men: 617 colour-blind—4·156 per cent.

[2] Colour-blindness proved to be the cause of *Lumberman* and *Isaac Bell* collision: ten lives lost. Colour-blindness proved to be the cause of loss of *City of Austin*—colour of buoys was mistaken: money loss £40,000. Colour-blindness or defective sight was the cause of collision between *Carbet Castle* and *J. H. Ramier*: money loss £1900.

supposed to be seen at a distance of two miles. Take the case of two steamers going in opposite directions at the rate of twenty miles an hour. It follows that from the very moment of those on board being aware of each other's position, but three minutes will elapse before they meet. Is not this little enough time to alter a course? And this is premising a clear atmosphere. Should the night be hazy, the oil poor, the wick badly trimmed, or the glass dirty, the distance at which the light can be seen is lessened, and the time to alter a course correspondingly diminished. Add to this the presence of a colour-blind or defective far-sighted "look-out" or officer, and there is present every attribute for the accomplishment of those terrible tales of the sea which year after year greet us with an alarming regularity. The *Times* (February 5, 1889), in reporting the terrible collision which occurred in the channel, *in fine clear weather*, in which both vessels (s.s. *Nereid* and s. *Killochan*) went to the bottom in less than five minutes, carrying to a watery grave twenty-three men out of a total of forty-two, remarks: "All inquiries respecting the cause of this disaster lead to the same conclusion, that it was due to one of those astounding errors of judgment on the part of one or other of the navigators which seem to defy all attempts at reasonable excuse." Read in the light we suggest, and the cause is as clear as daylight.

The same may be said of the terrible collision, when, again on a *perfectly clear night*, the s.s. *Douro* and the s.s. *Yrurac Bat* both went to the bottom with more than two score of their living burden. And who can say that the loss of the s.s. *Ville du Havre*, with its appalling death-roll, was not directly due to the colour-blindness of "look-out" or officer on one of the colliding vessels? We know how the inquiry ended. The English Admiralty decided that the English vessel was free from all blame, and the French Admiralty declared that the French vessel could not be in any way incriminated. But no one thought of attributing the mistake to the very probable one of colour-blindness. Now, what are the precautions taken to guard against those dangers which the employment of "colour-blind" and defective far-sighted sailors renders possible? We reply advisedly and after careful inquiry, "Practically none." It is true that twenty-two years *after* Dr. Wilson had so graphically described these dangers, the Board of Trade authorities awoke from their long sleep of indifference, and said that they recognized "the serious consequences which might arise from an officer of any vessel being unable to distinguish the colour of the lights and flags which were carried by vessels," and they instituted "tests and regulations," the value of which will be indicated by the following facts. The regulations do not prevent colour-blind "look-outs," colour-blind pilots, colour-blind A.B.'s, or colour-blind apprentices remaining sailors to the end of their days. They do not prevent colour-blind first mates or colour-blind captains and masters retaining their positions also to the end of their days; nay, more, they actually give colour-blind officers certificates that they are not colour-blind.

Should anyone doubt these grave statements, let him read the Board of Trade Reports for 1885, 1887, and 1888. He will find there that no less than forty-five officers rejected for colour-blindness were eventually given unendorsed certificates, which is identical to saying these men were not colour-blind, and that they were perfectly capable of taking charge of the lives of hundreds of helpless passengers, and of property to the extent of thousands. Is not this little less than a public scandal? We who know that colour-blindness is congenital and incurable, know either that these forty-five men were not colour-blind when rejected, or that they are colour-blind to-day. Which contention is the more likely may be gathered from the fact that, of these legalized non-colour-blind men, according to the Board of Trade's own reports, four were unable to distinguish red from green, twenty-two more called the colour red green, five others called the colour green red, and the remainder made mistakes of a marked character.

The Board of Trade so-called "tests for the detection of colour-blindness," viz. the requiring candidates to give the *names* of coloured cards, and lights shown them—tests which are stated to be "sufficient to prevent anyone who is more or less (!) colour-blind escaping detection by the examiner"—may be tests of a man's education in the *names* of colours, but as tests of the colour sense, they are not trustworthy, and tests which are not trustworthy are worse than no tests at all. Practically, the "tests" are barely worth the paper they are printed on. And the "regulations" dealing with a colour-blind officer, should he be detected, are of a like character, a snare and delusion. The public and the shipowners believed, rightly or wrongly it matters not, that the regulations were framed to absolutely keep colour-blind officers out of the sea service. They do nothing of the sort. With regard to the far-sight of a sailor or officer, there are no tests at all; a man may be the subject of any of the various forms of eye-disease, may have any degree of blindness, or may be so shortsighted as to be unable to see distinctly more than a few inches in front of his nose, and yet be at perfect liberty to be a sailor to the end of his days, or to become an officer. Are the public going to allow this grave condition of affairs to remain as they are? We answer emphatically "No," and we feel sure that when Dr. Farquharson brings the subject forward, the House of Commons will insist that the Board of Trade authorities who have the duty and privilege of providing for the safety of the travelling community of the first maritime nation of the world, will take, even though thus late, such precautions as will insure to the sea traveller immunity from those dangers which the present employment of colour-blind and defective far-sighted sailors renders possible.

ST. ELMO'S FIRE ON BEN NEVIS.[1]

St. Elmo's Fire as seen occasionally at the Ben Nevis Observatory takes the form of jets of light on the tops of all objects that stand any height above the general level of the roof of the Observatory, such as the chimneys, anemometers, lightning-rod, &c. In a very fine display the tops of the objects are quite ablaze with the phenomenon, which then glows and hisses in brilliant tongues of white and blue, from four to six, or even more, inches in length. Nor is the phenomenon confined to these objects alone in the finer displays, but if the observer stands on the roof his hair, hat, pencil, &c., glow with it as well, and when he raises a stick above his head the stick has also a long flame at the top. Further, however, than having a slight tingling in his head and hands he suffers no inconvenience. The hissing is a very marked characteristic of the phenomenon, being always heard during ordinary displays, though in the feebler displays, when the light can barely be seen, it cannot be distinguished from the hissing of the wind and the snow drift. On one occasion the sound was a very highly pitched note. In the finer and even in ordinary displays St. Elmo's Fire is an object of great beauty, and the stormy character of the weather—namely, squally winds with heavy showers of snow and hail, and with clouds of snow-drift flying all around—heightens rather than diminishes the effect, although at the same time it detracts from the convenience of observing with advantage.

Up till the summer of 1888 fifteen cases of St. Elmo's

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[1] Abstract of a paper on "St. Elmo's Fire on Ben Nevis," by Angus Rankin in the Journal of the Scottish Meteorological Society, third series, No. v.

Fire were noted at the Observatory, and these occurred all in the night-time, and all during the winter months, beginning with September and ending with February. The feebleness of the light it gives forth, in comparison with ordinary daylight, makes it difficult if not impossible to see it during the day, which is probably the reason for its being a nocturnal phenomenon, and partly for its being a winter phenomenon, for the short summer nights greatly lessen the chances of seeing it. Other reasons for its being a winter phenomenon, and not a summer one as well, are that the high temperature in summer is not favourable to its appearance, and that the weather type in which it appears is far more common in winter than in summer. The fifteen cases are distributed throughout the winter months as follows: two in September, three in October, five in November, two in December, one in January, and two in February. An investigation of these cases, and of the meteorological observations made before and after each appearance of St. Elmo's Fire, has shown that the weather which precedes, accompanies, and follows it has very definite characteristics, not only on Ben Nevis but also over the whole west of Europe. What these characteristics are will be briefly explained in what follows.

To arrive at a knowledge of the conditions obtaining on Ben Nevis, the observations of pressure, temperature, wind direction, and rainfall, made at the Observatory, were tabulated for each hour from thirty hours before to twenty-four hours after the appearance of the phenomenon in each case. The general averages thus obtained for the whole fifteen cases for each element give very decided curves. They show that, as regards pressure, the barometer, from being 24·993 inches at thirty hours before St. Elmo's Fire is seen, steadily falls till the sixth hour before, when it is 24·771 inches, and thereafter rises till the twenty-fourth hour after the display, when it is 24·979 inches. At the hour at which St. Elmo's Fire is seen and the following hour, however, a slight dip is indicated in the ascending curve. Taking all the pressure averages into account, they indicate a well-defined depression, in which St. Elmo's Fire is seen six hours after the centre, or point of lowest barometer, has passed. It is important to note that all the pressure averages come out below 25·000 inches, because this indicates that the depression occurs while Ben Nevis is in an area of general low pressure—the mean barometric pressure at the Observatory for the four years ending 1887 being 25·296 inches.

The temperature averages show a broad maximum from twenty-four to sixteen hours before and a minimum sixteen hours after St. Elmo's Fire is seen—the range being 3°·7; and between these hours there is a continuous fall in the temperature. The rate of fall is greater before than after the display. In some of the cases the range is much greater than 3°·7—in one it is 13°·4. It was chiefly from the observations of temperature that we were able on several occasions to give successful forecasts of the appearance of the phenomenon several hours before it occurred.

The chief points shown by the wind-direction averages are, that they all belong to the western half of the compass; that till the tenth hour before St. Elmo's Fire is seen the wind blows from a south of west direction, and thereafter from north of west; and that from the twenty-fourth hour before St. Elmo's Fire is seen, when the direction is south-west, the wind steadily veers till the fourth hour before, when it is west-north west, which it continues to be till after the appearance of the phenomenon, and goes on veering again till the tenth hour following, when it is north by west. This veering of the wind before St. Elmo's Fire is seen is well marked in all the cases.

The rainfall averages show two distinct maxima, the first between ten and six hours previous to the display, and the other at the hour St. Elmo's Fire is seen and the following. The latter maximum is wholly due to the heavy showers of snow and snow-hail that accompany the displays. This snow-hail differs from the usual flaky snow crystals in being of the shape of small cones with spherical bases, and being hard and dry. During the finer displays this kind of snow was always present.

Thus, as far as local observation goes, we see that St. Elmo's Fire is seen on an average six hours after the lowest reading of the barometer has been recorded, in a depression that occurs in a general low-pressure area; that it is preceded, accompanied, and followed by a falling temperature; that before it is seen the wind has veered considerably, and goes on veering for some time after its appearance; and that it is attended by heavy precipitation in the form of snow-hail.

The averages are of some interest apart from their connection with St. Elmo's Fire, for they show the relations existing in this class of storms between the four elements here discussed. The averages of pressure and of the direction of the wind are sufficient to show that the depression comes in from the Atlantic, and that the centre passes eastwards somewhere to the north of Ben Nevis. Then it is seen that the temperature is at a maximum when the barometer is falling, and is still falling when the barometer is at its lowest; that is, at a time when the atmosphere is very unstable and ascending currents at their strongest, the temperature is falling on Ben Nevis, a state of matters that must necessarily result in a rapid condensation of vapour, and a consequent copious precipitation. The rainfall averages quite agree with this.

As to the prevailing weather over the British Isles and west of Europe in general, about the times of St. Elmo's Fire being seen on Ben Nevis, the weather charts of the London Meteorological Office show that, in almost all the cases, the following conditions obtained—namely, somewhere to the south or south-east of the British Isles, usually over the south of France and over the Spanish peninsula, there was a distinct high-pressure area, or anticyclone; and that to the west or north of Scotland there was a low-pressure area, or cyclone. Between these two positions the barometric gradient was chiefly for south-westerly to westerly winds, and was usually pretty steep. The charts also showed that, so long as the anticyclone maintained its position to the south-east, so long did cyclones sweep in from the Atlantic with the above gradient wind, and pass our islands in a north-easterly or easterly direction. Thunder and lightning were noted in Ireland on several of the nights that St. Elmo's Fire was seen on Ben Nevis. Only on one occasion was any thunder and lightning observed on Ben Nevis, about the times of St. Elmo's Fire appearing, and then the phenomenon was seen two hours before the thunderstorm came on.

It might be inferred, from what has been said, that as St. Elmo's Fire appeared at the change of weather, when the centre of the storm had passed, it would be a good prognostic of improving weather. Such is not the case, however, for almost invariably another cyclone is approaching, and another spell of bad weather is experienced soon after the St. Elmo's storm has passed.

A. R.

NOTE.—Since the paper, of which the foregoing is an abstract, was written, several additional cases of St. Elmo's Fire have been observed on Ben Nevis. On one occasion we were fortunate enough in securing a photograph of the phenomenon, which shows St. Elmo's Fire as three small spots of white on the top of the kitchen chimney, the chimney being but very faintly seen.

TELESCOPES FOR STELLAR PHOTOGRAPHY.[1]

I.

I WILL ask you to remember that the subject of this paper is not that of the proposed international photographic survey of the heavens itself, but of the instruments which are to be used for that survey. No doubt a communication on the survey itself, dealing with the results aimed at, the conditions under which it is considered the best results may be arrived at, and the general scheme under which it is proposed to measure, define, and catalogue the position of the stars obtained, would be more generally interesting than one on the mere instrumental equipment; but this part of the subject has already been amply and most efficiently dealt with in lectures by Mr. Common and Dr. Gill at the Royal Institution, while the subject of the instruments to be used has only as yet been discussed in the more scientific and technical journals or proceedings of Societies; besides which, I may be pardoned for saying that I think when actual work is commenced, the perfection of the instrumental equipment will be found to be a larger factor in the attainment of success than has ever been the case in any previous astronomical research. There is probably nothing which surprises and excites the admiration of the modern astronomer more than the work done in bygone times by some of the older astronomers—work which was the outcome of marvellous patience and ingenuity while working with tools which would excite the pity and contempt of the merest tyro in astronomy of the present day; but while I am by no means a sceptic as to the most important part of all telescopes being "the man at the small end," I do believe that never before in any system of astronomical observing has "the man at the small end" been so completely dependent on the excellence of his instrumental equipment, a disarrangement of any one part of which would leave him utterly helpless. I trust, therefore, you will bear with me while discussing and describing a few of the more important mechanical details of these instruments.

You are aware, probably, that an International Congress of Astronomers was held last year in Paris, and that it was decided to start a number of Observatories, in various parts of the world, each to take its share in producing photographs of the heavens, to be afterwards used in compiling a general chart, in which stars down to the 14th magnitude would be entered.

Before we go further, it may be well to explain the difference between this system of charting and the old system, and what circumstances have led to this proposed revolution in astronomical work.

The system of mapping stars which has been used up to the present time consists, as you are aware, in observing all the stars *seriatim*, in a transit circle, or similar instrument, and tabulating their declination (*i.e.* angular distance north and south of the equator), or Polar distance, as found by readings of the vertical centre, and their right ascension (*i.e.* their distance measured on the equator from an empirically fixed point in the heavens), as found by the difference of time of the sidereal clock between the passage of the star across the centre wire of the telescope and that of the fixed point above referred to. The essential point in the above system which I have to direct your attention to is, that every single star has to be examined *by itself*.

The magnitude of the work of such a survey as the Paris Congress has decided upon may be inferred from the fact that there are probably some 20,000,000 stars to be examined and catalogued. It is a good year's work of a transit circle to tabulate 5000 stars; supposing, therefore, thirty or forty Observatories divide the work between them, the survey would still occupy over 100 years, and by that time the proper motion of the stars would render a new survey necessary.

Now, ever since photography has been practised, it has been the dream of the astronomer to photograph the heavens, and obtain, at one and the same time, the positions not of one but of hundreds, or perhaps thousands, of stars in each operation. But then, it may be asked, why was not photography employed long since? The answer is, that until recently the amount of sensitiveness obtained was not sufficient to allow of the fainter stars impressing the plate within reasonable time, and consequently it was found impossible to produce satisfactory stellar photographs, except of the larger stars.

Dr. Warren De la Rue was the first to point out the use which might be made of photography for the purpose of star-charting, and, as far back as 1860 and 1861, produced photographs of star-clusters, &c.

In 1864 or 1865, Rutherford, of New York, obtained photographs of the larger stars, and while photographing the moon with the great Melbourne telescope in 1867 I took, for the purpose of adjustment, some photographs of "Castor"; but in an article which I wrote for the "British Journal Photographic Almanack" in 1869, I pointed out that for the development of celestial photographs we would have to look to the chemist and not to the optician—in other words, that until we obtained more suitable plates we could not expect much advance. This has proved to be the fact, for with the advent of the gelatine plates, and consequent increase of sensitiveness, celestial photography received that impetus which has eventuated in this proposition of an international photographic survey. I also pointed out in that same article nineteen years ago that if by any possibility the exposures could be reduced so far as to render the unsteadiness of the image insensible—a rapidity which I said there was no reason to suppose might not be obtained in the case of the sun—we might expect great results, a prediction which has since been verified by Prof. Jansen's magnificent pictures of the sun, with which you are all familiar.

It is almost superfluous to remind you also of the magnificent picture of the nebula in Orion by Mr. Common, an example of celestial photography never yet surpassed.

In 1882, Dr. Gill sent home to the Royal Astronomical Society, and to the Paris Academy of Sciences, a photograph of the great comet of that year, and called attention to the large number of stars photographed on the same plate. This photograph was obtained with an ordinary photographic lens and camera attached to a clock-driven equatorial.

It was this, perhaps, that influenced the Paris Observatory to construct the photographic telescope of about 13 inches aperture, 11 feet focal length, specially corrected for the chemical rays, with which the splendid star charts of the Messrs. Henry were obtained. Meanwhile, others were not idle, and while Dr. Gill, through the munificence of Mr. Nasmyth, obtained a 9-inch achromatic, which I corrected of course for the chemical rays, Mr. Roberts, of Liverpool, had a 20-inch reflecting telescope constructed for the same purpose.

It soon became evident that this new departure in the system of star-charting was likely to be of very great importance, and consequently an International Congress of Astronomers assembled last year in Paris to discuss the whole question. This Congress defined the size and focus of the object glasses to be used, and laid down a certain standard for the correction of the chromatic aberration suitable to the nature of the work; but left almost all other points free for individual astronomers to deal with as they thought best. In fact, the Congress wisely defined only just such points as were necessary for securing uniformity in the scale of the photographs, all of which it is proposed shall be sent to some central bureau to be examined, discussed, and made use of in compiling the chart.

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[1] A Paper read by Sir Howard Grubb, F.R.S., before the Society of Arts, on April 18, 1888.

But it may be asked where is the great difference between this system and the old, as the positions of the star images on these photographs have to be measured *seriatim*, just as the stars themselves have to be measured *seriatim* in the old system. This is no doubt true, but under what different conditions are the measurements made.

None but those who have worked in this field know the labour represented by a volume of 5000 star places under the old system.

How many times does that wretched bit of cloud come across the field just as the star reaches the centre wire of the micrometer, and how many nights, beautiful and clear as they may look to ordinary individuals, prove utterly worthless for observing purposes; but under the new system, once a single good plate has been obtained, there is a permanent record of some hundreds, or in some cases thousands, of stars, which can be measured at leisure by day or by night, in good weather or bad weather, and in comfort in your office or study, and there also is that photograph as a permanent record which can be referred to at any time as a check on errors which might possibly creep in to some of the final reductions.

In fact, this new system gives us the means of taking advantage of the very few really favourable opportunities of observing, and of producing, during those favourable moments, a *facsimile*, so to speak, of any portion of the heavens which we can examine and survey at our leisure without any of the difficulties or discomforts attendant upon direct astronomical observations, and under conditions far more favourable to the obtaining of accuracy in the results.

Let us now consider what are the chief points to be attended to in the construction of the instrument. It is evident that what is required is an instrument which—

(1) Can be accurately pointed at any given object.

(2) That when pointed in the desired direction the clockwork shall cause it to follow that object as steadily as possible.

(3) That as the meridian is the best position for observation, the instrument should be capable of working for some distance on each side of the meridian without reversing.

(4) That means should be supplied by which the observer can watch and verify the accuracy of the clock-driving, or making any change in position rendered necessary by refraction.

The first point involves delicate and accurate slow motions in right ascension and declination.

The second involves great steadiness and rigidity of the mounting, great smoothness of motion of the polar axis in its bearings, and above all most accurate clock-driving motion.

The third point involves either the adoption of the old English form of mounting, or a modification of the German form, as the latter does not generally allow of motion for any considerable extent beyond the meridian without reversal.

The fourth merely involves a very powerful finder—or, rather, guiding—telescope with suitable micrometric eye-piece arrangements.

Respecting the relative merits of refractors and reflectors for this purpose, I shall speak just now; for the present I wish to direct your attention to the instrumental part only, and, for the better understanding the peculiarities of the various mountings, I will now throw photographs of some of them on the screen.

The first illustration is that of the Paris equatorial, with which the well-known and deservedly praised star pictures of the Messrs. Henry were produced. You see it is of the construction generally known as the "English" equatorial,—a long split polar axis, with bearings for carrying the telescope at the centre (its weakest point). This form of equatorial has many advantages, which at first seem to render it peculiarly fit for this special work. With good slow motion, and a large finder telescope, it admirably fulfils the first, third, and fourth of the above conditions; but, owing to its peculiar form, and the difficulty of using a driving sector of long radius, it is not well calculated to fulfil the second and most important of all conditions, viz. the accurate following. No doubt excellent work has been done with it by what is called the "eye and hand" guiding. That is, the star is watched in a powerful finder, and when it is seen to err sensibly in position, it is brought right again by the slow motion handle. I think it is generally allowed that the undoubted excellence of the work done by the Messrs. Henry is due more to extraordinary patience and skill in the "eye and hand" guiding than to any unusual perfection of the clock-driving. This form of instrument also possesses a disadvantage in being very difficult to arrange for work near the Pole.

The second illustration is of Dr. Gill's 9-inch photo telescope, with which he took his star pictures. There is nothing peculiar about the mounting of this, being in fact an old equatorial which I sent him some years since. In this case also the excellence of his results is to be attributed to the skill of the observer, and not to any inherent excellence of the clockwork; but that excellent work has been done with it is apparent from the perfection of some of his photographs.

The next illustration is that of the telescope of Mr. Isaac Roberts, of Liverpool, for whom I mounted a 20-inch reflector to experiment with. As you see, it is mounted on what I call the twin form, as I mounted Dr. Huggins's instrument, by the adoption of which the observer always has a second telescope available for visual work, if anything interesting should appear in the heavens. With this instrument Mr. Roberts, working in the exceptionally wretched atmosphere of Liverpool, has secured some most admirable work, a few specimens of which I now throw on the screen.

This is the first instrument, as far as I know, in which a successful attempt has been made to drive, for any considerable time, without "eye and hand guiding." The special system of clockwork used I will describe further on. So far as the general form of the instrument is concerned, it would appear that the balance of advantage lies with the German form (so called) — that usually adopted in this country. It is capable of being made of great stability, and it admirably fulfils all the conditions except the third, but that also, by a little modification, can be managed.

The next illustration shows the general form which I have adopted, the principal feature of which is great stability. The stand being cast all in one piece contributes to this, but the peculiarity of the system of equipoise probably has more to do with it. In designing these instruments, I proposed that the fulcrum of the levers which support the greater part of the weight of the polar axis, should be attached, not to the frame of the instrument, but to an independent pillar, so that only a very small portion of the weight of the moving parts should be carried by the main framing. Dr. Gill then proposed that I should also allow the levers to act in a purely vertical direction, instead of, as usual, in a direction at right angles to the polar axis, and to let the point of support be vertically under the centre of gravity of the whole moving part. This I have carried out, and the result is that only as much of the weight of the whole instrument as is necessary to insure steadiness will rest on the bearings (lateral or end bearings) of the polar axis; all the rest is transferred to the base of the stand.

We now come to consider the all-important part of the photographic equatorial—that is, the driving clock.

All clocks used for driving equatorials (which must of course move uniformly, and not step by step as pendulum clocks), may be divided into two classes—(*a*) those in which uniform motion is obtained, or sought to be obtained, by some variable friction or resistance which increases as the speed increases; and (*b*) those in which some such similar contrivance is supplemented by a system of electric control from a pendulum clock, which is itself incapable of being re-acted upon by the uniform motion clock.

For all ordinary observing, and even for micrometric work, clocks of the class *a* are made, which answer admirably, but for photographic equatorials I believe it will be found necessary to employ clocks of the *b* type, and for this reason: the tendency of the compensation in uncontrolled clocks (class *a*) is to correct the *rate* of the clock when from any momentary cause it is disturbed. The best it can do is to bring the *rate* absolutely right again, but it cannot act till an error has actually occurred, and therefore, although the rate is corrected, the *position* of the star on the plate is shifted by the amount of the error. I have heard it stated by the designers of some of these clocks that errors were corrected *before* they existed! It is hardly necessary to stop to show the fallacy of this, but it is evident that the increased or diminished resistance, or friction, or whatever it is, that checks the speed, can only exist from, and in consequence of, the error itself.

In the case of micrometric measures, it is not of very much consequence if a minute error occasionally creeps in, provided the speed keeps constant during the few seconds or minutes required for making the bisections, but in the case of the photographic telescopes, if the image of the star takes up a new position any time during the exposure, it is of course fatal.

Let us try now and get an idea of what amount of accuracy is really necessary for this work. We often hear of a perfect equatorial clock, but the word perfect is, I fear, as loosely used in this connection as in others.

I have heard in days gone by a perfect clock defined as one which drove the instrument so accurately that if you set the telescope on a star and went to dinner you would find the star still in the field when you resumed your observations after dinner. Allowing, say, two hours between the ante-prandial and the post-prandial observations, and assuming the eye-piece to be (as we may fairly do) a low one of about 20′ of arc field, this would mean that the clock did not vary more than 600″ of arc or 40 seconds of time an hour. The accuracy we now require for these photographic telescopes is something very different. The image of a 12th magnitude star impresses itself on the plate, with moderate exposure, in the form of a circular disk of about $\frac{1}{700}$ inch diameter. If the clock vary one-tenth of a second during the exposure, the disk will be elongated by $\frac{1}{1000}$ inch, producing a very sensible distortion.

We must not therefore have any errors over one-tenth of a second, and if possible it should be reduced to one-twentieth.

It will not be necessary that the clock keep within this one-twentieth of a second for more than ten or fifteen minutes, because it is always necessary to watch the image occasionally through the guiding telescope, and correct whenever refraction becomes apparent; but what I do urge as absolutely necessary is that the clock shall go so perfectly as not to require more than the occasional attention of the observer, instead of the constant and never-ceasing watching with ordinary clockwork. No one who has not tried it can imagine the strain required to keep a constant watch on a star image for 30 or 40 minutes, but if attention be only required for a second or so every few minutes there is no difficulty or irksomeness whatever.

Even the most enthusiastic admirers of various forms of equatorial clocks will not venture to assert that they will go for fifteen minutes without one-tenth of a second of error. There is now, however, no difficulty in controlling a uniform motion clock from a pendulum so that it will never vary one-twentieth of a second from it. It may therefore, I think, be assumed that some form of electrical control is necessary. There are, as far as I know, four forms of control to choose from.

First, Dr. Gill's, as applied to the 15-inch equatorial at Dun Echt with admirable success.

In this an electric current is sent once a second from an independent pendulum, which may be any distance away. That current passes through a certain wheel in the clock, with contacts so arranged that if the clock be going exactly with the pendulum the current is sent in a direction which keeps one of two rubbers rubbing on a quick-moving wheel of the clock. If the clock, however, goes the least quantity too fast, the wheel has revolved a little further than it should at the moment the next current comes from the pendulum, and the current is sent in such a direction as to cause both rubbers to rub on the clock wheel. If, on the contrary, the clock has gone a shade slower, the current is sent in a third direction, which lifts both rubbers off. This control, so far as it goes, acts almost perfectly, but it is open to this objection, that as it only corrects the errors of whatever shaft in the clock the contact-wheel is attached to, any error in wheels between that and the telescope screw are unaffected by it; also I find in practice that when it is attempted to control a clock by alteration of friction, on any heavy quick-moving part, it takes some little time to act, and then generally overdoes the correction, causing what is generally termed "hunting." The second form of control is the first which I introduced.

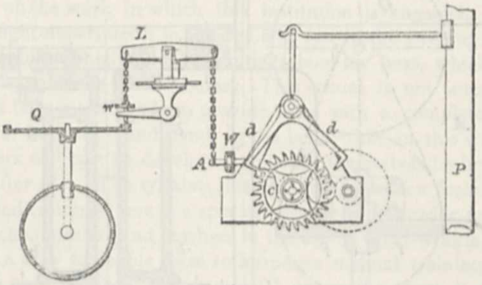

Fig. 1.

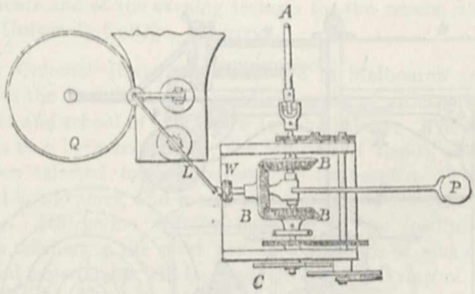

Fig. 2.

Fig. 1 is an elevation, and Fig. 2 a plan, of the arrangement which is attached to the back of the main clockwork, and can be seen in Fig. 3 at E, but on too small a scale for description; A is a portion of one of the uniform clock motion spindles, or any shaft coupled thereto; B, B, B, are the three wheels of an ordinary mitre *remontoire* train driving by weights, W, the scape-wheel, C, into the teeth of which gear the pallets, D D, which pallets are driven by the electric pendulum, P.

The electric pendulum is connected to and driven by a current from any independent clock. To the weight-carrying arm of the *remontoire* is attached a small chain or wire, which communicates any motion it may have to the lever, L, from the other end of which lever hangs a weight, *w*, smaller than W, which weight is therefore raised when the *remontoire* arm is lowered, and lowered when the *remontoire* arm is raised; Q is a disk of metal on a vertical spindle of a uniform motion clock, and revolving rapidly (say 300 per minute). When the weight, W, is below its mean position, it is in contact with the disk Q, and (the lower end of it being coated with leather) produces a considerable amount of friction, and therefore tends to retard the speed of the clock; when the weight, W, is above its mean position, it is altogether out of contact with the disk Q. The action is as follows: Supposing the shaft, A, to be revolving exactly one per minute, the pendulum to be vibrating exactly 60 per minute, and that there are 30 teeth in the scape-wheel, it is evident that the *remontoire* arm, and therefore, the weights, W and *w*, will vibrate backwards and forwards the same distance each second, and that the mean position of all will be the same each second. Under these circumstances, the weight, W, will be alternately 0·5 second in contact with the disk Q, and 0·5 second out of contact, and the uniform motion clock is rated, *per se*, just so much fast, that the resting of the weight, W, for 0·5 second in each second, will bring the rate right.

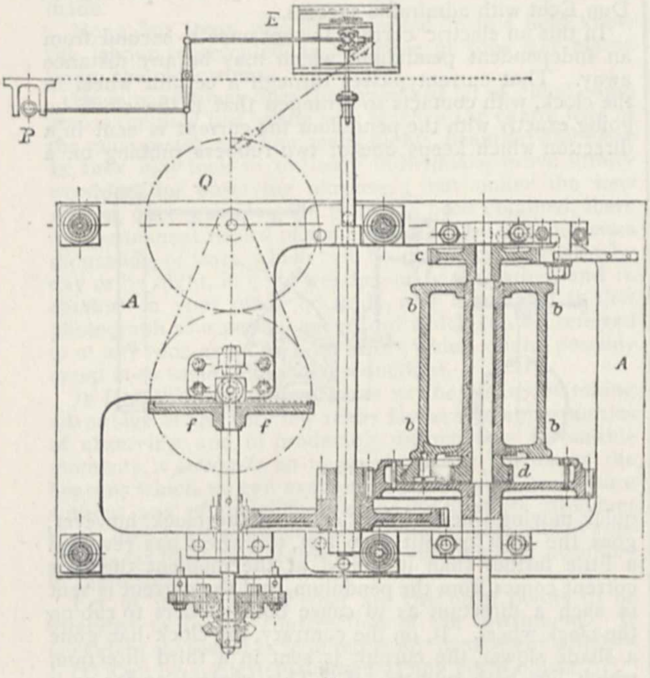

FIG. 3.

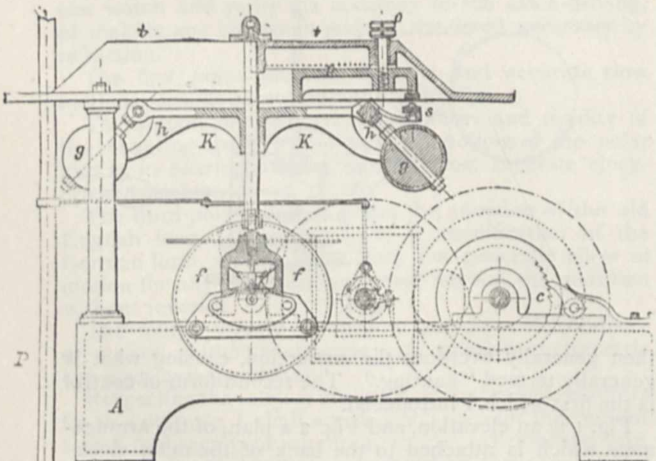

FIG. 4.

Now, suppose an error of acceleration to arise in the uniform motion clock, the mean position of *remontoire* arm will rise; therefore W will fall, and, instead of rubbing in contact with Q for 0·5 second, it will rub for 0·6 or 0·7 second, according to the extent of the error. This will tend to check the rate, and this check will continue till the relative position of the uniform motion shaft becomes as it was when the clock was going correctly.

If a retardation occurs, the reverse effect will take place, and the weight, W, will rub only for 0·4 or 0·3 second, instead of 0·5, until the error be corrected.

So far as described, there was no particular novelty, as most of this arrangement, in principle, had been tried before, the failure that had resulted being due to the fact that it was found impossible to prevent the pendulum being influenced by the difference of force on the pallets, under varying circumstances, the pendulum being in the former case driven by the scapement; not by electricity, as in this case.

This difficulty was got rid of by:—

(1) Making the pallets (as they are not required to drive the pendulum) of such form that the teeth of the scape-wheel impinge upon them nearly at the angle of repose.

(2) By driving the pendulum by electric current from another clock, thus virtually rendering the pendulum not a pendulum at all, but a lever worked backward and forward by electricity, and not subject to alteration in its rate by slightly varying force on the pallets.

An arrangement is also attached (but not shown in the figure, to avoid confusion) by which, if either portion of the clock fails to do its duty from want of winding, want of electric current, or other cause, the connection between the two systems is instantly severed, automatically.

(*To be continued.*)

NOTES.

MR. GRIESBACH, of the Geological Survey of India, who has lately been engaged in geological work in Afghanistan, and who was geologist to the Afghan Boundary Commission, has been appointed to Beloochistan, "to carry on geological investigations into the mineral deposits of the country."

AT the meeting of the British Association at Newcastle there will be a joint discussion by Sections B and G of a paper to be read in the latter Section by Sir I. Lowthian Bell, F.R.S., President of Section B, on "Blast Furnace Practice."

THE International Astronomical Congress will hold its sittings in Brussels from the 10th to the 12th inst., and at Liége on the 13th. The attendance from all European countries and America will, it is anticipated, be very large.

IT is reported from Brussels that M. Dutoit, the Transvaal Minister of Public Instruction, is now endeavouring in Belgium and Holland to recruit the necessary staff for organizing a Dutch University in Pretoria, the capital of the Transvaal.

COLONEL THUILLIER's report on the progress of the Survey of India for the past year shows that the party employed on the trigonometrical surveys has completed the 370 miles remaining of the secondary triangulation along the east coast of India. The secondary triangulation was also carried out for an aggregate length of 270 miles by parties employed in Beloochistan as a basis for topographical surveys in that region. The work of the geodetic party comprised measurement of seven arcs of longitude in Southern India, and the tidal survey party continued its observations with self-registering tide-gauges at several stations along the coast, where tidal observatories are established, and connected with the operations of spirit levelling. Geographical surveys have been carried out vigorously in Upper Burmah, nearly 21,000 square miles having been surveyed and mapped on a half-inch scale. Reconnaissance along the Nepal boundary has supplied a rough basis for a more accurate and detailed survey of the northern frontier when an opportunity offers. Interesting additional information regarding Bhootan and Tibet has been obtained from the adventurous travels of native explorers trained and sent thither by the Department. Of the new maps 4062 were published during the year, and heavy demands continue for trans-frontier maps, and maps of Upper Burmah. The photographic and lithographic offices show the large out-turn of 1,203,861 copies during the year, including high class illustrations for archæological and other reports.

A RECENT mail from New Guinea brings information that the Italian naturalist Signor Lorie had landed in that country, and was proceeding to carry out arrangements for a scheme of scientific investigation which he proposed to himself. His present intention is to remain for several years in New Guinea, where he intends, in addition to following his favourite scientific pursuits, to devote some of his time to exploration work. Signor Lorie is described as a great enthusiast, and a man of determination and courage.

THE *American Meteorological Journal* for August contains an instructive article, by Prof. W. Ferrel, on decrease of temperature with increase of altitude. The author reviews the cases of rapid decrease which would occur were the atmosphere without aqueous vapour and in a stable state; of the very low temperature that would exist a little above the earth, if there were no atmosphere; of the low temperature of the upper atmosphere, owing to radiation into space, if the earth were surrounded by a clear atmosphere, not heated by the solar rays. The very rapid decrease of temperature with height is prevented by the ascending currents, caused by unstable equilibrium, and by the heat of condensation given out after the vapour has ascended to the altitude where condensation commences. The average vertical gradient is less in the cloud region than in the lower strata of the atmosphere, and less in the lower strata in cloudy than in clear weather, as shown by the results of Glaisher's balloon observations. He also refers to the more frequent unstable state of the atmosphere in spring and early summer, owing to the lower strata at that season being warmed up faster than the upper strata; in the fall of the year the unstable state is not so readily produced, and more settled weather prevails. Mr. H. H. Clayton has a paper on diurnal and annual oscillations of the barometer. It is pointed out, in the report of the expedition to Lady Franklin Bay, that if the diurnal pressure at five Arctic stations differing largely in longitude be plotted in simultaneous time, the epochs of maxima and minima show a striking coincidence with each other; the author traces the probable cause of the occurrence of the maxima to the expansion and overflow of air from Asia and America to the Pole; and of the minima at the Pole to the fact that the outflow from the Pole towards those continents is not replaced by an influx in that direction from the oceans. The retardation of the annual maximum from the Arctic region to the Equator, and of the minimum from the southern parts of the continents to the Arctic region, is also attributed to the relative heating and cooling of the continents and oceans. The remaining articles are: on the mineral waters of Gratiot County, Michigan, by Dr. Brainerd, giving an analysis of the waters and the virtues of the substances contained in them; the State tornado charts of Kansas and Indian Territory, by Lieut. Finley; a translation of a paper, by Saussure in 1796, on the use of the sling psychrometer, from which has resulted the modern use of that instrument; and, on atmospheric economy of solar radiation, by A. Searle, with comments by Prof. Ferrel (the principal point is that the earth is kept at a higher temperature than it would be, owing to the fact that heat is transferred upwards by conduction and convection instead of by direct radiation).

THE syllabus for the year 1889–90 of the Manchester Technical School has been issued. It shows the importance and magnitude of the work in which this institution is engaged. There are eight departments in the day and ten in the evening school, beginning with a manual training school for boys, which is the beginning of the whole course. This school is not intended to teach boys a trade, but to provide them with a complete education of both head and hand, in the belief that in this way their powers will best be developed and they themselves be best fitted for after-life. The syllabus this year has some new features. In manual training there is a special course on Saturday mornings for schoolmasters and teachers in the use of wood-working tools, with a view to enable them to introduce manual training among their own pupils. In commercial geography there is to be a special course of lectures of a practical character. Type-writing is another novelty. The chief feature is instruction in the use of the Remington and Caligraph machines. With a view to obtain the necessary skill, each student has three hours' practice per week. There is a special class for women in shorthand, in addition to increased facilities for male students. A special honours class has been formed for evening students in magnetism and electricity, telegraphy, and electric lighting. Especially it should be noted that facilities are offered for a comprehensive study of commercial subjects. There are now great facilities to evening students for the study of science and of art in their application to all the more important industries. The school staff consists of 52 teachers. Last session the school was attended by 3328 individual students.

WE have received the prospectus of the day classes in arts and science and of the evening lectures for the session 1889–90 of the University College, Liverpool.

THE Colonial Board of Viticulture in Melbourne recently proposed the establishment at public expense of an experimental vineyard and school of viticulture for the colony. The suggestion has now been accepted by the Minister of Lands, and a site has been selected for the purpose at Rutherglen. The area selected is 200 acres, and it will be permanently reserved for the purpose. Instruction will be imparted at the institution by capable teachers in the most approved methods of vine cultivation, and experiments will be conducted with the view of testing the value of new plants said to be suitable for growth in Victoria. Funds for conducting the school will be provided in the present year's estimates, but pending the formal vote the Minister has authorized the expenditure of a sum sufficient to at once plant 20 acres of the reserve, and so expedite the work. This will enable the Board of Viticulture to take advantage of the present favourable season.

IN his last Report, the British Vice-Consul at Nisch mentions the terrible havoc which is being made by disafforestation in Servia since its independence. He says that during the Turkish occupation Servia was covered with magnificent forests of oak, beech, chestnut, and walnut trees, by means of which the country was assured of a regular and plentiful supply of water, and in the recesses of which the natives found shelter and refuge from their foreign conquerors. From the date of her independence a destruction of these invaluable treasures commenced which has been carried on with remorseless and unreflecting perseverance, and it appears as though there were at the present day a race against time to complete the havoc. From time to time the consciences of Ministers and Governments have roused them to interfere, but, beyond passing laws which remain a dead letter, hardly anything has been done to arrest the evil. Floods in winter and drought in summer were declared by Mr. Borchgrave, in 1883, to have already begun to exact the penalty which carelessness or want of foresight must be called upon to pay; but the peasant and his goats continue their work of destruction, whilst the authorities are apparently more anxious to avoid occasions of discontent which restrictive measures would create than of applying such remedies as legislation has placed in their hands. Whole mountains may be seen completely denuded of timber, with the exception of a low worthless scrub, which were, a few years ago, covered with woods, but which have fallen victims to the innumerable herds of goats which are allowed to browse at will. The peasants amongst whom the land was divided at the time of the Servian independence have cleared vast tracts for the purposes of agriculture, and possess the right of cutting timber for firewood in those forests which are under the management of the different *communes*. Very little coal is used for household purposes, and the amount of wood required for daily consumption adds enormously to the drain on the national resources. The best-wooded parts of Servia are the districts of the south and south-east, but especially the department of Toplitza, which may be said to contain the only remaining virgin forests of Servia, and whence are annually drawn large supplies of walnut trunks and oak staves for casks. The heights of the Nischava Valley, Stalatz, and Krushevatz furnish excellent building timber. Oak forests are abundant on the Turkish frontier of Vrania. Walnut trees, which attain to an enormous growth, have been mercilessly dealt with, the value of this timber having attracted the attention of Austrian merchants, who send agents to choose and cut the wood for exportation. The fir and juniper are found in the central and western valleys, and on the great Kopavnik Range on the south-east, the pine on the heights of Zlatibor.

HERR TROGNITZ contributes to the last issue of *Petermann's Mitteilungen* the results of calculations which he has made of the areas of the various South American States. These are based on the maps in the latest edition of Stieler's Atlas, which are compiled from the latest official and other information. The figures are in square kilometres.

Brazil	8,361,350
French Guiana	78,900
Dutch ,, ...	129,100
British ,, ...	229,600
Venezuela	1,043,900
Columbia	1,203,100
Ecuador	299,600
Peru	1,137,000
Bolivia	1,334,200
Chili	776,000
Argentine	2,789,400
Uruguay	178,700
Paraguay	253,100
Total	17,813,950

THE *Colonies and India* reports from Tasmania that a movement is on foot in Hobart for the creation of a University. A notice of motion directing attention to the desirability of such a step was given in the Council of Education by the Minister of Education, and the subject was to be considered at a meeting on June 19. "We hope to hear that it has been favourably considered, as the colony is quite prosperous enough to maintain such an institution, and it would be one which would be extremely useful, as well as being to some extent an adornment to the picturesque capital of the Island Colony."

MR. G. W. ROOSEVELT, American Consul at Bordeaux, in a report on the treatment of diseases of vines in France, says that in spite of the numerous inventions meant to destroy Phylloxera, it still continues its ravages. One of the most recent plans is that of an American, Mr. L. H. Davis, who inoculates the vine, through a carefully made excision, with a preparation which he claims is destructive to the Phylloxera, while it leaves the vine uninjured. It is too soon yet to speak of the results of this plan. Dr. Griffin advocates a distribution by a machine constructed by him of a substance which can be used in either a dry or a liquid state. Last spring he operated on a vineyard placed at his disposal by the French Government, and had the satisfaction of seeing the vines treated by him sound and healthy, while other plants in the same vineyard were perishing. The most generally employed remedy has been found to be very serviceable, and free from the danger that was thought to follow it—that is, the submersion for not less than forty days in carbon of sulphur dissolved in water. In light permeable soils a strong mixture is used, but on hard soils a weaker solution is better. Within the past few years the actual area of the vines destroyed by this pest is 1,200,000 hectares, or, roughly speaking, one-half of the vineyards of France; and if we remember that a hectare of vines is worth about 6000 francs we can see what a terrible loss France has suffered. In the case of Oïdium, as in that of Phylloxera, no positive remedy has yet been discovered, but the usual mode—that is, the application of sulphur, pure or mixed—checks the disease, and at the same time helps the growth of the vine. In fact, so great have been the good results of the use of sulphur, that it will for the future be used in most vineyards, even where Oïdium does not exist. Till the year 1885 no remedy was known for mildew. Since that year, however, salts of copper have been successfully employed, though there is some doubt whether that substance is really beneficial to the vineyards. The most general method is to pluck off the diseased leaves and burn them. Besides these there are other methods, such as the use of *bouillie bordelaise*, *eau céleste*, ammoniate of copper, and verdigris with powdered sulphate of copper. On account of the recent appearance of the disease called black rot, no satisfactory remedy has yet been tried. With regard to anthracnose, if steps are taken early in the spring, the disease may be brought under control. Perhaps the best remedy is a mixture of lime and sulphur. A first sulphuring is given when the shoots are four or five inches long; then, if lesions appear, the operation is repeated in about a fortnight with a mixture of lime and sulphur, the proportion being one part of sulphur to three of lime. A mixture of plaster and sulphate of iron has also been very successful. The only really efficacious remedy for pourridie is by removing and burning all roots showing traces of the disease. Erinnose may be treated like mildew—that is, by repeated applications of sulphur.

A REPORT on the appearance of the Hessian fly in this country, by Mr. Charles Whitehead, the Agricultural Adviser, has been issued by the Agricultural Department of the Privy Council. The fly was first seen in 1886 in Great Britain, and in that year did some harm to wheat and barley plants in England and Scotland. In 1887 it was noticed in twenty counties in England and ten in Scotland, wheat and barley crops being considerably damaged by its action. The weather during the summer of 1887 was hot and dry, like that which normally prevails in America, and was presumably favourable to the development and progress of the fly. During 1888, when the summer was unusually wet and cold, very little was heard or seen of the Hessian fly either in England or Scotland. But during the early months of the present year the temperature was high and the rainfall small, and from the reports received by the Agricultural Department the infested area has largely increased in England. In Scotland it does not appear to have made so much progress. Still it is present in many Scotch counties. The actual amount of injury to the crops is slight, and, so far as can be ascertained, is not in any instance so important as that caused in some cases in 1887. It is most probable that the injurious operations of the insect have been checked by the wet, cold weather which has followed the abnormal heat of May, and the warmth and dryness of June. When a cycle of hot summers occurs, it may happen that the ravages of the Hessian fly may be general and calamitous. Mr. Whitehead therefore urges the desirability of careful watching and the prompt adoption of simple methods, which he describes, for preventing the increase of the pest.

THE *Industrie Textile* has a long account of the treatment of wild silks (that is, those which are furnished by silkworms other than those of the domesticated *Bombyx mori*) in their native countries. In India there are no less than fifty varieties of silk-bearing insects, the most important of which is called tussur, that is, "the weaver's shuttle." The caterpillar, like the moth, is of a great size, and feeds upon more than thirty species of plants. The cocoons of the tussur, which make their appearance twice in the year, are found attached to the branches of trees in the jungle in large oval masses. The caterpillar lives from thirty to forty days, and then weaves its cocoon. In four or six weeks from this time the moth comes out and lays eggs from which comes a second generation of caterpillar. These wrap themselves in the cocoon, and remain hanging to the trees throughout the rainy season—that is, for seven or eight months. The cocoon, which is about four times the size of that of the mulberry silkworm, is composed of a double and interrupted thread of about 1400 metres in length. The thread is impregnated with uric acid of sodium, which must be removed by the aid of an alkaline wash before the thread is unwound. The tussur is tended with great care; in fact, for centuries various religious usages have been employed in rearing it. The moth, which is a large insect of a brownish colour, having its wings beautified by four transparent eyes, is venerated, and may be only approached by people of a certain caste. Unlike the tussur, which has been domesticated in India for some thousands of years, the cocoons of the other species are collected in the jungle. Amongst these is the *Attacus cynthea*, which feeds on the castor-oil plant, and of which the cocoon is white. Other species are the *Antheræa assama* and the *Cricula trifenestra*, which lives on the mangrove tree and spins a cocoon of a bright golden colour. The most important Chinese species is the *Antheræa pernyi*, which is cultivated in the province of Sze-chuan. In China also is found the most beautiful of all moths, the *Attacus altas*, which spins an enormous cocoon, covered at both ends with a very thick silk, known as Fagara silk. In Japan are the *Ailanthus* caterpillar, and the *Yamanai*, which till lately was reserved for the exclusive use of the Mikado, and the exportation of the eggs was an offence punishable with death. At present attempts are being made to cultivate this species in France, and it is believed they will be successful.

MESSRS. BLACKIE AND SON will publish immediately a translation of the well-known "Organische Chemie" of Prof. Bernthsen, of Heidelberg. The translation is by Dr. George McGowan, University College, Bangor, and the original text has been specially brought up to date for this edition by the author, who has throughout shown keen practical interest in the perfecting of the English edition.

THE Royal Meteorological Institute at Utrecht has just published a valuable atlas of twenty-two charts, containing the results of observations in the Indian Ocean, for the separate months of December, January, and February. Some portions of the ocean are naturally without data, but the amount of material dealt with may be judged of from the fact that 51,799 observations have been used in the construction of the wind chart for December. The temperature of the sea-surface is represented by isotherms drawn for every 2° C., and shows in certain regions, *e.g.* near the Cape of Good Hope, considerable differences of temperature in the various months. The currents are plotted in 1° squares, in two colours, showing the resultants of the currents setting towards the west and of those towards the east, separately, together with the number of observations from which they are calculated. Atmospheric pressure is represented by isobars drawn for every 2·5 mm. They show an area of high pressure between lat. 30° and 35° S. and long. 80° and 90° E.; in January there are two centres of high pressure, at long. 55° and 85°. The temperature of the air is exhibited by isotherms for every 2° C. The difference between the air and sea temperature is generally small, but the excess of the latter sometimes amounts to 5° C. The wind is represented by roses showing the relative frequency for direction (only), arranged according to homogeneous areas, combining the same prevalent winds in irregular spaces. Other charts show specific gravity, rain, percentages of storms, and other interesting information.

THE third session of the Edinburgh University Extension Summer Vacation Course was held, as before, during August, at Granton Marine Station, through the kindness of Dr. Murray, Director of the *Challenger* Expedition, and of Mr. Irvine, of Royston. The courses of botany and zoology were conducted, as last year, by Prof. Geddes and Mr. G. A. Thomson. This year each course was divided into an elementary and an advanced section, the former dealing with Vertebrates and Phanerogams, the latter with Invertebrates and Cryptogams. Prof. Geddes also delivered a course of twenty lectures on sociology. Some twenty-five or thirty students attended. All the courses were supplemented by demonstrations in the field and on the shore, and by visits to public and private gardens and to the Museum.

IN the abstract of Dr. Stefan's paper on "Ice Growth" (NATURE, August 22, p. 400), the third paragraph should read as follows:—"The theory gives for the thickness (h) of ice formed in the time (t) the following formula, which is approximately correct—

$$h^2\left(1 + \frac{cf}{3\lambda}\right) = \frac{2k\mathrm{T}}{\lambda\sigma}.$$

In this formula (c) is the specific and (λ) the latent heat of ice; (k) is the coefficient of conduction, (σ) is specific gravity, (f) is the temperature at the surface of the ice at the time (t), (T) the sum of cold for the same time; the last is the sum of the temperatures counted downwards from 28° F., or the freezing-point of sea-water, from the commencement of ice formation up to the time (t)."

THE additions to the Zoological Society's Gardens during the past week include a Common Peafowl (*Pavo cristatus* ♂, white variety) from India, presented by Mr. Richard Hunter; a Manx Shearwater (*Puffinus anglorum*) from Essex, presented by Mr. J. M. Wood, C.E.; a Lesser Razor-billed Curassow (*Mitua tomentosa*) from Guiana, presented by Mr. G. H. Hawtayne, C.M.Z.S.; a Louisianian Meadow Starling (*Sturnella ludoviciana* ♀) from North America, presented by Mr. Newton Hayley; a Turtle Dove (*Turtur communis*), European, presented by Mr. C. W. Cousins; five Herring Gulls (*Larus argentatus*), British, presented by Sir Richard Nicholson.

OUR ASTRONOMICAL COLUMN.

YALE COLLEGE OBSERVATORY.—The report of this Observatory for the year ending June 1889 has recently appeared. Mr. Brown, the Secretary, records the carrying out of several improvements in the grounds of the Observatory, and the continuation of the work of the Thermometric Bureau, 7475 thermometers having been received for verification during the year. Dr. Elkin, the astronomer in charge of the heliometer, completed the measures for the triangulation of the region near the North Pole during the summer of 1888, and the necessary reductions are well advanced. In October 1888 a series of observations for the parallax of Iris was commenced in connection with similar series to be effected at the Cape and at Leipzig, but measures were only obtained on thirty-four instead of sixty-five nights. In addition to these, however, 168 sets, each consisting of sixteen pointings, were obtained by Messrs. Elkin and Hall, for the diurnal parallax of the same planet. The discussion of the whole series of measures has been undertaken by the Yale astronomers, and the work has already been commenced. A series of measures for the parallaxes of Victoria and Sappho are now being undertaken, and it is expected that two additional observatories, those of Bamberg and Göttingen, will co-operate in the work. The heliometer has also been employed in further researches on stellar parallax; Procyon and Altair having been taken up by Mr. Hall, Vega and Regulus by Dr. Elkin. During the winter Mr. Hall completed the reductions of his work on the orbit of Titan; whilst Dr. Elkin took part in the observation of the total solar eclipse of January 1, 1889, which he observed from Winnemucca, Nevada, under very favourable circumstances.

NEW MINOR PLANET.—A new minor planet, No. 287, was discovered by Prof. Peters at the Clinton Observatory, on August 25.

Should the two planets discovered on August 3 both be confirmed as new bodies, that discovered by M. Charlois will be No. 285, whilst Herr Palisa's will be No. 286; the former having been discovered at 10h. 46m. G.M.T., and the latter at 11h. 27m.

COMET 1889 *d* (BROOKS, JULY 6.)—Dr. K. Zelbr has found elliptic elements for this comet, with a period of 12⅓ years. The ephemeris from these elements compares as follows with Herr Knopf's ephemeris which is given below :—

Zelbr-Knopf.

1889.	R.A. m. s.	Decl. ′
Sept. 8	− 2 17	+ 1·7
Oct. 2	− 5 49	+ 5·1

Herr Knopf's Ephemeris for Berlin Midnight.

1889.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Sept. 8 ...	0 5 14 ...	5 43·8 S. ...	0·3709 ...	0·1333 ...	1·34
12 ...	0 3 51 ...	5 39·0 ...	0·3734 ..	0·1353 ...	1·31
16 ...	0 2 21 ...	5 34·0 ...	0·3759 ..	0·1386 ...	1·28
20 ...	0 0 48 ...	5 28·6 ...	0·3786 ...	0·1430 ...	1·25
24 ...	23 59 15 ...	5 22·4 ...	0·3814 ...	0·1487 ...	1·21
28 ...	23 57 47 ...	5 15·3 ...	0·3843 ...	0·1554 ...	1·16
Oct. 2 ...	23 56 26 ...	5 7·0 S. ...	0·3873 ...	0·1632 ...	1·09

The brightness on July 8 is taken as unity.

COMETS 1888 *e* (BARNARD, SEPTEMBER 2) AND 1889 *b* (BARNARD, MARCH 31).—The following ephemerides are in continuation of those given in NATURE for 1889 August 1 :—

	Comet 1888 *e*.		*Comet* 1889 *b*.	
1889.	R.A. h. m. s.	Decl. ° ′	R.A. h. m. s.	Decl. ° ′
Sept. 7 ...	18 32 7 ...	8 46·3 S.	4 25 52 ...	2 13·1 N.
11 ...	18 27 51 ...	9 9·2	4 17 53 ...	1 1·8 N.
15 ...	18 24 14 ...	9 30·3	4 8 52 ...	0 15·0 S.
19 ...	18 21 15 ...	9 49·8	3 58 47 ...	1 37·1
23 ...	18 18 47 ...	10 7·8	3 47 36 ...	3 3·9
27 ...	18 16 50 ...	10 24·4	3 35 21 ...	4 34·5
Oct. 1 ...	18 15 19 ...	10 39·7 S.	3 22 1 ...	6 7·4 S.

REDUCTION OF RUTHERFURD'S PHOTOGRAPHS OF THE PLEIADES AND PRÆSEPE.—Two papers by Dr. B. A. Gould have recently been published in the memoirs of the National Academy of Sciences, which possess a very special interest at the present time, for they show that in the very dawn of astronomical photography, it was possible to determine the relative places of the members of a star-cluster from a series of photographs with a precision comparable to that attained even with a heliometer. In 1865, Rutherfurd had obtained a number of photographs of the Pleiades, and early in 1866 he placed the results of his measurements of these plates in the hands of Dr. B. A. Gould, who deduced from them the R.A.'s and Decls. of nearly fifty stars of the group, and who, further, compared these results with the heliometer measures of Bessel, made more than a quarter of a century earlier. The comparison, even as it stood, was a most satisfactory one, for, in spite of imperfections in the method of measuring the photographs, such as naturally occurred in a first essay, the probable error of a measure, either of distance or position, appeared as small for the photograph as for the heliometer, and the general agreement of the two methods was most gratifying. The paper in which this discussion was given, though presented to the Academy on August 11, 1866, has only recently appeared—a regrettable delay, for it might well have been that so striking a demonstration of the possibilities of the photographic method might have insured its adoption by astronomers a decade, or even two, earlier than has actually been the case. Dr. Elkin has now (*Astron. Jour.* No. 197) compared Dr. Gould's places of the Pleiades with values interpolated between the Königsberg heliometer places for 1840, and the Yale places for 1885, and after clearing the photographic results for some systematic errors thus disclosed, he finds the residuals very small indeed. Of sixty-eight values, only one exceeds 0″·38, and forty-seven are less than 0″·20, nor do they show any systematic character depending on distance or direction from the centre of the field. The probable error of a co-ordinate from the photographic measurements, he deduces as :—

For the brighter stars, ± 0″·079,
,, fainter ,, ± 0″·101.

Dr. Elkin concludes, therefore, that "the smallness of these probable errors must be convincing proof that in photography we have really a means of investigation for micrometric work at least on a par with any existing methods as regards magnitude, and doubtless far surpassing them in ease of measurement and output of work."

The paper on Rutherfurd's photographs of the Præsepe was presented to the Academy on April 14, 1870, and the central star to which the others were referred being a small one, instead, as in the Pleiades, of a very bright one, which had been, therefore, always much over-exposed, the results were even more satisfactory.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *SEPTEMBER* 8–14.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 8

Sun rises, 5h. 26m. ; souths, 11h. 57m. 27·9s. ; daily decrease of southing, 20·6s. ; sets, 18h. 29m. : right asc. on meridian, 11h. 8·4m. ; decl. 5° 32′ N. Sidereal Time at Sunset, 17h. 41m.

Moon (Full on September 9, 13h.) rises, 18h. 37m. ; souths, 23h. 35m. ; sets, 4h. 44m.* : right asc. on meridian, 22h. 47·7m. ; decl. 12° 20′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	 ° ′
Mercury..	7 43 ...	13 22 ...	19 1 ...	12 33·5 ...	4 47 S.
Venus ...	1 49 ..	9 30 ...	17 11 ...	8 40·3 ..	18 5 N.
Mars ...	2 56 ..	10 23 ...	17 50 ...	9 33·7 ...	15 45 N.
Jupiter ...	14 50 ...	18 43 ...	22 36 ...	17 54·6 ...	23 27 S.
Saturn ...	3 32 ...	10 47 ...	18 2 ...	9 57·4 ...	13 44 N.
Uranus ...	8 38 ...	14 4 ...	19 30 ...	13 15·3 ...	7 20 S.
Neptune..	21 12*..	5 2 ...	12 52 ...	4 11·7 ...	19 26 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Sept.	h.	
10 ...	19 ...	Mercury at greatest distance from the Sun.

Meteor-Showers.

	R.A. °	Decl. °	
Near ξ Persei	60 ...	37 N. ...	Swift ; streaks.
,, Aldebaran	72 ...	14 N. ...	,, ,,
,, Vega	282 ...	42 N. ...	Swift ; bright.
	354 ...	38 N. ...	Very swift.

Variable Stars.

Star.	R.A. h.	R.A. m.	Decl. °	Decl. ′				h.	m.	
Algol	3	1·0 ...	40	32	N. ...	Sept.	14,	2	3	*m*
W Tauri	4	21·7 ...	15	51	N. ...	,,	11,			*m*
X Boötis	14	18·9 ...	16	49	N. ...	,,	10,			*m*
U Coronæ	15	13·7 ...	32	3	N. ...	,,	9,	23	43	*m*
U Ophiuchi... ...	17	10·9 ...	1	20	N. ...	,,	8,	1	33	*m*
						and at intervals of		20	8	
X Sagittarii... ...	17	40·6 ..	27	47	S. ...	Sept.	9,	1	0	*M*
W Sagittarii ...	17	57·9 ...	29	35	S. ...	,,	14,	23	0	*m*
U Aquilæ	19	23·4 ...	7	16	S. ...	,,	12,	3	0	*m*
η Aquilæ	19	46·8 ...	0	43	N. ...	,,	8,	1	0	*M*
X Cygni	20	39·1 ...	35	11	N. ...	,,	8,	21	0	*M*
T Vulpeculæ ...	20	46·8 ...	27	50	N. ...	,,	9,	21	0	*M*
R Vulpeculæ ...	20	59·5 ...	23	23	N. ...	,,	11,			*M*
δ Cephei	22	25·1 ...	57	51	N. ...	,,	10,	22	0	*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

FROM advices just received from Queensland the *Colonies and India* understands that Sir William MacGregor left Port Moresby, for the ascent of Mount Owen Stanley, on April 19, in an open boat, with a party of fourteen for Vanapa River, 30 miles westward. He arrived safely, and pushed the boat up river for eight days, during which period he encountered many difficulties in crossing rapids and dragging the boat over rocks. When he could get no further he camped on the left bank of the river and sent Mr. Cameron (his secretary) back to Port Moresby for supplies, with native carriers to cross the mountain. Mr. Cameron returned with two boats loaded with provisions, thirty natives, and six Polynesians. All being ready, on May 17 the party, comprising forty-two men, left the camp, all packing, and the Governor taking the heaviest load. There were only four whites in the party. They crossed Mount Gleason at Eyton Junction, and then shaped a course north-east by east. At a height of 175 feet they crossed Mount Gunbar, next Mount Kulwald. Mount Belford was crossed at the Joseph River, after which they descended to the Goodwin Spur, and saw the first native house at Goodwin's village, Mount Musgrave, where they camped, the Governor going ahead with four Polynesians and seven natives; then ascended Mount Musgrave for over 7000 feet to Vanapa River and Mount Knutsford Range, over rough country; they followed a spur leading west. After three days' march they descended the spur, and began the ascent of Mount Owen Stanley on June 9, reaching the top on June 11 and 12, returning to Mount Musgrave on June 16. All hands started homewards and arrived at River Camp on June 22. They left on June 23, visited a mountain village on June 24, and met the steam launch from the *Merrie England*, which was searching for the Governor's party, and took them in tow. They arrived at Mana Mana on the 25th, and were taken on to Port Moresby, where the party landed after two months, all well. The only death that occurred was that of a native. The country traversed was very mountainous, and no table-land was discovered. Of the geological formation the country is mainly decomposed slate, granite, and quartz, with no sign of gold. Specimens of rock were collected by the Governor. The climate to 8000 feet is moist, above that dry and bracing. Natives were met on only two occasions, and were extremely friendly. They were stout well-built men, but no women were ever seen. Cultivation paddocks were fenced in. Potatoes, yams, and sugar-cane were plentiful, as also was tobacco. Natives, who were devoid of warlike implements, paid particular attention to head-dresses made of shells procured from the natives on the eastern coast of German New Guinea, who were showing friendly communication. Across the Owen Stanley Range the Governor collected many specimens of new plants, among others being some beautiful yellow rhododendrons, which he has since sent to Melbourne to Baron Von Mueller for report. A great number of new grasses in large patches were discovered. At Mount Victoria (Goodwin) he secured several new birds and one animal, which was something like a native bear, but had a long tail and dusty-brown collar and black extremities. The extreme length was 3 feet 6 inches, of which the tail was 1 foot 6 inches. There were five claws on all the feet, the tail was bushy, and it was estimated that the weight of the animal was 40 pounds. The birds of the lower altitudes were the same as those before seen, except as to a new paradise bird similar to the Great Epimachus. Sir William procured a female *Astrachia stephania*, the only male bird of that species being in the Museum, Berlin. The Governor procured several new small birds at Mount Victoria, including the identical English lark. Unfortunately, they were eaten by the Polynesians. Entomological specimens were obtained, including a milk butterfly. Only a few were captured.

ACCORDING to a *Times* telegram from Zanzibar, the Sultan has signed a concession to the British East Africa Company, of Lamu and the Ben-Adir coast, embracing all his territory from Kipini to Mruti. The concession embraces the administration and government of the island and port of Lamu, and of the northern mainland ports of Kismayu, Brava, Magadisho, and Warsheikh. The company's jurisdiction is thereby extended from the River Umba, in the south, to the port of Warsheikh in the north, an extent of about 700 miles of coast-line. Lamu is, next to Zanzibar and Mombasa, the most important port on the East African coast, and commands the trade of the mainland south of Kismayu, and that of the fine waterway of the Tana. It has been for years past the seat of a flourishing commerce, which is mostly in the hands of British Indian subjects, and it is a port of call for the British India Company's steamers.

OUR SENSATIONS OF MOTION.[1]

WE may distinguish two quite different kinds of sensation of motion, active and passive. When we walk or run or row, we use our muscles, and this use of our muscles is the cause of our motion, and also the cause of special sensations which may in a sense be called active sensations of motion. But we have other sensations than these connected with motion. For, if we are carried, or rocked in a boat, or dropped from a height, we are not only moved, but we are conscious of a very well marked sensation which we may call a passive sensation of motion. When we move ourselves we feel both kinds, and it is difficult for us to analyze what we feel and distinguish between our sensations as movers and our sensations as moved. It is to our passive sensations of motion that I wish to direct your attention to-night, and as these can best be examined in cases where they are not complicated with the other kind, we shall confine our attention almost exclusively to passive motion—that is, to cases where we are moved without any exertion of our own muscles. Now the first thing I have to say is in at all events apparent contradiction to the title of this lecture: it is that we have no direct sensation of motion as such. That this is so will be at once obvious if we consider the fact known to all, that we are at this moment being moved with very great velocity through space. We know that this is so, astronomers can prove it, but we are so perfectly unconscious of it that I dare say most of us here could not point the direction in which we are moving; in fact, as we are ignorant of the direction and rate of motion of the great system of which our solar system is a part, no one can say how fast and towards what point in space we are travelling. What we are conscious of is change of motion. It is because the motion of the earth is so steady, because, although very rapid, its changes are very slow, that we do not feel it.

There are two altogether different ways in which a body can be moved. These have been called respectively translatory and rotational. In translatory motion the body is always similarly oriented. Thus, if we consider motion within so small a part of the earth's surface that we may neglect the earth's curvature, such an object as this desk is subjected to purely translatory motion if we move it thus, so that the same side always looks up, and the same side always looks east. Rotational motion involves a change of orientation, and is rotation about an axis. This axis may always be the same, or it may change, and the change of axis may be abrupt or may be continuous. Most of the motions which we observe are made up of both kinds. When we travel by rail—always supposing that we sit still in the carriage—we are subjected to a purely translatory motion only when the train is running along a perfectly straight piece of the line. When it goes round a curve, we are—always supposing we sit still—subjected to rotation as well as to translation; because our face no longer continues to look in the same direction, but, as long as the train is running on the curve, continuously changes the direction in which it looks.

Let us examine what we feel when we are passively subjected to purely translatory motion. As long as the motion is steady,

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[1] Armitstead Lecture delivered in Dundee, by Prof. A. Crum Brown.

unchanged in speed or in direction, we feel nothing, as has been pointed out already. Let us look at the case when the speed changes, the direction remaining the same. We have to consider separately three different directions: (1) horizontal, (2) up, and (3) down, because we shall see that our sensations are different in these three cases. With change of speed in horizontal motion we are all familiar. The starting and the stopping of a train or of a steamer give us ample means of studying it. We all know the jolt of a badly started train. What we feel in such a case is, mainly at all events, due to our body being jerked forward or backward, according as we are sitting with our back or our face to the engine. But if the train is carefully started we find that our rate of motion may in a very short time be changed from nothing (relatively to the earth) to, say, thirty or forty miles an hour, without our feeling anything but the up and down rattle due to the slight unevenness of the rails. And the same was the case till comparatively lately with stopping. Now, however, since the introduction of the continuous brake, a train can be so rapidly stopped—its rate of motion, that is our rate of motion when we are in it, can be so very quickly changed from, say, sixty miles an hour, to nothing—that we do feel a strange, not altogether pleasant sensation. Experience has taught us what that sensation means, but at first it was so novel that experience was necessary to interpret it. It is not a sensation of jolt, the change though rapid is not abrupt. What we really feel, although it takes some amount of careful observation and some thought to see this clearly, is that the direction of the vertical, the direction in which a body falls, the direction in which our body presses has been changed. We feel this most distinctly if we are standing when the brake is applied; we feel that if we do not take means to prevent it, we shall fall over, and we prevent this by bringing our body into the line of the new vertical. Our feeling of unsteadiness depends on our uncertainty how long the new state of matters is to last. It lasts as long as the speed is being changed at the same rate, and the deviation of the new from the real vertical depends on the rate at which the speed is being changed. Our perception of deviation from the vertical is pretty acute. Most of us can tell a line to be off the vertical when it is inclined only a few degrees to it. In ordinary cases we have extraneous help in judging. We have walls or chimneys, known to be vertical, or surfaces known to be level, with which to compare, but even when we have no assistance of this kind we are not often far wrong. It might be supposed that it is the pressure of our body on the floor or ground that gives us the idea of the vertical, but that idea still exists in cases where we can feel no such pressure. If our body is supported in water, or entirely submerged, as in diving, we still have a very distinct, and fairly accurate notion of up and down, although in such cases, as our body is very nearly of the same density as water, the resultant pressure on it is small. We shall see in a little a possible explanation of our sense of the vertical. When the train in which we are travelling runs quickly round a sharp curve, we feel something very like the sensation just described. And indeed it is due to a perfectly similar cause; the apparent vertical is the direction of the resultant of the force of gravity and the centrifugal force, and is, as every engineer knows, more inclined to the real vertical as the curve is sharper and the speed of the train greater. In this case the sensation is complicated, because the motion is not one purely of translation, but, as already pointed out, is compounded of translatory and rotational motion.

Let us now look at cases of up and down motion. As we can study horizontal motion in the railway train, so we can up and down motion in a lift. Here also we see that it is change of motion which we really perceive. For, once the lift is started, and is moving smoothly and uniformly, either up or down, we are quite unconscious of the motion. It is the start and the stop, or the quickening or slowing of the motion only that we feel. And the stopping of the upward motion produces exactly the same feeling as the starting of the downward motion, provided they are equally smooth and free from jerk. It is easy to see what are the physical conditions here. Just as the acceleration in a horizontal direction inclines the apparent direction of gravity, so acceleration up increases, and acceleration downwards diminishes, the apparent intensity of gravity. If the lift *fell* down, unrestrained, its inmates, during the short time the experiment would last, would have no sense of the force of gravity at all. An object dropped from the hand would not fall down to the floor, because the floor itself would be falling at the very same rate as the object. And what is true in this extreme case is true also in a measure in all cases of downward acceleration. But only in cases of *acceleration*, for, however fast the lift goes down, if it moves uniformly, without change of speed, the bodies of those in it press on its floor exactly as if it were at rest. Similarly, upward acceleration increases the apparent force of gravity. The physical conditions, then, of our perception of acceleration of translatory motion in any direction are change in the apparent direction, intensity, or both, of the force of gravity. It is a strange and interesting fact that our perception of downward acceleration—that is, of diminished force of gravity—is more acute than that of acceleration upward or in a horizontal direction. We feel the starting of the lift as it goes down, and its stoppage when it has come up, much more distinctly than the start on the way up or the stop at the end of the journey down. And when we are rocked in a rolling steamer, it is the beginning of the downward move that is most perceived.

Having now discussed the phenomena of our sensations connected with translatory motion, let us examine what our experience is when we are turned round, or subjected to rotational motion. We execute such movements every minute of our waking life. But as with translational motion, so, and even more, with rotational motion, it is impossible to analyze our sensations when they are complicated with what we feel we do. And in the case of rotation a very serious complication is introduced by our seeing how we are being moved. So that, to make a strict examination of our sensations in this matter, the observer must place himself blind-folded on the rotating apparatus, and be passively turned round. Or, as in Prof. Mach's very ingenious experiments, a small hut with translucent paper windows may be placed on the turn-table for the reception of the observer. Just as we can move, or be moved, right or left, backwards or forwards, up or down, so we can be spun round about a fore-and-aft, a right-and-left, or an up-and-down axis, and about each of these axes either the one or the other way round. It is plain, however, that we can get simple results only in the case of rotation about a vertical axis, because otherwise a great complication would be introduced by the varying position of our body relatively to the direction of gravity. We shall see that we can get everything we require with rotation about a vertical axis. Here we find, as in the former case, that it is only change of motion that is perceived. The observer sits on a chair on the turn-table, his eyes are bandaged, and an assistant gives the table a steady, uniform rotation. At first the observer feels the turning quite distinctly, but after less than a whole revolution the sensation becomes very indistinct, and, while the turning still continues at the same rate, soon disappears altogether. If the rate of turning is now increased, he feels it begin again, all that he perceives being the increase; his perception of that also soon dies away, so that in a short time he may be spinning rapidly round, while he feels completely at rest, and is only aware that he has been gently turned round a little, two or three times. But if you now stop him he feels a turning round in the opposite way to that in which he really was turning, the fancied rate of turning being, at the moment of stopping, that of the real turning which has just been stopped. This imaginary rotation dies away, exactly as the sensation of the real rotation did. Now, *very nearly* the same thing takes place, whatever is the position in which the head is placed during the experiment, *if the head is kept rigidly in the same position during the whole of the experiment*. Let us look at a case in which the position of the head is not kept the same during the experiment. The observer sits on the table, with his head inclined to one side, so that the line from ear to ear is vertical. He is now turned uniformly round; as before, he feels the turning at first, but as the uniform turning goes on the sensation dies away. When he feels perfectly at rest, let him give the word to stop, and at the same instant raise his head into the ordinary position. He will now feel as if he were being turned about the line from ear to ear—that is, now, about a horizontal axis. If his right ear was down when he was actually being rotated, and if the turning was with the hands of a watch lying with its face up, then the imaginary rotation will be the opposite way round—he will feel as if his head were going forward and his feet back. This sensation will last only a short time, but there is a risk in trying the experiment, that the observer may try to correct this alarming overturn by throwing himself backwards; if he is nervous it may therefore be as well to have him strapped to the chair. Whatever line in the head we make vertical while the real rotation is going on—that is, whatever line in the head we make the axis of the real rotation—that line is the axis of the apparent rotation which we feel when the real rotation stops, however we may move our head at the time of the stoppage. There is a practical joke depending on this principle, which I have seen played. The subject of the joke, who ought, of course, to be a person not conversant with the laws of the sensation of motion, is asked to hold a poker upright on the floor, and, placing his forehead on the top of it, to walk three times round it, rise up, and walk across the room. His march round the poker is a rotation about the fore-and-aft axis of his head; when he rises up he feels the contrary imaginary rotation, about the same axis, now, of course, horizontal, so that, if he went round with the sun, he falls to the right, and to the left if he went round the other way. In a very interesting experiment with the turn-table we have a combination of real and apparent rotation. Lie down on the table, say on the left side, so that the left ear is vertically under the right ear, so making the right-and-left axis of the head the axis of rotation; let the table be turned round at a uniform rate, wait until all sensation of rotation has ceased, and then, while the uniform turning is still going on, roll yourself over on your back. You will then experience a very startling sensation. The new axis of rotation of the head is the fore-and-aft one; there was no rotation about it before, therefore you feel that real rotation, the real rotation about the old axis—right-and-left—has just ceased, therefore you feel the imaginary opposite rotation. The sudden occurrence of the combination of these motions, felt as real, resulting from a cause so seemingly inadequate, rolling from your side to your back, gives rise to an almost dreadful sense of insecurity.

Thus then in rotational as in translational motion it is change of motion, what is technically called acceleration, which we perceive. There are two questions which naturally arise in this connection: (1) What is the use of this sense? and (2) What is the organ of this sense, and how does it work? What is the use of it? Everyone will, I am sure, admit that it must be of great use to us to have a constant knowledge of the direction of the vertical; to have, as it were, a private level of our own, which we cannot mislay. As to our sense of rotation, it is to it chiefly that we owe what we call our knowledge of the airts; it enables us, as we walk about on winding roads, or through narrow crooked streets, to retain some idea of the real directions. But the chief use, no doubt, of the sense of rotation is to enable us to control and regulate the rotatory movements of our head—movements we are constantly making as we look about us. It may be asked, in these short quick movements of the head, where is the secondary sensation of turning the other way which I described? We never experience it at all. Mach has very clearly, and with great penetration, explained how this comes about. These quick movements, our habitual movements of rotation, are so short in their duration, that during them we do not come to feel that our head is at rest. The sense of the original real rotation is still vivid when the rotation is stopped, so that the secondary sensation of an imaginary motion the other way round merely annuls the primary sensation, puts an end to it when the real rotation stops. Without such a stopper of sensation we should go on feeling the rotation for a short time after it was done.

What is the organ of this sense? There is in our head a very remarkable organ which has been for long a puzzle to physiologists, an organ which is found not only in our heads, but also in the heads of all mammals, of all birds, and of all but the very lowest fishes (and even in the very lowest fishes it occurs in a less developed form). This organ is so closely related in position to the organ of hearing, that it was long supposed to be a part of it, and we shall see what attempts were made to explain it as an organ of hearing. I shall give as short a description of it as is compatible with making it plain how it can act as the organ of the sense we have been considering. I must at the same time confess that in some points our knowledge of the matter is still imperfect, and that much has still to be done before we can explain its action as fully as we can that of the eye, for instance.

The organ in question is lodged in a bony cavity continuous with that which contains the organ of hearing, and for this reason was long, and perhaps by some is still, regarded as itself having something to do with the perception of sound. This cavity in the hardest bone in our head consists of four parts—the vestibule, and the three semicircular canals. The vestibule is an irregular chamber, in man about ¼ of an inch long and ⅐ of an inch broad and deep. In its walls are five openings leading to the semicircular canals. These are tunnels in the bone having an elliptical or circular section, and opening at each end into the vestibule. The central line of each canal lies nearly in one plane, which we may call the plane of the canal. At one end of each canal there is an enlargement called the ampulla. The planes of the three canals are approximately at right angles to one another. The canals are named from their position—the horizontal, the superior, and the posterior; the two latter unite at their non-ampullary ends before joining the vestibule, so that there are five and not six openings into the vestibule from the canals—three ampullary, one for each canal, and two non-ampullary, one for the horizontal, and one common to the superior and the posterior canals. The plane of the horizontal canal is nearly horizontal in the ordinary position of the head in all animals, and is therefore at right angles to the mesial plane: the planes of the two other canals make nearly equal angles with the mesial plane.

In the bony labyrinth just described there is inclosed a membranous labyrinth of a generally similar form. It consists of the utricle, lodged in the vestibule, and of three membranous canals, each furnished with a membranous ampulla. The membranous labyrinth does not fit tight into its bony case. The utricle is much smaller than the vestibule, which contains, besides, the saccule, an organ connected with the cochlea; and the diameter of the membranous canals, except at the ampullæ, is much less than that of the bony canals. The membranous ampullæ, on the other hand, nearly fill the bony ampullæ. The entire cavity is thus divided into two spaces, one within, the other around, the membranous labyrinth; each is filled with a liquid, the endolymph and the perilymph. The nerves are distributed to one spot in the utricle, and to a crescent-shaped ridge near the middle of each ampulla. The nerves end in hair-cells, the hairs of which project into the endolymph. The *macula acustica*, the spot in the utricle to which nerves are distributed, is covered with a gelatinous layer in which are embedded small crystals of carbonate of lime.

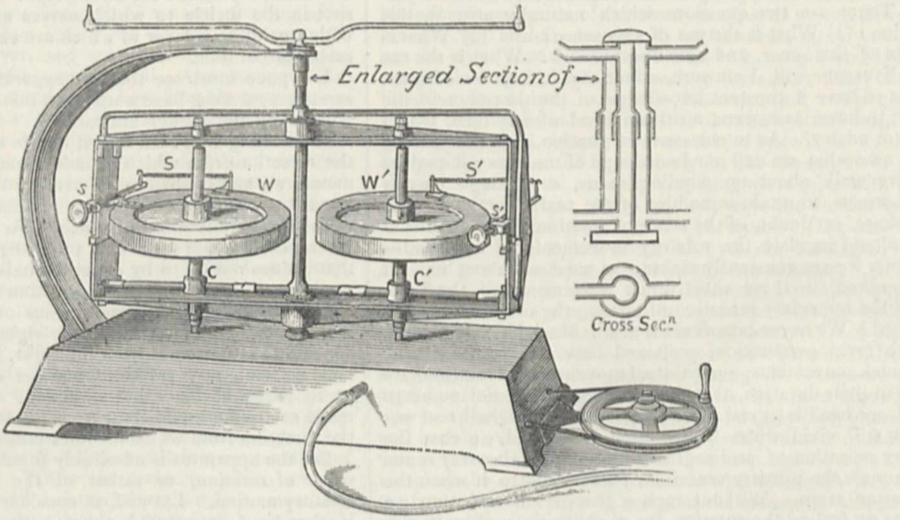
Everyone must see that an apparatus so purpose-like in its arrangement must have a use, and this use must be one applicable to all the higher animals.

It was long supposed that it had to do with our perception of the direction from which sounds come to us. The idea is not unnatural, and is obviously derived from the nearness of the apparatus to the organ of hearing, and from the relation of its form to the three dimensions of space. No explanation has ever been given how it could serve this purpose; and we can easily show that it does not do so by experimentally showing that we have no means of ascertaining the direction from which a sound comes except by two or more simultaneous or successive observations. If a sound is heard louder in the right ear than in the left we conclude that it comes from the right, and by turning round the head we soon get a sufficient number of observations to enable us to judge of the exact direction. If a short sharp noise is made at a point equidistant from the two ears, we do *not* know the direction from which it comes unless we see what causes it.

But the apparatus is admirably fitted to act as the organ of the sense of rotation, or rather of the sense of acceleration of rotatory motion. Let us first consider the action of one canal. If the head is rotated about a line at right angles to the plane of the canal, with the ampulla leading, you will see from looking at the diagram that there will be a tendency of the endolymph to go from the utricle into the ampulla, and of perilymph to go from the space between the bony and the membranous canals into the utricle. These will conspire to stretch the membranous ampulla where the nerve-endings are, and we can easily see how this will stimulate the nerves, and send a message to the brain. But this stretching will not take place if the head be rotated the other way about. In that case the tendency of the fluids will be in the opposite directions, and will rather make the ampulla less tightly stretched, and we can readily suppose that this may not stimulate the nerves, and no message will be sent to the brain. One canal will thus be able to give indications of rotation about one axis, in one of the two ways round. But for each axis we have two canals, one turned the one way and the other the other. And as by means of three rectangular axes we can represent any rotation, so any rotation will be perfectly recognized by means of the six canals. That this is actually the function of this organ is further proved by the effects of injury or disease. One ear is sometimes attacked by inflammation while the other ear is unaffected. In such cases the patient suffers from persistent vertigo—that is, sense of rotation where no real rotation occurs. This is, at least, one form of what is known as Ménière's disease, so-called from the name of the physician who first drew the attention of medical men to it. Now it is obvious that, if the six canals act in the way I have indicated, the pathological irritation of the three canals of one ear will produce a sensation of rotation about an axis the position of which we can foretell from the relative positions of the planes of these three canals. And clinical observations on persons suffering from this disease show that the vertigo actually is about this axis. Deaf-mutes are persons who from a very early age have had no sense of hearing at all. This condition may arise from the imperfect development of the organ of hearing, or from its early destruction by disease. In either case it often happens that the organs we have been discussing, from their nearness to the organ of hearing, are involved in the mischief, and are also imperfectly developed, or destroyed by disease. Deaf-mutes have therefore not unfrequently the semicircular canals in a state unfit for use. Experiments have been made on deaf-mutes with the object of testing the accuracy of their sense of rotation. Those who have made these experiments report that many deaf-mutes are insensitive to rotation. If these observations are confirmed, the theory I have just been explaining will receive a very great support.

In order to illustrate this theory, and to show that the principle on which it is based is a sound one, I have devised a sort of working model which I shall now show you. I may say that when I accepted your invitation to lecture here, and had selected the subject of our sensations of motion, the idea occurred to me of making a sort of working model of the semicircular canals. The difficulty was to find an instrument-maker who could help me over the obstacles which always lie in the way of a designer who is not himself an engineer. I take this opportunity of thanking Mr. Alex. Frazer for his help in this matter. He at once understood what I wanted, and so gave me the use of his skill and experience that the instrument here is exactly what I intended it to be, and a great deal better than my most sanguine hopes.

The model, as perhaps you will better see from this somewhat diagrammatic drawing than from the machine itself, consists essentially of two heavy wheels, placed side by side, with their axes parallel, in a frame which itself can be turned round about an axis parallel to that of the wheels. These heavy wheels correspond to two parallel canals, say the two horizontal canals. As it is the inertia of the fluid in the canals which enables them to work, so here it is the inertia of the heavy wheels. Each wheel has a stop, which altogether prevents its turning, in one way round, beyond a certain point. The one wheel is thus checked in turning the one way, the other in turning the other way. Each wheel is just held against its stop by a spring which is stretched when the wheel turns away from the stop. Each wheel with its stop and spring is as nearly the mirror image of the other as it could be made. When I turn round the frame, both wheels tend to lag behind the rotation of the frame, on account of their inertia. One of them cannot lag behind at all because of its stop, and the other cannot lag much behind because of its spring; the stronger we make the spring the less can it lag behind. This lagging behind is, of course, a turning of the wheel on its axis, relatively to the frame, in the opposite sense to that in which the frame is turned. As we continue to turn the frame with uniform speed, the spring brings the wheel back to its original place against the stop, and further rotation at the same rate makes no change in the relative position of the parts of the machine. But if we now quickly bring the frame to rest, both wheels in virtue of their inertia tend to continue their rotation: one, that one which made the relative movement before, cannot continue its rotation because of its stop; the other can rotate a little, not much, because of its spring—it turns a little, but is soon brought back to its original position against its stop by the spring. You will easily see that just as in the model the inertia of the wheel corresponds to the inertia of the fluid, so here the stretching of the spring corresponds to the stretching of the ampulla. All that we want to make the model complete is to find some way of making the stretching of the spring visible, something which shall correspond to the message sent to the brain. You cannot easily see the stretching of the spring while the frame is turning round, and it was necessary to devise some way of making it visible. We must here leave the analogy of the living organ. The brain turns with the labyrinth, but we are the brain of this machine, and we do not turn with it. After a good deal of consideration, and after thinking of and rejecting a good many plans, I fell upon the one I shall now show you. In the lower end of the axle of each wheel there is fixed a stop-cock, through which gas can pass from one pipe to another. When the wheel is against the stop only a very little gas passes—just enough to prevent the jet going out. When the wheel turns away from its stop, the stop-cock is opened, and the stop-cock is so adjusted that the quantity of gas passing shall be roughly in proportion to the stretching of the spring. By a contrivance indicated in the diagram, the two gas pipes, one from each stop-cock, are brought through the axle of the frame and led each to a gas jet.



Now, when I begin to turn the frame one jet flares up, but as I continue the turning, as nearly uniformly as I can, you see the jets remain at their minimum, which I shall call zero of sensation. Of course I could have made this zero the zero of gas too, but then we should have needed a subsidiary flame to light the gas when the stop-cock opened. I now stop the frame, and you see the other jet flare up for a little. That corresponds to the secondary imaginary rotation which we feel when a real rotation is stopped. I ought to apologize for so often calling this a secondary or imaginary rotation. I hope you all see now that it is as really an *acceleration* in the strict meaning of the word as the original start from rest.

I have taken this question—What is the organ by means of which we perceive acceleration of *rotatory* motion?—first, because it has been most fully worked out. We now come to the question, How do we perceive acceleration of translatory motion? This, as we have seen, is the same as the question, How do we by our senses recognize the direction and estimate the intensity of what is to us at the moment the force of gravity? A very natural suggestion as to the way in which we perceive the intensity of this force is that it is a skin sensation; that it is by the greater or less pressure which we feel on that part of our body which rests on our support. Prof. Mach, to whose experiments I have had often to refer, and to whom we owe, more than to any other investigator, our knowledge of the whole subject of the sensation of motion, has very clearly proved that this is not the case. Everything we know as to this sense leads us to look for its organ in the head. And there is an organ which, to some extent, at all events, seems to be what we are in search of. The *macula acustica* in the utricle is a spot well furnished with nerves, and we have not found out any special function for it. There is a similar *macula* in the saccule, that other membranous bag contained in the vestibule. Mach has suggested that the *macula* of the utricle may be the organ by means of which we perceive acceleration of translatory motion. Let us look at it, and see how far it is fitted to act as a level. Its length is stated by Prof. Schwalbe to be about $\frac{1}{8}$ of an inch, and its breadth a little less. According to the same observer, it covers a part of the floor, the anterior wall, and a part of the external wall of that part of the utricle called the "recess." Its nerves end, as has already been stated, in hair-cells, and these are covered by a gelatinous substance filled with a fine powder of crystals of carbonate of lime. What is interesting to us in this description is that it looks in three directions, and that the whole of it is covered with a powder of considerably greater density than the fluid (the endolymph) with which the utricle is filled.

Let us try to imagine a model of this structure. Let us take a box of glass, so that we may see what goes on in the inside of it. Let us put on a part of the bottom of the box, on the end of it, and on a part of the side of it, a layer of thin jelly mixed with fine sand. Fill up the box with water, and put on a lid. We shall find that we have an apparatus that does to some extent answer the purpose of a level. When we change its inclination, the jelly but for the sand would indeed have very little tendency to change its position, but the sand, being specifically heavier than the water will, and will either move through the jelly if that is thin enough, or pull the jelly with it. In any case, a change of position of the box will involve a change in the relative position of its contents. In the actual case of the *macula acustica* such a change in the relative position of the sand and the hair-cells must give rise to an irritation of the terminations of the nerves, and send a message to the brain. We cannot as yet work out all this in detail, as we can the way in which the canals give us information as to the acceleration of rotational motion, but we know enough to turn our attention to the subject; and we may hope that, by more accurate study of the sensation phenomena, and by comparison of them with the anatomical facts, this important and interesting physiological question may be satisfactorily answered.

ON THE GEOLOGICAL HISTORY OF THE PREHISTORIC FLORA OF SWEDEN.

FIFTY years have passed since the Danish Professor, Japetus Steenstrup, presented to the world his masterly researches on the history of Denmark's peat-bogs. These researches clearly demonstrated that the forests of Denmark had suffered remarkable variations. The oldest forests had consisted chiefly of aspen (*Populus tremula*), next, for a long period, of Scotch fir (*Pinus sylvestris*), then of oak, and, finally, of alder (*Alnus glutinosa*). Remains of the beech—now Denmark's chief species of tree—are, however, entirely absent from the peat-bog. Consequently, it must have been the last to immigrate. It was a natural surmise—and one even advanced by Steenstrup—that the changes referred to were connected with a gradual softening of the climate, a view defended, too, by Prof. Forchhammer.

But Steenstrup's researches were in advance of their time; glacial geology was only in its infancy, and voices were therefore raised both in Denmark and Sweden claiming to interpret these changes in the forest vegetation as one of Nature's great systems of evolution, whereby one variety, so to speak, prepared the soil for the next comer, without any reference to climate. However, this view has now but an historical interest. For, since our knowledge of the geology of the Glacial age has become more and more enlarged, and since remains have been found in Scania of a true Arctic flora embedded in the fresh-water clay deposits of that province, a return to Steenstrup's theory that the changes of climate and forest vegetation were related was but natural. Indeed, the same Arctic flora was shortly discovered underneath the aspen layer in the Danish peat-bogs, so that the aspen flora cannot be regarded as the first after the Glacial age. By degrees, as the ice melted, the denuded soil was invaded by the Arctic flora from the south. First, when the climate became still milder, and a forest vegetation could flourish, the aspen and the birch immigrated, and in turn the pine, oak, and alder. Each one formed in its day the forest of Denmark, and they were naturally accompanied each by its own peculiar undergrowth of shrubs and plants. Enormous ages elapsed between these events. The remains of the peat-bogs show that generations upon generations of grand firs flourished before the oak immigrated and before the pine flora was at last ousted. And in its turn the oak reigned supreme during countless ages, until that, too, was extinguished by the alder and the beech. During the fir period, the men of the Stone Age spread themselves over the land; when the fir was supplanted by the oak, the Bronze Age began. If we bear in mind how exceedingly slow the extension of the oak and the beech is, we can form some idea of the immense time that must have elapsed since the Ice age.

Therefore the flora of Denmark consists entirely of the offspring of immigrated plants. Many of the species which appeared in the country did not remain; to them Denmark was only a station on the road towards higher latitudes. This was, for instance, the case with the greater portion of the Arctic flora, as well as with the fir. From the beginning of the historical period the beech has flourished throughout the whole country.

In Sweden, in districts rich in calcareous matter, tuff strata are deposited from springs which, by their contents of carbonic acid, contain carbonate of lime. When calcareous water is exposed to the air, the carbonic acid gas evaporates, and the carbonate of lime is precipitated in the form of a white deposit, which soon hardens to stone. Such a deposit is therefore particularly natural around the mouth of a spring or in some pool into which the calcareous water is discharged. Consequently the leaves or other remains of plants growing around are covered, soon after falling into the water, with a thin coating of chalk, and although they are by degrees destroyed, this imprint in the chalk remains—often so distinctly that the finest fibres of a leaf may be traced.

A necessity for the formation of calcareous tuff is therefore the presence of calcareous rocks, whence the water may draw the lime. And, indeed, with us all calcareous tuff deposits, as far as we know, are confined to where the chalk formations are richest, as, for instance, Scania, Western and Eastern Gothia, Jemtland, Ångermanland, and Åsele Lappmark.

The greatest in extent and richest in leaf imprints are the old well-known tuff strata at Benestad, in Scania, north of the town of Ystad. They were formerly largely worked for building purposes, as, for instance, for several churches. The tuff is referred to by early writers as rich in leaf imprints, but in their writings a serious error crept in, viz. that even leaves of the beech were imprinted. This is wholly without foundation.

In consequence of the quantity of stone removed, it is now impossible to fix precisely the nature of the stratification, and what we know on this point is due to the particulars supplied by Baron Claes Kurck, who carefully examined the strata. His researches fully confirm the views of the writer, expressed as far back as 1872, viz. that the oldest strata were deposited whilst the aspen was the predominating tree in the districts, and before the fir had immigrated. Kurck has also found here traces of birch, grey sallow (*Salix cinerea*), and possibly the common sallow (*Salix caprea*). Above this stratum we come to the fir, deposited when that predominated. The imprints of the fir, in the shape of needles, branches, bark, and cones, show that this tree grew close by the springs. Most of the tuff dates from the fir period, but during the same age other species of trees gradually immigrated, of which indications are found in the lower parts of the fir deposit. From these the National Museum possess a rich collection, chiefly made by Nordenskiöld in 1873. It contains several rare species of plants, and the imprints of leaves are so remarkably clear that the collection is one of the greatest ornaments of the palæontological section.

Of the trees which flourished contemporaneously with the fir, we learn from Kurck that the birch, mountain-ash (*Sorbus aucuporia*), *Salix caprea*, and *Salix aurita*, were the oldest, and the hazel but little younger. To somewhat later strata he refers dog-wood (*Cornus sanguinea*) and berry alder (*Rhamnus frangula*), and he believes that the remains found of Dutch rush (*Equisetum hyemale*), and the guelder rose (*Viburnum opulus*) also date from the same period. If this be the case, it is probable that some leaves in the Nordenskiöld collection of hawthorn (perhaps *Cratægus monogyna*) also belong to this stratum.

Naturally, the species named also appear in this collection. The later strata from the fir period are, according to Kurck, distinguished by a quantity of leaves of the mountain elm (*Ulmus montana*). Leaves of hazel, birch, aspen, &c., are also found; whilst, for the first time, leaves of the lime (*Tilia parvifolia*) are met with, but very rarely. In this stratum Kurck has also found the common bracken (*Pteris aquilina*) and meadowsweet (*Spiræa ulmaria*). Some leaves of the alder he believes to be still younger.

Besides the species named—which are all found with remains of the fir—are some others. Already in Nordenskiöld's collection notice was attracted to portions of a stratum in which the fir was absent, and which in a measure differs in appearance somewhat from the rest of the tuff. These portions contain leaves of the mountain ash, the common oak (*Quercus pedunculata*), viz. hazel, lime, salix, and birch. This stratum has been re-discovered by Kurck, and is, as might be expected, younger than those named. It may be added that, according to earlier writers, leaves of the maple (*Acer platanoides*) have also been found at Benestad, but latterly none have been discovered.

It would seem, then, that there is at Benestad a counterpart to the Danish aspen, fir, and oak periods; but we cannot quite say that as regards the second period. There is, for instance, nothing to prevent the fir having vegetated, through some accidental causes, at Benestad, even during a part of the oak period. This is the more likely as we find the tree at the present day in Northern Scania. If, therefore, on the one hand, it is certain that the elder pine strata at Benestad belong to the true fir period—the time before the immigration of the oak—it cannot, on the other, be denied that the younger section of the fir strata may have been deposited already when the oak immigrated thither. That some of the fir-bearing strata must have been deposited relatively late seems evident from the presence of such plants as dogwood, hawthorn, elm, &c. However, the problem will no doubt be finally solved when once the peat-bogs of the province have been palæontologically examined.

Of the beeches and hornbeams (*Carpinus betulus*), now common in the district, there are no traces in the strata, and although the springs still yield plenty of water, there appears to be no deposit of calcareous tuff whatever. Thus, all we are really able to say respecting the palæontological remains in the fir period at Benestad is that these plants immigrated before the beech, and, most probably, even before the oak, and that all of them came from the south-west.

Respecting the flora which flourished in Scania previous to the aspen and fir periods, we know its characteristics through the vegetable remains in the fresh-water clays of the province. They display a rich Arctic flora, comprising *Dryas octopetala*, *Salix polaris*, *herbacea*, *reticulata*, *Betula nana*, *Oxyria digyna*, &c. An animal fossil must also be mentioned, only recently found among them, viz. *Apus glacialis*, now common in Spitzbergen lakes, but which in Scandinavia is not found south of the lakes in the Dovre Mountains.

In proportion as the inland ice melted, the Arctic flora, so rich in varieties, advanced, but as the climate became milder this flora was replaced by the forest vegetation immigrating from the south, and, at all events at Benestad, the trees followed each other in the same succession as in Denmark, viz. aspen, fir, oak, whilst the beech immigrated considerably later. It is most probable that the forest vegetation followed in the same order in the whole of Southern Sweden, but we do not know this with certainty. For we still know too little respecting the remains of plants in the peat-bogs of Southern Sweden. That we are far behind in this respect is chiefly due to the circumstance that in Denmark the peat-bogs are turned largely to profitable account.

Of the calcareous tuff strata in the province of West Gothia we know unfortunately very little. They are, however, small in extent, and contain only remains of trees still found in the neighbourhood, viz. hazel, salix, and aspen. Near the Eskedal railway station there is certainly a very large deposit, but not formed of continuous tuff, but of loose calcareous *débris*, which does not retain imprints.

In East Gothia we have two calcareous formations, one near Vreta cloister, which appears to be of a rather recent date, as it contains salix, hazel, oak, and lime. The other, north of Vadstena, is, however, much older, and therefore of great interest. This contains, among other remains of a pure Alpine plant, *Dryas octopetala* (L.), which we already know from the Scania fossil glacial flora, besides dwarf birch (*Betula nana*), and perhaps also *Betula intermedia*. There are also leaves of several varieties of willow, birch (*Betula odorata*), crowberry (*Empetrum*), *Vaccinium uliginosum*, and fir needles. The discovery of Dryas in this locality is of the highest interest and importance, as it leads us to hope that we may discover in other parts of Sweden between Scania and Jemtland remains of an Arctic flora, which, judging from the discovery referred to, must, at all events partly, have advanced from the south through the whole country. Formerly we did not possess a single palæontological proof from this part of Sweden in support of this assumption. The discovery is also of great interest in another respect, inasmuch as it shows that Lake Vettern must have become separated from the Baltic, with which it was once connected, at a time when the climate was Arctic—an assumption in full accord with Prof. S. Lovén's discoveries respecting the Arctic Sea fauna that to the present day is found at great depths in this lake.

The Jemtland strata have of late been examined by Herr A. F. Carlson. This province is rather rich in calcareous tuff deposits, some twenty localities having already been discovered. Here, too, the first forests appear to have consisted of aspen, birch, chiefly *Betula odorata*, and fir, of which latter remains have been found in several places. In four localities *Dryas octopetala* have been found, and in two *Salix reticulata*, both of the Arctic flora. As the former were found at Fillsta and Digernäs, in Sunne parish, and the other at Semla, in Mörsils parish, it would appear to be proved that the Alpine flora of Jemtland formerly had a far greater extension within the province than at present, and that at that period it reached as far as the basin of Lake Storsjö. Without doubt an examination of the fresh-water clays of the province would go to show that the Alpine flora formerly covered the whole of Jemtland. The tuff strata which contain such remains were deposited a little later, whilst the Alpine flora was in process of being displaced by the forest vegetation.

The remaining plants that have been found in Jemtland are Dutch rush (*Equisetum hyemale*), salix, several varieties of willow, dwarf birch, *Betula intermedia* and *alpestris*, *Alnus incana*, crowberry, mountain ash, *Sorbus aucuporia*, *Vaccinium uliginosum*, and sea buckthorn (*Hippophaë rhamnoides*).

From the province of Ångermanland we know two tuff deposits, near each other, but the palæontological remains are few and badly preserved. Here have hitherto been found only some lichen, fir, birch, salix, and willow.

In Åsele, Lappmark, between Långfors and Långsile, there are two deposits—as far as we know the northernmost in Sweden. Both are rich in leaf imprints, and thanks to Colonel N. Sjöberg, of Åsele, the National Museum has obtained specimens. The species are certainly not numerous, but they are well preserved, and consist of lichen, leaves of fir, birch, aspen, salix, some willows, and *Hippophaë rhamnoides*.

As I have already stated, leaves of this plant have also been found in a spot in Jemtland situated more than 1500 feet above sea-level. Here the leaves are found in common with remains of Dryas, but whilst the latter now has to be sought high up in the mountains, the former is, on the contrary, only found by the coast. It is found along the shores of the Baltic, from Roslagen (near Stockholm) to Vesterbotten in the north, and the thorny shrub, with its whitish silvery leaves, and yellow or chrome-coloured sour berry-like fruit, here goes by the name of *haf-thorn* (*i.e.* sea thorn). In other parts of Central and Northern Europe where this shrub grows, it is also a coast plant, but it is found beside the glacial rivers of the Alps; and its former existence in the localities named in Norrland—right in the heart of the country, high above the sea, together with other Arctic plants—shows indisputably that here also it was originally an Arctic plant. But whilst *Dryas*, *Salix reticulata*, &c., were forced northwards by the immigrating forest flora, this plant, on the contrary, found a place of refuge on the seashore, where it flourishes to the present day. It seems, indeed, hardly credible that two plants now so widely separated, geographically, as Dryas in the high mountains and Hippophaë by the Baltic shore, once grew side by side in the heart of the country.

Having dealt with the proofs respecting the former extension of the Swedish flora furnished by the palæontological remains of the calcareous tuff strata, I purpose to touch upon an equally important point, viz. that the spruce immigrated comparatively late. In no single tuff deposit has trace been found of this tree, and this circumstance is too uniform throughout to be a matter of accident. Especially peculiar are the conditions in

Jemtland, where several of the localities in which the palæontological discoveries were made are surrounded by spruce forests, and where the tree grows higher up on the mountains than the fir, which is the reverse of what is generally the case. In Jemtland, therefore, the spruce clearly immigrated after the fir, and first when the principal tuff formation had ceased. Whence did the spruce immigrate into Sweden? It cannot have been from the south or from Denmark, for remains of the tree are totally absent in that country's well-explored peat-bogs, neither does it belong to Denmark's present wild flora. Neither can the spruce have come from the British Isles. Certainly it appears from palæontological evidence that the spruce existed in England before the Ice age, but it appears to have been extinguished during that age, as it is absent from the post-glacial deposits as well as from the British flora of the present day. Finally, the scarcity of the spruce in the western parts of South-Eastern Norway fully proves that it did not immigrate from the west. There cannot, therefore, be the slightest doubt that the spruce immigrated into Sweden from the east. This assumption corresponds entirely with the present extension of the tree east of the Baltic. But this immigration cannot have taken place *viâ* Northern Sweden around the Gulf of Bothnia, as this part can be supposed to have had only at a later period a climate mild enough for its existence. It is more probable that the immigration took place *viâ* the Island of Gothland on the south-east coast, or *viâ* the Åland Islands, off Central Sweden, and that the spruce afterwards spread north, west, and southwards.

It may be mentioned, by the way, that the spruce existed in the neighbourhood of Enköping at the time when Lake Mälaren was a bay of the Baltic, and the sea covered the spot where the town is now situated. Of great importance with regard to this point is the discovery recently made by Dr. H. Munthe, that the spruce was found in Gothland at a time so far remote that the division of land and sea then was wholly different from that which at present exists. This seems to speak for the immigration of the spruce by this road. Further, Dr. R. Hult, of Helsingfors, last summer discovered in West Nyland, in Finland, sub-fossil remains of spruce in a stratum which he estimates to be older than the remains of spruce found in Scandinavia, and he therefore considers that the spruce immigrated from Finland.

Considering the present and past extension of the spruce in Europe, one might be inclined to assume that the true home of this tree was Scandinavia, whence it was driven in the Glacial age, but this, it is now being demonstrated, was not the case.

Now, the spruce, in spite of its relatively late immigration, has in Sweden spread greatly, forming huge forests; and we might be disposed to conclude that in its turn it would extinguish other species. Experience from our forests goes to prove that this is really the case. Thus, from Southern Sweden we know from the researches of Dr. R. Hult in Blekinge that the spruce (except in dry localities) generally extinguishes the fir. And reports by Herr C. G. Holmerz and Herr Th. Örtenblad show that this is also the case in Norrbotten, where "the spruce in all more favoured localities wedges itself in between firs and birches, and finally exterminates its predecessors in occupation." Even the oak is ousted by the spruce. Prof. Elias Fries states that "the spread of our noblest foliage tree is being arrested in recent times. In a primæval spruce forest, where there is no more oak in the locality, I have found below an immense layer of moss oak trunks of such dimensions that I doubt whether there are their equal in all Sweden." In Blekinge also similar facts have been brought to light, and recently Prof. F. R. Kjellman expressed the opinion that "the oak flora formerly had a greater extension in our country, but has been thrust aside by the spruce."

Although the spruce is victorious over some species, it is unable to conquer the beech. Therefore there is little chance that the spruce will take possession of our southern counties.

In conclusion, as the result of the known palæontological facts respecting the immigration of our flora, we are able to express the opinion that the greatest part of the Arctic flora, as the inland ice melted, immigrated from the south; and that, of the various forest trees, the birch, aspen, and fir came by the same road, forming the country's oldest forests. From the south, too, we must assume that salix, mountain ash, mountain elm, hazel, lime, with accompanying shrubs, certain willows, guelder rose, *Rhamnus frangula*, *Cornus sanguinea*, and the hawthorn came, and from the same quarter, at a later period, the common ash, oak, and ivy. Furthermore, later still, the beech and the hornbeam, with accompanying shrubs, came from this quarter. The spruce and the beech both immigrated late, but the former from the east. These two forest trees are at present, in different localities, the two most favoured, the beech in the south, and the spruce in all other parts as far as the northern limit of coniferous trees. But the contest between the different species of trees is no longer undisturbed. For since man settled in the land a new factor has arisen, and this factor participates both directly and indirectly in the contest. Originally slight, his influence has grown greater and greater, and the time may come when he will be the arbiter as to the trees that are to form the forests of the future.

A. G. NATHORST.

Geological Survey of Sweden.

AGRICULTURAL EXPERIMENTAL STATIONS IN THE UNITED STATES.

THE following interesting information in reference to the establishment of agricultural experimental stations in the United States is given in a memorandum recently issued by the Agricultural Department of the Privy Council, and laid before Parliament. An "Office of Experiment Stations" has been instituted as a special branch of the United States Department of Agriculture. For the expense in connection therewith a sum of £2000 was voted by Congress in 1888, to enable the Commissioner of Agriculture to carry out Section 3 of the Act of Congress of March 1887, by which experimental stations were established. This said section provides that, "in order to secure, as far as practicable, uniformity of methods and results in the work of the said stations, it shall be the duty of the United States Commissioner of Agriculture to furnish forms, as far as practicable, for the tabulation of results of investigation or experiments; to indicate, from time to time, such lines of inquiry as to him shall seem most important; and in general to furnish such advice and assistance as will best promote the purposes of this Act. It shall be the duty of each such station annually, on or before February 1, to make to the Governor of the State or Territory in which it is located, a full and detailed report of its operation, including a statement of receipts and expenditure to the said Commissioner of Agriculture, and to the Secretary of the Treasury of the United States." In 1888 an Act was passed by Congress making an appropriation for the Department of Agriculture for the fiscal year ending June 30, 1889, and for other purposes, appropriating £119,000, including the £2000 specially reserved for the Commission of Agriculture, for the purpose of endowing and assisting agricultural and experimental stations throughout the country. Besides this sum, the several States have contributed £25,000, making a total sum of £144,000 given from public funds for the support of these stations. There are now forty-six of these stations in the United States, so that, taking an average, each station will receive over £3000 this year. It is said, however, that several of these stations have sub-stations under them, and that there are 370 trained men connected with the stations in the prosecution of scientific and practical agricultural experiments. The first agricultural experiment station in America was established in 1875 in Connecticut, and the next in California in the year following. In 1879 the well-known Cornell University Station was founded, which has done so much good work, and the equally valuable Wisconsin Station in 1883. No less than twenty-six stations were founded last year, in consequence of the inducements set forth by the Act of 1887. In a recent Report as to the organization of these experimental stations, a list of the staff of each is given, from which list a few examples may be taken to show the extent of work that is performed, or may be performed. At the Connecticut Agricultural Station there is a director who is a Master of Arts, a vice-director who is a Doctor of Philosophy, and a chemist. There are three other chemists who are Doctors or Bachelors of Philosophy, a mycologist, and a practical farmer in charge of grounds and buildings. The staff of the Dakota Station is still more extensive, consisting of a director, a superintendent of the farm, a superintendent of forestry and horticultural experiments, an entomologist, an analytical chemist, a veterinarian, an accountant and stenographer, and a librarian. Upon the staff of the Iowa Station there are two chemists, one for ordinary and one for special work, a botanist for ordinary and special work, an entomologist, a veterinarian, a horticulturist, and a practical farmer. The Cornell University Station staff comprises a chemist, veterinarian, botanist, and arboriculturist, a horticulturist, an entomologist, a cryptogamic botanist, besides an assistant in entomology, chemistry, veterinary science, and horticulture. Among the operations of these agricultural experiment stations are "fertilizer control," or the analyses of manures, the analyses of fodder and feeding-stuffs drainage experiments, feeding experiments with farm animals, observations on milk, the determination of injurious insects, with remedies against their attacks, fruit culture experiments, drinking-water analyses, ensilage experiments, meteorology, seed-testing, analyses of soils and rocks, the culture of various plants for fodder and corn, with other useful work.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 26.—M. Des Cloizeaux, President, in the chair.—On the molecular tactics of the artificial macle of Iceland spar produced by Baumhauer by means of a knife, by Sir William Thomson. The substance of this paper has already been communicated to the Royal Society of Edinburgh, and will shortly be published, under the title of "Molecular Tactics of Crystals," in the Proceedings of the Society. The author also contributes a paper on the equilibrium of atoms, and the elasticity of solids in Boscovich's theory of matter.—Note on the orbits of shooting-stars, and on stationary radiant points, by M. F. Tisserand. A calculation of the elements (mostly parabolic) of their several orbits leads to the inference that the meteoric showers encountered by the earth at different times of the year do not all emanate from the same radiating centre, but belong to different systems proceeding from quite independent radiant points. A series of essays based on the assumption that the orbits are not parabolic, but elliptic, lead to the same conclusion.—On the relations of atmospheric nitrogen to vegetable soil, by M. Th. Schlœsing. This is a reply to M. Berthelot's recent strictures on the author's negative results. These results are here maintained, and M. Schlœsing again argues on fresh grounds that there is no fixation of nitrogen by vegetable humus except through the actual process of vegetation.—Pathogenic properties of the microbes present in malignant tumours, by M. Verneuil. The author still adheres to the opinion already enunciated in 1883, that these parasites have nothing to do with the initial stage of boils, ulcers, cancer, and the like. At the same time he does not regard their presence as a matter of indifference, but admits that in certain cases they may themselves possess special pathogenic properties, in virtue of which they act on the system like septic poisons.—On the progress of the zoological station at Roscoff, by M. de Lacaze-Duthiers. The author speaks in satisfactory terms of the present condition of this station, and of the complementary establishment at Banyuls, which have now been placed in connection with the Sorbonne. The electric light, introduced at Roscoff by the aid of private munificence, is now in perfect working order.—The Eiffel Tower struck by lightning, by M. Mascart. A correct account is given of this occurrence, which took place on August 19, and exaggerated reports of which appeared in the daily papers. The conductor was struck, with the normal results, showing perfect communication with earth, and consequently complete safety of the structure from any danger on this score.—Observations with the pendulum effected in Russia, by General Steibnitski. The author reports that the Russian Imperial Geographical Society is now in possession of three Repsold pendulums, with which the latitude and longitude of Karmakul in Novaya Zemlya and Archangel, the two northernmost stations in European Russia, have been accurately determined.—Occultation of Jupiter by the moon, August 7, 1889, by M. Ch. André. The results are given of the three observations taken at the Observatory of Lyons by MM. André, Le Cadet, and Marchand. None of the satellites disappeared instantaneously, as is the case with stars of the same magnitude (seventh). The disappearance of satellites III., II., and IV. occupied 1″·5, 1″·1, and 0″·5 respectively, giving for their several diameters 0″·46, 0″·30, and 0″·15.—On the angle of polarization of the moon, by M. J. J. Landerer. A method is described by means of which this element has been determined at 33° 17′, a mean value resulting from eleven series of observations with probable error ± 7′. The same process is equally applicable to the planet Venus.—On the solar spots, by M. G. Spœrer. Besides some brilliant protuberances, the large spot visible from June 16 to 18 was observed on the last day at 10.43 a.m. at Potsdam. But a photograph of the same taken a few minutes before the observation shows no trace of the spot, which is replaced by an even depression on the solar rim, exactly where the spot had been observed. An explanation is suggested of this phenomenon.—Specific heat of aqueous vapour under constant volume, by M. Ch. Antoine. For Regnault's curves of the form $x = A + Mt_s - Nt_s^2$, the author substitutes functions of the temperature t and of the tension p, such as $x = B. + ct_s = \phi(p)$, deducing for aqueous vapour two determinations for specific heat under constant pressure and constant volume. Analogous formulas may be obtained for other vapours, such as ether, chloroform, acetone, benzene, chloride, and sulphide of carbon.—Papers were contributed by M. Léo Vignon on the action of water on stannic chloride; by M. G. Raulin, on the action of phosphates on the growth of cereals; by M. C. Timiriazeff, on the relation between the intensity of solar radiation and the decomposition of carbonic acid by plants; and by M. Armand Sabatier, on the zoological station at Cette.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Marine Aquaria: R. A. R. Bennett (Gill).—Narrative of an Explorer in Tropical South Africa; F. Galton (Ward, Lock).—The Mathematical Theory of Electricity and Magnetism; vol. ii. Magnetism and Electrodynamics: Watson and Burbury (Clarendon Press).—Bulletin of the U.S. National Museum; No. 34. The Batrachia of North America: E. D. Cope (Washington).—Bulletin of the U.S. National Museum; Contributions to the Natural History of the Cetaceans, a Review of the Family Delphinidæ: F. W. True (Washington).—Calcul des Probabilités: J. Bertrand (Paris, Gauthier-Villars).—Die Fossilen Pferde der Pampasformation: Dr. H. Burmeister (Buenos Aires).—Elementary Physiography, 2nd edition: G. Thom (Edinburgh, Thin).—The Eiffel Tower: G. Tissandier (Low).—Brain, July (Macmillan).—The Esclepiad, No. 23, vol. vi.: Dr. B. W. Richardson (Longmans).—Journal of the College of Science, Imperial University, Japan, vol. iii., Parts 1 and 2 (Tōkyō).

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