THURSDAY, JANUARY 9, 1879

AMERICAN SURVEYS AND EXPLORATIONS

ATTENTION has frequently been called in these columns to the progress of American exploration. Only a few months ago[1] reference was made to the want of concert among the different surveying expeditions, to the consequent loss of labour and reduplication of work, and to the desirability of consolidating the whole exploratory service under one connected organisation. It is satisfactory to know that an important movement in this direction is now in progress, and that Congress has called in to its assistance the advice of the most eminent scientific authorities in the States.

Readers of NATURE may remember that a few years ago (1874) a discussion was raised in Congress as to the alleged repetition of the survey of the same area of territory by independent expeditions, and that a committee of inquiry was appointed to take evidence on the subject and report. The result of that inquiry was a recommendation that the Engineer Department should be restricted to such surveys as might be necessary for military purposes; but "that all other surveys for geographical, geological, topographic, and scientific purposes should be continued under the direction of the Department of the Interior." It was easy to see from the evidence given before this Committee that a good deal of personal feeling had been evoked by the conflict of interests among the various surveying corps. The Engineer Bureau, in particular, with its well organised military equipment and its just pride in the large amount of exploratory work it had accomplished, seemed to resent the existence of the civilian expeditions as an infringement of its own proper sphere of operations. We may suppose that it was proportionately chagrined by the decision of the Congress Committee.

There was thus no great love between the rival surveyors in the beginning, and heaven seems to have decreased it on better acquaintance. With their plotting and counter-plotting, of which there has, no doubt, been plenty, we have of course nothing to do. Last summer the subject came up again before Congress. Representative Hewitt moved a resolution there, referring the question of the Geological and Geographical Surveys of the Territories for consideration and report by the National Academy of Sciences. It was known that double surveying had been carried on to a large extent, notwithstanding the information elicited and recommendations given by the Congressional Committee of 1874. One officer, indeed, was alleged to have duplicated surveys to the extent of more than 100,000 square miles, at a cost to the public exchequer of nearly half a million of dollars. The object of the resolution in Congress is said to have been to consolidate the power of the military surveys; but certainly nothing could be more impartial and sweeping than the law passed last June. It was to the following effect:—"The National Academy of Sciences is hereby required, at their next meeting, to take into consideration the methods and expenses of conducting all surveys of a scientific character under the War or Interior Department, and the surveys of the Land Office, and to report to Congress, as soon thereafter as may be practicable, a plan for surveying and mapping the territories of the United States on such general system as will, in their judgment, secure the best results at the least possible cost; and also to recommend to Congress a suitable plan for the publication and distribution of the reports, maps, and documents, and other results of said surveys."

The Academy, in accordance with this requirement, appointed a committee to consider the question. The weight of authority of this Committee may be judged from the names of its members: O. C. Marsh, James D. Dana, William B. Rogers, J. S. Newberry, W. P. Trowbridge, Simon Newcomb, Alexander Agassiz. The finding arrived at by such a group of men must command respect all over the Union, as it will on this side of the Atlantic. At a meeting of the Academy held in New York on November 6th, the result of the deliberations of the Committee was presented in the shape of a formal report, which, being approved and adopted, was forwarded to the President of the Senate on the 26th of the same month.

In this Report the various surveys of the public domain are broadly grouped into two divisions: 1. Surveys of mensuration; and 2. Surveys of geology and economic resources of the soil. Each of these divisions is discussed somewhat in detail.

1. Under the first group are included no fewer than five different and independent organisations: 1. The Coast and Geodetic Survey. 2. The surveys carried on by the War Department to the west of the 100th meridian. 3 and 4. The topographical portion of the work carried on by the two surveys under the Interior Department. 5. The survey for land-parcelling under the Land Office. Between these various kindred works no concert or co-ordination of any kind exists. In the language of the Report, "their original determinations of position are independent, their systems of surveys discordant, their results show many contradictions, and involve unnecessary expenditure." On the one hand the geographical reconnaissances of the Engineers and the Interior Department are too sketchy to serve for the subdivision of public lands; on the other hand, the land-parcelling surveys are of correspondingly slight topographical or geographical value. The National Academy insists that as all these surveys must be based upon accurate determinations of position, they can never be effectively and economically conducted until they are united into one system conducted under the same head. On a review of the powers and capabilities of the different surveying staffs, the Academy has come to the conclusion that the Coast and Geodetic Survey is, practically, best prepared to undertake the charge of the unified system proposed to be established. It recommends that this survey be transferred from the Treasury to the Department of the Interior, and that, with its modified and extended functions, it should hereafter be known as the United States Coast and Interior Survey, with a Superintendent appointed by the President, and reporting directly to the Secretary of the Interior. The duties of this branch of the public service, besides those of the present Coast and Geodetic Survey, should include a rigid geodetic survey of the whole public domain; a topographical survey, including detailed topographical work, as well as

[1] NATURE, vol. xviii. p. 634.

rapid reconnaissances like those now carried on by the War and Interior Departments; and, lastly, surveys for the parcelling of public land.

2. Having regard to the enormous area of territory yet to be explored and surveyed, its vast mineral wealth, its agricultural and pastoral resources, its stores of timber, its capabilities of soil, the Academy believes that the best interests of the country require that, for purposes of intelligent administration, a thorough knowledge must be obtained of the geological structure, natural resources, and products of these regions. It therefore recommends the establishment of an independent organisation, with a Director appointed by the President, to be placed under the Department of the Interior, and to be styled the United States Geological Survey. The duties of this Survey would include the investigation of the geological structure and of the economic resources of the public domain.

This consolidation of all the surveying work, sanctioned and paid for by Congress, would of course involve radical changes in some of the Departments. The Bureau of Engineers, in particular, would be required to give up all surveying work except what might be necessary for merely military purposes, and for such engineering operations as the rectification of rivers, irrigation and drainage, reclamation and protection of alluvial land. The various geographical and geological surveys west of the 100th meridian, now carried on by the War and Interior Departments, would be discontinued, though of course they would, in some cases, be resumed under the proposed new organisation.

Three distinct branches of the public service are thus proposed to be established for dealing with the public domain:—First, the United States Coast and Interior Survey, charged with the accurate mapping of the country; second, the United States Geological Survey, for the investigation of the geological structure and natural resources of the domain; third, the Land Office, having charge of the subdivision and sale of the public lands, and entitled, therefore, to call upon the Coast and Interior Survey for all necessary surveys and measurements, and upon the Geological Survey for all information as to the value and classification of lands.

Considerable liberty is proposed to be given to the chiefs of the two surveys as to the nature and extent of their publications. They are each to present an annual report of operations, and provision is to be made for the issue of such maps, charts, reports, discussions, treatises, and other documents as they may deem to be of value. Most liberal provision is likewise recommended to be made for the distribution of the reports of the Surveys. Besides the number of copies required by Congress for its own use, 3,000 copies are proposed to be published for scientific exchanges by the heads of the surveys and for sale. The special reports are to be issued in uniform quarto size, liberty being left to each director to choose such a form for his chartographic publications as shall combine the most effective style with the greatest economy. All specimens collected by the two surveys when no longer required for the investigations in progress are to be transferred to the National Museum.

Such in brief are the recommendations made by the National Academy in response to the requirement of Congress. That they are eminently wise and thoroughly practicable must be freely admitted by all capable of forming an opinion on the subject. It is simply impossible that things can go on as they are. Each one of the Surveys now in progress has done good work; several of them most admirable work. But work as good could be got with less labour and at less cost. This cannot be effected without combination; and the Academy has pointed out with great clearness and judgment how the combination may be achieved. It is not to be expected that changes of this kind can be carried out without irritating some of the individuals whose position is thereby affected. But save the severance of the Bureau of Engineers from all control of the surveys there need be comparatively little disturbance of the work now going on. Dr. Hayden, who with his staff has done so much in recent years for American geology, would doubtless take a high command under the new system; and it may be hoped that his position will be so secured as to enable him to devote his whole time to the scientific work for which he has shown himself to be so admirably qualified. Mr. Powell and his colleagues might continue their interesting and important Colorado investigations. To Mr. Clarence King fresh fields of research lie open where he may win laurels as bright as those he now wears. To all these officers in so far as they have at present geographical and topographical work to carry on, the allocation of all such duties of mensuration to a special geodetic survey should be a welcome relief, as it will set them free for their own special investigations. The Academy in its Report contemplates the possibility of officers, both of the army and navy, being desirous to volunteer for employment in these surveys, and recommends that when their services are not otherwise required they should be permitted to take part in the general survey. In this way a connection with the engineers might be re-established, and we may be sure that every engineer officer of capacity would be welcome, and would take a good position under the Department of the Interior.

The Report of the Academy, on being presented to Congress, was, on the 2nd ultimo, referred to the Committee on Appropriations, and ordered to be printed. As Congress rises at the beginning of March, some action may be expected to be taken on the matter before that date. It will be a subject for sincere congratulation among all well-wishers of American science and general progress, should the decision be in the direction pointed out by the National Academy of Sciences.

ARCH. GEIKIE

KERNER'S "FLOWERS AND THEIR UNBIDDEN GUESTS"

Flowers and their Unbidden Guests. By Dr. A. Kerner. With a Prefatory Letter by C. Darwin, M.A., F.R.S. The translation revised and edited by W. Ogle, M.A., M.D. (London: C. Kegan Paul and Co., 1878.)

THIS charming book is the record of an extension, in a somewhat different direction, of the researches of Darwin, Hermann Müller, and others, on the assistance rendered by insects in the cross-fertilisation of flowers. Attention has hitherto been directed almost exclusively to the contrivances by which insects, while feeding on or sucking the honey from the nectary, are almost compelled to be the involuntary agents in the transmission of the pollen from the anther to the stigma in a different flower of the same species. In almost all cases, however, it is only a comparatively small number of insects that would be adapted, by the size of their body, the length of their proboscis, and other points in their structure, to insure the cross-fertilisation of any particular species of flower. It is obvious that the visits of all other insects, which would consume the honey without aiding in the transmission of pollen, not only can be of no advantage, but must be positively injurious to the plant, by preventing the visits of the useful insects. It is this point that Prof. Kerner has studied with great industry and acumen; and he has compiled a number of most interesting illustrations of the contrivances presented in the structure of the flower, which not only force the useful insects to enter it in the particular way which shall be of most advantage to it, but keep out all others.

Dr. Kerner is dissatisfied with the vague use in botanical works of the words "self-fertilisation" and "cross-fertilisation," and proposes to substitute them by others which shall have a more definite meaning. He uses the term "autogamy" for the fecundation of a flower by the pollen from the andrœcium of the same flower; "geitonogamy" for the fecundation of a flower by pollen from other flowers on the same plant; and "xenogamy" for fecundation by the pollen from a flower on another plant; while the term "allogamy" would include the last two as contrasted with autogamy. The only previous attempt at a strict scientific terminology with which I am acquainted is by Delpino,[1] who distinguishes four kinds of fecundation: homoclinic homogamy, fecundation by pollen from the andrœcium of the same hermaphrodite flower; homocephalic homogamy, fecundation by pollen from the andrœcium of a different flower of the same inflorescence; monoicous homogamy, fecundation by pollen from the andrœcium of a flower belonging to a different inflorescence on the same plant; and dichogamy, fecundation by pollen from the andrœcium of a flower on a different plant. Prof. Kerner's terms will at all events be admitted to have the merit of being the simplest, and will probably be generally adopted in future.

It may be stated as a general rule that, while the visits of winged insects are beneficial, those of wingless insects are mostly injurious, since these consume the nectar without immediately afterwards visiting another flower so as to favour allogamy. Hence the majority of the contrivances which Kerner describes are for the purpose of excluding from the flower wingless insects, and in particular ants, the great enemies of flowers, and the aphides which attract ants. The flora of the mountain valleys of the Tyrol furnishes almost inexhaustible material for working out a problem of this kind; and of this material Dr. Kerner, who holds the position of professor of botany in the University of Innsbrück, has availed himself to the full.

Simplest among the contrivances for excluding useless, while admitting useful insects, are the more or less dense collections of hairs which cover up the entrance to the nectary in so many flowers, rendering the road inaccessible to very small insects, while presenting no obstacle to larger insects which can brush them aside or pierce them with their proboscis. Beautiful instances of this arrangement are described and illustrated in the work before us in the cases of *Anchusa arvensis*, *Lonicera alpigena*, *Veronica chamædrys* and *officinalis*, *Malva rotundifolia*, *Monotropa hypopitys*, *Menyanthes trifoliata*, and many others. Sometimes these coverings take the form of appendages to the corolla which protect the openings to the nectaries with flaps, as in *Gentiana nana* and *Soldanella alpina*. Again, access to the nectary by small insects is often prevented by the parts of the flower being bent, dilated, or crowded together, the same contrivances resulting also in forcing those insects which are useful to enter and leave the flower in such a way as to be of the greatest service in the transmission of pollen. Thus, in *Nigella* each spoon-shaped petal is hollowed out into a kind of pit, into which nectar is secreted in abundance. At the point where the handle of the spoon is continuous with the bowl the petal gives off an excrescence which covers in the whole nectar-cavity like a lid, closing it completely, and no insect can possibly rifle the nectar unless it be strong enough to lift up this lid. Ants are by this means entirely excluded, while the common honey-bee is able to lift the lid with ease, in doing which he must inevitably rub against either anther or stigma, according to the stage of development of the flower. In his "Entdeckte Geheimniss der Natur" (1793) Sprengel describes, with wonderful accuracy, the proterandrous flower of *Nigella*, and the mode in which the structure of the various parts secures allogamy by the agency of bees. He observed the flaps over the nectaries, but suggests no other interpretation of them than to serve as a protection against rain. A similar occlusion of the nectary against small insects is effected in *Cynoglossum* by the outgrowths from the mouth of the corolla, in *Campanula* and *Phytemna* by the strap-shaped lower parts of the filaments, in *Ranunculus glacialis* and many Mesembryanthemaceæ and Cactaceæ by the mass of crowded filaments, in *Pentstemon* by the fifth barren stamen.

These protective and prohibitive arrangements are frequently placed, not inside, but outside, the flower, one very common form being the hairs, glandular or not, with which the calyx, bracts, or upper part of the stem are so frequently clothed; and the purpose is still more effectively answered when the glandular hairs or the stem itself exude a viscid secretion. In such plants as *Cistus ladaniferus*, *Listera ovata*, *Geranium sylvaticum*, *Euphrasia viscosa*, *Lychnis viscaria*, and a large number of species belonging to the orders Caryophyllaceæ, Saxifragaceæ, Labiatæ, and Scrophulariaceæ, it is almost impossible for the flower to be visited by any but winged insects. Prof. Kerner believes that the numerous insects which are found adhering to the viscid peduncles and stems of these plants are not digested so as to serve the plant with nutriment, but that the object of their destruction is simply to prevent their reaching and rifling the nectary. An explanation is thus offered of the well-known fact that the same species will frequently assume a hairy habit when growing on land, and a glabrous habit when growing in water, water-plants being almost invariably destitute of hairs. An admirable instance of this

[1] *Nuovo Giornale Botanico Italiano*, vol. viii., 1876, p. 146.

law is furnished by *Polygonum amphibium*. Kerner shows that the nectaries of this plant are entirely unprotected against the incursions of "unbidden guests." When growing in water this is no disadvantage, because none but flying insects can reach the flower. But when growing on land the nectar would be liable to be rifled by small creeping insects that would carry it away without performing any compensating service to the plant, and in such circumstances an innumerable quantity of glandular hairs make their appearance on the epidermis of the leaves and stem which effectually bar the way against the unwelcome visitors. "If the ground on which a *Polygonum* has grown for years in dryness, so as to have become covered with these trichomes, again be flooded, and the stems and peduncles again therefore be encircled with water, the trichomes with their viscidity disappear, and the epidermis again becomes smooth and even." I find this statement difficult to reconcile with a dictum laid down further on in the volume—and, as it appears to me, laid down hastily without sufficient warrant—that "the so-called process of 'adaptation' is never a direct one, never comes simply in response to a want. In other words, external conditions can never occasion an inheritable change of form, whether advantageous or the contrary, can neither determine the development of an organ nor its abortion."

Although glandular hairs or viscid secretions are the most common contrivances for preventing the access to the nectary of useless insects, they are by no means the only ones. The same object is attained by the prickles which cover the upper portion of the stem or the peduncles, and the spines into which the involucre of many Compositæ is converted. The waxy or even the glabrous epidermis in some plants prevents creeping insects from reaching the flowers. Even the latex or milky juice of such orders as Euphorbiaceæ, Convolvulaceæ, and Cichoriaceæ is pressed into the service. Kerner placed various kinds of ants on plants that were full of milky juice, such as the common lettuce. No sooner did they reach the uppermost leaves or peduncles than their feet cut through the tender epidermis of those parts, causing the latex to flow, which immediately glued the little animals to the stem so that they were totally unable to escape, and most of them miserably perished. The extra-floral nectaries, on the leaves or other parts of the plant, of *Viburnum tinus* and *opulus*, *Impatiens bicornis*, and many Leguminosæ, serve a similar purpose of diverting creeping, but not winged insects from the flower; since an insect crawling up the stem would always reach these secretions of nectar before the flower.

Some plants have to be protected from animals of a larger size, ruminants and other herbivorous quadrupeds. Some are altogether so protected by their prickly stem and leaves, or by the nauseous or unwholesome secretions of their tissues. But unpalatable secretions are much more common in the petals than the leaves; and with many plants the leaves are eagerly devoured by grazing animals or by caterpillars, while the flowers are left entirely untouched. While the comparatively large size of the flowers of alpine plants no doubt has for one object the attraction of hymenoptera and lepidoptera from a distance, the large area occupied by them in comparison to the leaves—the very character which renders many of them such favourite ornaments of our rockeries and flower-beds—doubtless also serves to protect them from destruction by goats and other mountain quadrupeds.

Space does not allow me even to refer to many other singular and interesting relationships pointed out by Prof. Kerner. It is of course quite possible that further examination may modify some of his conclusions in their detail. For example his belief that the main object of the viscid secretion on the leaves of *Pinguicula* is to prevent the access of creeping insects to the flower hardly appears consistent with the fact that most species of the genus flower early in the spring, while the secretion continues its activity through the summer and autumn. But the book is a perfect mine of original research, and is indispensable to all who are interested in the many problems connected with the fertilisation of flowers.

Dr. Ogle's translation is, with but little exception, easy and graceful. His editorial notes are useful, and he has adopted the praiseworthy practice—since the work is intended for non-scientific as well as for botanical readers—of explaining in foot-notes the meaning of technical terms used by the writer. In a future edition this practice might with advantage be extended. Such a term as "epiblasteme" does not carry its own meaning with it; and even botanists not well read up in recent literature would be puzzled by it. Or perhaps a glossary would be more useful. Three large-sized lithographic plates crowded with detail add greatly to the lucidity of the descriptions. ALFRED W. BENNETT

FLAMMARION ON DOUBLE STARS

Catalogue des Étoiles Doubles et Multiples en Mouvement relatif certain. Par Camille Flammarion. (Paris: G. Villars, 1878.)

IN this compact volume of less than two hundred octavo pages M. Flammarion has collected together the large number of measures of double and multiple stars, exhibiting change in the relative positions of the components, which have been made by various observers since the time of the father of double-star astronomy, Sir W. Herschel. Those who have been occupied in the study of this branch of the science will be well aware of the difficulty and trouble attending the preparation of a complete history of any of these objects from the measures being scattered through a great many astronomical works, some of them not always easily accessible, and M. Flammarion has not yet attached his name to any volume which is likely to compare with the present one in usefulness.

The author's authorities are about one hundred in number, and he refers to them by abbreviations, a list of which precedes his catalogue, but it is to be regretted that he has not also prefixed the titles of the volumes whence the various measures have been taken, and the more so as there are indications that the original authorities have not been invariably consulted. Thus a number of Capt. Jacob's measures made with the Lerebours equatorial at Madras and published in the first catalogue in the volume of Observations 1848-52, are omitted in M. Flammarion's lists, though he has others which appear in the second catalogue in the same volume, formed after the substitution of a new object-glass. In the case of π Lupi, where he regrets "que les étoiles australes soient si négligées," he has omitted all Capt. Jacob's measures subsequent to 1848, and as instances where some measures are wanting, may be mentioned γ Argûs, ρ Herculis, δ Herculis, τ Ophiuchi, 70 Ophiuchi, ϵ Equulei 61 Cygni, θ Indi, &c., &c.

After exhibiting the measures of each object, M. Flammarion, in the great majority of cases, appends his own conclusions with respect to the cause of the relative changes of position, which have generally been carefully considered, though there are some few in which we should hardly be disposed to follow him. But the reader having nearly all that is known of the different objects before him, in M. Flammarion's summary, will be able to form his own inferences. If an observer he will be guided thereby to a selection of objects most worthy of his attention, or most requiring further measures for the elucidation of the cause of altered position.

In a provisional examination of the volume ample proof is afforded of the care taken by the author in his work, which has no doubt been as he describes it long and laborious. There are a few such oversights as H_2 3678 for H_2 4087; and under Procyon, misled by a measure of Secchi's in 1856 as printed, he refers to a companion at 83°·6 and 33″·16; this measure, however, really belonged to Powell's distant companion, and instead of 33″·16 the distance should be 331″·6, as it is given in another page of the same volume of Memoirs of the Roman Observatory. There is no reference to some of Argelander's determinations of proper motion, as in the case of a distant companion of γ Leonis, upon which M. Flammarion enters into some detail. Omissions like this, however, are perhaps unavoidable in the first preparation of such a work, but the author will doubtless have his attention called to them, and will be able to make his second edition a still more inclusive manual of double-star astronomy, than even this first impression.

Through the kindness of Leverrier, M. Flammarion was allowed the use of one of the equatorials at the Observatory of Paris during the year 1877 for the re-measurement of a number of the double stars; these measures applying to about 130 objects are given at the end of the preface to this volume: amongst them we note the close pair of 40 Eridani, a rapidly revolving star which has not received the attention it deserves from observers.

M. Flammarion's work will doubtless soon find its way into the hands of every one who is interested in the double and multiple stars, and he will certainly experience the satisfaction of receiving the well-earned thanks of many amateurs who have no convenient access to large astronomical libraries and to whom his volume will be a valuable *vade-mecum*.

OUR BOOK SHELF

The Mollusca of the Firth of Clyde; being a Catalogue of Recent Marine Species Found in that Estuary. By Alfred Brown. (Glasgow: Hugh Hopkins, 1878.)

ALTHOUGH the recent mollusca of this district have during the last few years received a good deal of attention, especially from the labours of M'Andrew, Barlee, and Merle Norman, still the various memoirs detailing the results of these labours were only to be found widely scattered through a number of scientific periodicals, and Mr. Brown has in this neatly printed work given us not only a *résumé* of the labours of the naturalists we have referred to, but also of all those who have collected on the Firth of Clyde, and joined these to the labours of Mr. David Robertson and his own. The result is, so far as the testaceous mollusca go, a large and apparently accurate catalogue, which will show not only what has been done but also among the nudibranchs and cuttle-fish what is yet to be done. The notes under the heading of Habitat in this catalogue are often most interesting, giving details not only of the exact localities for the species, but notes also of their local names.

Wanderings in Patagonia; or, Life among the Ostrich Hunters. By Julius Beerbohm. Map and Illustrations. (London: Chatto and Windus, 1879.)

THE title of this book is somewhat misleading, as the author's "Wanderings" were of a very limited extent, embracing only a small portion of the south-east coast region of Patagonia. Its most important feature is the account given of the life of the ostrich-hunters, and it adds little to our knowledge of Patagonia in addition to what has been told us by Musters and the one or two others who have really "explored" more or less of the wild region. The author's story is pleasant to read.

LETTERS TO THE EDITOR

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

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

Tempel's Comet

THE well-known comet- and nebula-finder of the observatory of Arcetri, Tempel, has just made an observation of great interest in reference to his Comet No. II. of 1873, which, as astronomers know, has an orbit between Earth and Jupiter. It has no tail, but a nebular surrounding, which Tempel observed to be gradually diminishing in luminousness without losing bulk, and finally has entirely disappeared, leaving the comet perfectly distinct, but with a slight scintillation or rather an appearance of being composed of several masses having motion in the rest of the nucleus; probably an optical effect due to our own atmosphere, but which is at all events seen quite distinctly enough to make it certain that the disappearance of the nebulous surrounding is not due to failure of the telescope to show it.

The comet was last observed on December 18, at 6h. 53m. 12s. mean time of Arcetri in Right Ascension 23h. 3m. 14·15s., and in South Declination 19° 15′ 54″·8. It was seen on the 21st but briefly, and no observation could be made. Since then the continually cloudy sky has prevented it from being seen, but Tempel is confident of being able to see it through January. It is now amongst the asteroids.

W. J. STILLMAN

Florence, January 1

The Cosine Galvanometer

IN NATURE, vol. xix. p. 98, my name appears in a way that might lead the reader to infer that I was the inventor of the "cosine galvanometer." My knowledge of this useful instrument was derived from Prof. Trowbridge, of Cambridge, U.S., who described it in 1871 in the *American Journal of Science*, vol. cii. p. 118. In my "Physical Manipulation" I omitted to mention Prof. Trowbridge's name, supposing that his connection with the instrument was too well known to render it necessary.

EDWARD C. PICKERING

Harvard College Observatory, Cambridge, U.S., December 20, 1878

Force and Energy [1]

II.

IN passing it may be noticed that the plus sign thus deduced for a tensive force is otherwise convenient because tension results in a positive increase of the dimensions in the direction of the tension of the body through which the tension is transmitted.

[1] Continued from p. 196.

With regard now to the investigation of the equilibrium or the acceleration of momentum of a body founded on a knowledge of the forces acting upon it, that is, of the various rates of transference of momentum between it and other bodies through its surfaces, we must evidently give signs to the various surfaces of the body, surfaces which are parallel, and face opposite ways, being given opposite signs. If we multiply the transferences of momentum taking place through the different surfaces each by the sign of the surface through which it takes place, and add all these products together, the sum will be the acceleration of momentum of the body. Thus two equal tensive forces acting through parallel opposite faces would keep the body in balance. Or two numerically equal forces, the one tensive and the other compressive, acting through parallel surfaces facing the same way, would also keep the body in balance. If the phrase "intensity of force" be used to mean the force per unit area through any surface, so that it is simply a generalisation of the two common phrases "intensity of pressure" and "intensity of tension;" then if each small element of the surface of a body be given its proper sign and multiplied by the intensity of force through that small element, this force being also given its proper sign, and if all such products for the whole surface be summed up, the total will be the acceleration of momentum of the body. The direction of this acceleration will be shown by the sign of this total, the sign having reference to the relative position of the surfaces which have been arbitrarily called positive and negative. Thus let two tensions, *i.e.*, positive forces, be applied to two parallel opposite faces, and let the force applied to the positive face be greater than that applied to the negative face; then the body will suffer a positive acceleration of momentum; that is, an acceleration in the direction from the negative face towards the positive face. The faces perpendicular to the positive and negative faces must be given the signs $+\sqrt{-1}$ and $-\sqrt{-1}$.

Thus a pair of positive forces applied to faces with the signs $+1$ and $+\sqrt{-1}$ cannot possibly balance each other. But a positive force applied to a $+$ face can be balanced by a tangential, or shearing, force applied to a $\sqrt{-1}$ face. Because the shearing force has either the sign $+\sqrt{-1}$ or $-\sqrt{-1}$, and multiplied by the sign of the face, gives either $+1$ or -1, as the sign of the product. Surfaces oblique to what is chosen as the positive direction must be considered as partly scalar and partly vector, as also forces oblique to the surfaces through which they act, or rather oblique to their direction of transmission. Oblique surfaces must be multiplied by oblique forces according to the ordinary rule of vector multiplication. This system of notation requires no further explanation, I think, to those who are likely to approve of it.

It has become lately a common habit to look upon those things which are conserved, that is, those which have an enduring existence, as objectively real; while those which may come into existence and go out of it again are considered as objectively unreal. Whether this is a correct philosophic habit or not, it has certainly tended to create suspicion as to the objective reality of all mechanical quantities. A gradually extending recognition of the relativity of these quantities is apt to lead on to a reluctant apprehension that all so-called physical facts are mere formal logical deductions from arbitrary definitions. The dark shadow of distrust first fell upon momentum because the fact that it is distinctly a relative quantity is most easily recognised, and thus became earlier a part and parcel of our familiar ideas. Then somebody suddenly recalled to mind the distinction, according to definition, between external and internal kinetic energy, and found that the external kinetic energy which it had been fondly hoped had some lingering flavour of the ABSOLUTE still clinging to it, was no more than a part of the internal kinetic energy of a larger group of bodies; and it became clear at a glance that energy, that grand ABSOLUTE REALITY which, being once borne into existence by triumphant modern science is now far too carefully conserved by its enthusiastic worshippers to allow of there being any risk of its dropping again out of existence, is just as purely relative in its nature as the velocity which has to be squared in order to calculate its amount. It had been thought that because a velocity has a direction and the square of a velocity has no direction, therefore we might calmly and fearlessly contemplate the total or partial destruction of momentum, steadfast in the assurance that energy would still live for us. And thus with much waiting in fluttering hope and trembling fear upon the brink of the Unseen Universe, and becoming impatient at the non-arrival of any clear intimations of immortality for ourselves, or for energy, or even for matter itself—which is clearly neither more real nor more unreal than her faithful spouse energy—a cloud of dismal despair seemed to be settling on the heads of the scientific nations, when a stern but cheering voice was heard from Munich bidding us be satisfied with our finite human faculty of perceiving relations only, and promising us that, if we would only not aspire to divine knowledge of the absolute, we might KNOW even now and also hereafter.

While admitting fully the relativity of all the physical facts which we may learn, I think it would be very unfortunate if we were to allow ourselves to confuse this with the idea that all mechanics is a mere phantasmagoria conjured up by a process of formal logical deduction from a basis of abitrary definitions. The clearest exponent of this theory of formality in mechanics that has come to my notice is Dr. V. A. Julius, in his letters on "Time" to NATURE, vol. xvi. pp. 122, 420. The argument may be thrown into the form of four short propositions and a conclusion, all of which are derived by purely formal reasoning from the ordinary definitions of the various quantities involved, and which a friend of mine pretends make out a "clear demonstration of the utter absurdity, futility, and falsity of all mechanics."

1. All motions and velocities are simply relative. Within a given isolated system, nothing with reference to the motions of its parts can be known beyond the motions of these parts relatively to the centre of inertia of the system.

2. Relatively to any other system, or single body, the velocity of the centre of inertia of this first system is, by definition, simply the mean of the velocities of its parts. The sum of the velocities of the parts relatively to the centre of inertia of this first system is, therefore, always zero.

3. Within one portion of this system, therefore, there cannot be any loss of average velocity without there being a simultaneous equal gain of average velocity in some other portion.

4. The changes which can possibly take place in the system with regard to velocity consist, therefore, in balanced exchanges of relative momentum between its parts, and, therefore, the equality of "action and reaction"—whether calculated with reference to rate of transference of momentum, or with reference to rate of transference of energy, *i.e.*, rate of doing work—is a purely formal deduction from the definition of the centre of inertia.

Conclusion. A purely formal deduction from an arbitrary definition is just as likely not to agree with reality as to agree with it. Q.E.D.

The fallacy of the argument lies in the artful omission of a few words in 3, which are necessary to make the meaning quite explicit. At the end of 2, the sum set equal to zero is that of the velocities *relatively to the centre of inertia of the system* itself. These, therefore, are the velocities referred to in 3. Therefore, in 4, the exchanges of momentum that are balanced are those of momentum measured relatively to the centre of inertia of the system itself; and it does not at all follow, by *pure logic*, that such a balanced exchange of momentum relatively to this centre does not produce an acceleration of velocity of the centre of inertia relatively to some body outside this system. Of course, if we add this outside body to the first system, then pure logic will compel the exchanges of momentum throughout this new combined system and measured relatively to the centre of inertia of the new combined system to balance. But pure logic does not *necessitate* the exchange of momentum within one part of the system relative to the centre of inertia of that part being unaccompanied by a simultaneous exchange of momentum between that part and some other part, or every other part. Thus the fact of conservation of momentum is not, that when two bodies exchange momentum, the amounts lost and gained measured relatively to the centre of inertia of the two, are numerically equal,—that would be a mere truism—but that the amounts lost and gained measured relatively to a third body are equal to each other. This latter is a physical fact, only to be proved by experiment, not by logic. The statement that action and reaction between two bodies are equal, does not mean anything in particular; but the statement that the action of a force between two bodies does not accelerate the velocity of their centre of inertia relatively to a third body is a statement of experimental fact. The mechanics of a system of two bodies might be built up by means of formal reasoning alone; but not that of a system of three, or of more, bodies without the experimental establishment of the law of conservation of momentum.

And the more complicated the system be, the larger the number of possible combinations of three bodies within it, the greater is the number of experiments or observations we can make to prove that the conservation of momentum is a general physical fact. The larger the number of such observations becomes, the further removed is the doctrine of the conservation of momentum from the character of a logical deduction from definitions.

Still, of course, the doctrine has only to do with relative velocities and relative accelerations of velocities. It loses, however, none of its reality and truthfulness on account of this. Why should not relations be capable of being real, even if not permanent? We are indeed incapable of conceiving anything as real which does not owe its reality in our conception simply to its relations to other things. If objective reality is in any way the opposite of relativity, then, certainly, so far as our knowledge goes, there is no such thing as objective reality. Our notions of momentum and of force, then, are relative to three bodies, and not to two bodies, and this seems to me to be an important point. The ELEMENTARY notion of momentum derived from DEFINITION is relative to TWO bodies only; but the PRACTICAL notion derived from EXPERIENCE is relative to three bodies at least, or to a complicated system of bodies. It should not be forgotten that the physical realities among which we live owe their existence to the complexity of nature. Throughout the complexity there are certain simple invariable relations, and these are the physical laws of nature. The law of conservation of momentum is this: the momentum of one system relative to another system remains unchanged by exchanges of momentum between the parts of the former system. Otherwise stated it is: exchanges of momentum may and do take place between the parts of a system without these exchanges being necessarily accompanied by an exchange of momentum between this system and any other system.

Energy is, of course, a quantity of as relative a character as momentum, although its relativity is not of just the same kind. Energy in general is usually defined as the power of doing work. Curiously enough this definition is frequently followed closely by the statement that a system may possess a very large amount of energy, and yet if there are no differences of potential within it no work can be done by it. The correct statement of what is meant by this last has often been given, viz., that in this case no work can be done by one part of the system upon another part of the same system. But still more often is the inaccuracy indulged in of saying that energy of one kind or another may be transformed into work. Now work is not energy and has no kind of similarity to energy, and therefore energy can never be converted into work. When energy is transferred from one body to another the first does work upon the second, the amount of work done being measured by the amount of energy transferred. The rate at which energy is transferred is the rate of doing work, or the horse-power. The doing of work or more shortly WORK, is the transference of energy from one body to another, but is not the energy itself. The confusion has never entered into the practical use of the word "work," which has always really been applied in the sense here explained, although very probably a good deal of confusion of ideas among both practical and theoretical men, may have been caused by the above noted incorrect statement that energy and work are convertible. The confusion is of the same sort as if we were to use the word force in the sense I have advocated and confuse it with acceleration of momentum. During some transferences of energy there is an invariable transformation of energy. If during the transference, the whole of the energy transferred is also simultaneously transformed, then the rate of doing work is also equal to the rate of transformation, and the amount of work done is numerically equal to the amount of energy transformed. But the phrase "work done" is only used when transference takes place. When a portion of one kind of energy in a body is converted into energy of another kind without any energy leaving the body, it is not the custom to say that work has been done. Work is only done by one body upon another, so that work is the TRANSFERENCE, not the TRANSFORMATION of energy. To say that so much energy has been spent in doing an equivalent amount of work is a convenient and quite allowable mode of saying that this amount of energy has been transferred from the working body without specifying what has become of the energy; that is, without specifying into what other body the energy has been transferred, and without specifying in what form the energy has appeared in the other body. But to say that the energy is converted into work is quite a different thing, and altogether wrong.

When a body possesses in two parts of it two quantities of heat at two different temperatures, the amount of work which the one part has the power of doing on the other in consequence of this difference of temperature is not nearly equal to the whole amount of heat energy in the two parts. Thus the energy in a body is not the power measured quantitatively, possessed by its parts of doing work on each other.

If in a collection of bodies there be a certain one body with a certain amount of kinetic energy, calculated from its velocity, relative to the centre of inertia of the group, that one body might deliver up the whole of this kinetic energy by direct impact upon another body which had zero velocity relative to that centre of inertia, provided these two bodies were exactly alike in certain particulars as to mass and shape. But if there did not exist in the group any body which had this particular relation of shape and velocity to the first, then this first could not possibly deliver up all its kinetic energy, so as to get its velocity relative to the centre of inertia of the whole group reduced to zero. It is thus clear that the internal kinetic energy of a collection of masses is not measured by the amount of kinetic energy calculated from the velocities relative to the centre of inertia of the collection that can be transferred from one part to another.

Also, if another body, or another group of bodies, existed apart from this first group, and possessed a velocity of centre of inertia either zero, or of any other value, relative to the centre of inertia of the first group, the kinetic energy of this first group, measured either relatively to its own centre of inertia, or to that of the other group, or to the centre of inertia of the two combined, could only be wholly transferred to this second group, provided that this second group had very special and very ingeniously contrived relations with regard to mass and configuration to the first group. Thus the kinetic energy of any collection is not measured by the power it may possibly have of doing work upon bodies outside the collection. And quite evidently the same may be said of any other kind of energy possessed by the body.

For each kind of energy we have more or less accurate means of comparing quantitatively different amounts of that kind of energy, and thus of measuring the amount of that kind of energy possessed by a body in terms of the quantity which is adopted as unit of that kind of energy. We have also means of converting different amounts of any one kind into most other kinds of energy; and since in several carefully-made experiments upon the conversion of different kinds of energy there has on the whole been a very fair agreement in the ratios furnished by these experiments between the adopted units of the different kinds, we have come to believe in the truth of the law of conservation of energy—the more especially since this belief is supported by theoretical reasoning based on the hypothesis of the truth of the conservation of momentum. This latter theoretical reasoning, however, we have, hitherto, at any rate succeeded in applying only to transferences of kinetic energy of visible motion, and to the thermodynamics of perfect gases.

But taking this principle of conservation of energy for granted as true, we have the means of measuring the amount of energy of any kind possessed by a body in terms of the adopted unit for kinetic energy of visible motion.

ROBERT H. SMITH

(*To be continued.*)

The Unseen Universe—Paradoxical Philosophy

WILL you permit me to ask through your columns how the idea of the authors—that the present universe is developed out of our unseen universe, which unseen universe is itself developed out of another, and so on in an endless vista up to the unconditioned—works when applied to the present universe as itself developing a lower universe?

The present universe must be a conditioning as well as a conditioned universe, or there would be a breach of the principle of continuity, and there must, on the same principle, be an endless vista of such lower universes.

Have we any hint of any lower universe? Ought we not to have more than a hint? Ought we not to be fully conscious that our own universe is developing and sustaining such a lower universe, to the living intelligent beings in which we are, in fact, supernatural agents, as the angels in the universe above us are to ourselves?

I think that the authors have expanded their idea in one direction only, and I have not seen any reviews of their books applying this idea in the other direction. If, however, this application has been made, I shall be glad to be referred to the passages containing it. W. A. T. HALLOWES

New University Club, St. James's Street, S.W. January 4

Atmospheric Electricity

THE traces afforded by the self-registering electrometer at this observatory show that the conditions of the atmospheric electricity at Kew were very similar during the recent frosts to those observed at Montsouris by M. Descroix. We have, however, in the automatic instrument the great advantage of continuous registration, and therefore our information is not limited to the results afforded by seven observations daily.

The whole period of the frost was characterised by extremely high tension which with us averaged and frequently exceeded the amount which sufficed to derange the French instrument.

The absolute maximum tension recorded equalled 600 volts, and occurred about 4 P.M. on December 16.

The most noticeable feature in the curves of electrical disturbance during the period is that of the daily range of the instrument having attained a maximum usually between 8 to 10 P.M., the tension reaching over 400 volts at the time on the 17th, 18th, and 21st, and over 500 on the 22nd ult.

The fall in tension on the 25th was irregular and the value became almost zero at 6 A.M. on the 26th, for the whole of which day it continued low. Negative electricity was recorded for the first time from 1 to 3 A.M. on the 29th.

Undoubtedly the value of the tension of the atmospheric electricity, as measured by the Thomson electrometer is, as M. Descroix states, only a relative one. We have determined experimentally that with the same instrument the indicated tension is largely influenced by the distance of the nozzle of the water-dropping collector from the wall of the building in which the instrument is placed, and in accordance with a suggestion of Sir W. Thomson, we replace during the passage of thunderstorms our ordinary discharge-tube by a very short one, so as to get the scale of tensions within the range of the electrometer.

Kew Observatory, January 6 G. M. WHIPPLE

Electrical Phenomenon

I HAVE just read in NATURE (vol. xix. p. 182) an account of a strange electrical phenomenon observed at Teignmouth. In connection with it the following incident may be of some interest:—When in Switzerland, not long since, I made with some friends the ascent of Monte Rosa. The weather was unsettled, and on gaining the summit we saw a thunderstorm advancing in our direction from the Italian valleys, and not wishing to turn ourselves into lightning-conductors we deemed it wise to retire from the summit. We had retreated a very short distance along the *arête* when the storm-clouds swept up upon us; the fine snow fell so thick that we could hardly see one another, and we were all suddenly attracted by a peculiar ticking or fizzing from our hair; when I held up my axe the ticking was most distinctly heard from the top of it. The thunder ceased, and we felt that we were acting as points, through which the ground electricity was flowing off into the cloud; if it had been dark, the bluish light observed at Teignmouth might have been visible.

As at Teignmouth, so on Monte Rosa; it was freezing hard when the phenomenon was observed. W. S. GREEN

Alta Terrace, Monkstown, Cork

Time and Longitude

As the questions I propounded under this head in NATURE, vol. xviii. p. 40, have been again alluded to by Mr. E. L. Layard, I may remark that they receive a complete answer in the "Geographical Reader," by C. B. Clarke, M.A. (Macmillan and Co., 1876). At p. 19 he says: "At the town of Sitka, in Alaska, half the population are Russians who have arrived from Russia across Asia; half the population are Americans who have arrived *viâ* the United States. Hence, when it is Sunday with the Russians it is Saturday with the Americans; the Russians are busy on Monday while the Americans are in church on Sunday to the great interruption of business."

It is evident, then, that our new year first commenced in Alaska at 9 A.M. Greenwich time on December 31. Each of our days commences at the same hour and lasts forty-eight hours; the year exists for 366 days. LATIMER CLARK

January 4

Magnetic Storm of May 14, 15

THE magnetic storm of May 14, 15, which was observed simultaneously in England, China, and Australia, and which made itself felt in the telegraph wires of Persia and India, was also perfectly observed in America. Mr. G. F. Kingston, director of the government observatory at Toronto, Canada, has kindly forwarded to me a tracing of his magnetograms, and I find that all the principal inflexions of the declination, as well as of the components of the intensity, bear a striking resemblance to those recorded at the Stonyhurst observatory. The correspondence between the two vertical force curves on May 14 is very remarkable for such distant stations. Comparing the times of the principal minimas in the V.F. trace, and of the chief maximum of the declination, we have the following results in Toronto mean time:—

	Principal V.F. min. P.M.		Secondary V.F. min. P.M.		Decl. Max. P.M.
Toronto Observatory	6 17	...	4 0	...	6 39
Stonyhurst Observatory	6 42	...	4 20	...	6 54
	0 25	...	0 20	...	0 15

The disturbing force would thus appear to have been felt somewhat earlier in Canada than in Europe.

The extent of the extreme oscillation of the V.F. magnets cannot be compared, as that at Stonyhurst was too sensitive, and was consequently thrown off its balance; but the rapid movement of the declination needle immediately preceding the maximum was almost identical in England and in Canada, the Stonyhurst curves showing a rise of 28′ 39″ in less than twenty minutes, and that of Toronto an increase of 26′ 53″ in the same time.

It is important to note that I have used the terms maximum and minimum in reference to increase and decrease of ordinate, but it so happens that an increase of ordinate signifies a decrease of H.F. and V.F., and also of W. declination in the Toronto curves, whilst it shows an increase of all these elements in the magnetograms of Stonyhurst. S. J. PERRY

Stonyhurst Observatory, December 28, 1878

Blowpipe Experiment

I BEG to inform you of the following curious results which may be considered of sufficient interest to lead to further investigation of the subject.

Having received a quantity of blowpipe charcoal from Freiberg, about two months ago, I placed two sticks in a "stoneware" jar full of pure water in order to saturate them therewith, so that small squares cut with a saw and placed on aluminium plate as a support, might stand the blowpipe heat longer. I also found that thus treated there is little or no black sawdust, which dirties the hands, &c., more than anything else in blowpipe operations.

Having also placed in the same jar of water two "aluminium spoons" (thick rods about five inches long), I was surprised to find that after the charcoal had sunk to the bottom on saturation, the aluminium rods were covered with semi-opaque roundish *crystals* (part being perfectly transparent) near the surface of the water, and also at the very bottom where the spoons rested on the jar.

Thinking the crystals might be due (although I could not tell how with such a deliquescent substance) to some phosphoric acid I had previously fused upon the aluminium spoons, I cleaned them thoroughly and placed them in fresh pure water with the charcoal about a fortnight ago, and they are again covered with the same kind of crystals. I now carefully scraped the crystals off the aluminium rods with a penknife and placed them on an agate slab, where, when dry, they had a perfectly white, sugary appearance, with some minute transparent fragments. Taking up some of these opaque white fragments upon a hot bead of boric acid, I submitted them to the action of the blowpipe, and found—.

(*a*) That they at first emitted a slight yellow pyrochrome, so that they could not be due to *potash*.

(*b*) The green pyrochrome of the boric acid was unaltered (no *soda*).

(*c*) The substance floated in the bead in bluish-white, fat-like, amorphous fragments like alumina or opaque silica as *tabasheer*, but—

(*d*) On continued heating, the fragments gradually disappeared, leaving bubbles, until in half an hour, with fresh boric acid, there was simply a transparent bead left; exactly the behaviour of minute fragments of diamond in boric acid.

Silica is absolutely, and alumina nearly, insoluble in boric acid before the blowpipe.

One conclusion, therefore, seems this: that a slow solution of charcoal in the water takes place, and that crystals of carbon are deposited upon the aluminium.

London, December, 1878 W. A. Ross

Observations on the Microphone

With regard to an explanation of the action of the microphone I observed a fact which, though it was already known from some anterior experiments with strong galvanic currents, has not been remarked, as far as I know, with this instrument. On connecting the current from six Grove cells with the microphone (the telephone not being in the circuit) composed of the three carbon rods, the vertical one assumes a vibratory motion between its supports, which causes a very audible sound, especially when placed on a sounding-board.

I think this experiment may serve as another illustration of the well-known fact, discovered by Ampère, of the repulsive action between the subsequent parts of a rectilinear current. Most of the experimental proofs bearing on this point leave some doubt as to a true demonstration, because a dilatation from the heating effect of the very strong currents used with metallic bodies may interfere, and are considered, for example, to explain the experiments of Forbes and Gore.[1] But in my case, with a substance which has a very small coefficient of dilatation, I think the vibratory motion may be considered as an electrodynamic effect. As a supporting demonstration, I suspended with insulated metallic wires near another, three horizontal pieces of carbon (3 cm. long) in such a way that they could move freely away, and the two outer ones were connected with a battery of twenty Grove cells; *immediately* on closing the circuit a repulsion ensued between them and an oscillating motion set in, whilst bright sparks appeared between the contacts.

This experiment may throw some light on a recent controversy which has arisen between Mr. Varley and Prof. Hughes.[2] The latter insists on a change in contact resulting from alternating varying forms of the molecules or their spheres of action, in accordance with the sonorous vibrations. Mr. Varley points out a quite distinct cause. By using a contact-breaker moved with the hand he discovered, on applying a microscope with a 350 times magnifying power when the circuit was closed or opened, a grey cloud issuing between the nearest carbon-points. This seems to prove that little particles of carbon are loosened by an effect of trituration on the contact-surface, the cohesion being lessened by the heating effect of the current. This cloud of microscopic dust serves as a vehicle to the current (when the carbon piece is vibrating), and its resistance of course is easily modified by the impinging sound-vibrations. Now my experiment, though with a strong current, supports this fact, and shows that the vertical carbon is actually vibrating under its influence, and may prepare the above-mentioned condition, or at least render it very ready to change its contact in accordance with sound-waves acting on it with more or less force.

Perhaps it will be observed that a microphone acts very well in transmitting sound when even the weak current from a couple of Leclanché cells is used, but then, also, we may admit a propensity (through the influence of this current) of the vertical carbon rod to get into a vibratory condition, which the sonorous vibrations will easily actuate according to their own period, if really it is not already vibrating at microscopical distances.

Prof. Hughes alludes to an experiment which, as he thinks, gives an evident support to his theory. In a sealed glass tube are inclosed five loose pieces of carbon with terminals to admit a current. He remarks that, properly pressed mere mechanical shaking produces no variation of the current except that due to a constantly increasing resistance caused by abrasion of the carbon contacts, whilst under the influence of sonorous vibrations a varying current is produced, because the tube in this case is varying its length, and the molecules undergo proportional change of form. I think this proof may be as well, and with more probability explained by the facts mentioned above. The tube contains four or five *loose* pieces of carbon, and besides these some air, which, as it is in a closed space, will press from all sides on the carbon parcels when it is put in vibration by sound, and therefore alter in a mechanical way their distances, the more because the surfaces in contact are rough ones. In conclusion I will observe that the audible vibration of the vertical carbon rod in the microphone certainly elucidates the facts discovered by Blyth concerning sound transmitted only with coal cinders forming a receiving and a transmitting apparatus in a galvanic circuit, and on which he insists in a recent communication to this journal (NATURE, vol. xix. p. 72). L. Bleekrode

The Hague, December 8, 1878

[1] Forbes, *Phil. Mag.*, t. xvii. p. 358. Gore, *ibid.*, t. xv. p. 519.

[2] *Telegraphic Journal*, October 1 and 15, 1878.

Shakespeare's Colour-Names

In the name of scientific accuracy and fair criticism I protest against Mr. Murphy's letter in NATURE, vol. xix. p. 197. His remarks proceed on the perfectly gratuitous assumption that *all* eagles have blue eyes. As this is not a fact (the only live ones I have examined had both of them green eyes), I have no hesitation in asserting that when Shakespeare wrote "An eagle, madam, hath not so green, so quick, so fine an eye as Paris hath," he did so, after having seen an eagle or eagles, and that when he said green he "evidently" meant green, and not blue.

Edinburgh, January 4 A. Craig-Christie

Your correspondent, Mr. J. J. Murphy, in his letter, NATURE, vol. xix. p. 197, overlooks the fact that *blue* is quite as inappropriate as *green* to describe the eye of an eagle. Shakespeare would never have used either epithet; the word he made use of was doubtless *keen*. Green has been substituted by the mistake of some transcriber of the play working by ear, and not by eye. I only wonder the correction has not been made long ago by some commentator. Robert Brewin

Exeter, January 4

The Meteor Shower of January 2

After a very heavy fall of rain, sleet, and then snow (equalling in the aggregate 1·472 inch), on the evening and night of January 1, the clouds partly cleared away on the ensuing morning, and during a watch of twenty minutes (6·14 to 6·34 A.M., January 2) in a sky fully two-thirds overcast, fourteen meteors were seen, all of them belonging to the special shower in Quadrans. This radiant was evidently very active at the time I saw it, and in a cloudless sky, must have supplied meteors at the rate of more than one per minute (for one observer). The paths were short and quick without streaks or trains. Radiant point at $230° + 51°$, but not very exactly found owing to the clouds and haze through which several of the meteors were indistinctly seen. Three or four were as bright as 1st mag. stars. W. F. Denning

Ashleydown, Bristol, January 2

OUR ASTRONOMICAL COLUMN

Missing Nebulæ.—In Mr. Ellery's Report, to which reference was made last week, it is stated that "two nebulæ, H 4223 and H 1561, widely separated from each other, and described by Herschel as prominent objects, cannot now be found, although careful search has been made for them." The first of these nebulæ is near the cluster Dunlop 413: in the "General Catalogue" it is called "a remarkable object," but being very large and faint, it might, perhaps, be suspected that its invisibility in the Melbourne reflector is owing to the same cause that has led to the Pleiades-nebula, and other similar diffused objects (as G. C. 132, 4570, 5051) being overlooked in very large telescopes though obvious in much smaller ones. But in the case of H 1561 no such supposition is admissible. It was observed by Sir John Herschel on five occasions, in sweeps made between December, 1834, and February, 1836; when best seen it was termed pretty bright, from 25″ to 35″ in diameter, gradually brighter towards the centre, and situate to the south of, though very near to, three stars of the eleventh magnitude. Taking a mean of the five observations of position, and reducing to 1880, its R.A. is 7h. 35m. 8·0s. and N.P.D., 159° 0′ 46″.

We do not hear of recent observation of the nebula situated near ζ Tauri, which was discovered by Chacornac, and which was sufficiently bright from 1855, October 19, to 1856, January 27, to "cause surprise that it had not been inserted by Mr. Hind upon his ecliptic charts," though it was not perceived on the meridian even with a refractor of 25 centimetres from 1853, December 3, to 1854, December 17. In 1855–56 the nebula was projected upon a star of the eleventh magnitude, the position of which for 1880 is in R.A. 5h. 30m. 16·0s. and N.P.D. 68° 51′ 29″. Chacornac says: "Elle offrait une forme presque rectangulaire, dont le plus grand côte soutendait un angle de trois minutes et demie, et le plus petit un arc de deux minutes et demie." D'Arrest, 1863, September 82, could not perceive any nebulosity about the star, nor, 1165, January 25, "cœlo valde eximio." He notes that the star is double, estimated distance 40″. It precedes ζ Tauri 12·5s., and is north of it 4′ 28″.

Some years since the approximate places of three nebulosities remarked with the comet-seeker at Cambridge, U.S., by Mr. G. P. Bond, but subsequently missed, were published. One seen 1850, February 27, in R.A. 0h. 47m. 41s. ± 1m., N.P.D. 26° 36′ ± 19′, could not be found 1863, September 9. A faint and rather large nebula, seen 1850, December 30, was not to be found, 1863, August 17; R.A. 14h. 37m. ± 3m., N.P.D. 67° 0′ ± 30′, and a third nebulosity resembling a comet, observed 1850, November 7, in R.A. 23h. 50m. 46s., N.P.D. 123° 24′, requires verification; the place is for 1850.

GEOGRAPHICAL NOTES

UNDER the direction of the United States Hydrographic Office, Lieut.-Commander F. M. Green, U.S.N., and the officers under his command, have during the last four years been engaged in determining exactly secondary meridians of longitude by means of the submarine telegraph cables in the West Indies and South America. The result of the West India work in 1874, 1875, and 1876 was the determining the latitude and longitude of a large number of points in the West Indies with the utmost possible exactness; and during the past year this work, of the greatest value to geographical and geodetical science, has been continued by making a chain of telegraphic measurements from the Royal Observatory at Lisbon, by way of Madeira, St. Vincent, Pernambuco, Bahia, Rio de Janeiro, and Monte-Video to Buenos Ayres, there connecting with the observatories of Cordova and Santiago. This chain is perfect, with the exception of one link on the coast of Brazil, where the cable was broken, necessitating the procuring of new cable from England; but its completion will be effected before the computation of the observations already made can be finished. The method used for determining the latitude was in all cases that of the zenith telescope, brought to great perfection by the United States Engineers and the Coast Survey; that for differences of longitude, the comparison by repeated telegraphic signals of two chronometers at the ends of the telegraph cable, determining their errors both before and after the comparison by numerous transits of stars over the meridian. All that is needed to make the work of the last year perfect and complete is the telegraphic determination of the differences of longitude between the Greenwich and Lisbon observatories, and the completion of the imperfect link on the Brazilian coast, both of which will be done during the coming year. Until the observations have been carefully discussed, the results as compared with former determinations cannot be known exactly, but a preliminary computation indicates that the longitude of the coast of Brazil is laid down about three or three and a half miles too far west, this westerly error being indicated in a less degree in the longitudes of Madeira and St. Vincent.

LORD AUGUSTUS LOFTUS has recently forwarded to the Foreign Office, from St. Petersburg, a translation of a Russian letter from Cabul, descriptive of the journey of General Stoletoff's mission from Samarcand, which supplies some notes of interest respecting the country traversed. The road selected for reaching the Oxus was through Huzar, Shirabad, and Chushkogosar, which was traversed in five days. On this route the mission passed through the famous defile known in ancient times under the name of the "Iron Gates," and now called Burghasse Khana. The mission crossed the Oxus in very primitive boats, and marching by night, passed over a sandy arid steppe, and next morning reached Kurshiak settlement, situated in a cultivated country. They made three stages before reaching Mizar and Sheriff, where great crowds thronged the streets, and gazed with curiosity on the people from the distant north. After leaving Tashurgan, the party reached the spurs of the Hindu Kush, and journeyed to Cabul during twenty days. Ascending at first in gentle slopes, the Hindu Kush gradually rises higher and higher, forming, amidst its frequent passes, terraces of increasing height. After traversing a series of such terraces, the mission reached the elevated Bamian Valley (8,500 feet), near which are the Kalu and Great Tran Passes (13,000 feet). Passing the famous Bamian idols, chiselled on the face of the rock, they emerged from the last-named pass, and then descended from the Ugly Pass into the Cabul Darya Valley, at a place three days' journey from the capital of Afghanistan.

THE French papers published last week news from the Gaboon settlements stating that the Ogowe exploring mission had arrived in Libreville, the head city of the colony. A telegram read at the last sitting of the Paris Geographical Society announced that M. Brazza, the chief of the mission, had arrived in Lisbon with some of his subordinates, on his way to Paris. It was expected he would arrive in time for the meeting of the Society on Tuesday. The exploration of the mission has lasted three years, and many highly important results are said to have been obtained.

AT the last sitting of the Paris Geographical Society M. de Lesseps read a telegram received from Capt. Roudaire, stating that he had found nothing but compressed sand when boring to a depth of 30 metres in the Gabes Isthmus, so that no real difficulty prevented the opening of it for the intended Saharan Sea.

ON the authority of Mr. Oscar Dickson, it is stated that the Nordenskjöld expedition is wintering forty miles north of Cape East in Behring Straits. This news has been given to American whalers by a party of trustworthy natives, and a number of whalers are said to be wintering with the *Vega*.

THE just issued October *Bulletin* of the Paris Geographical Society contains a long paper by M. Léon Rousset, giving the results of a journey in the upper basin of the Yellow River and the region of the loess which overspreads so large an area of China, and of which Richthofen makes so much in his great work on China. M. Dutreuil de Rhins contributes a very useful account of Annam and the province of Hué; M. H. Harrisse discusses the question of the burial-place of Columbus, and M. d'Abbadie concludes his useful description of the instruments to be used in travel.

A LITTLE work on Afghanistan has just been published by Dr. Josef Chavanne, the author of the excellent work on the Sahara. It is written with special reference to the present war, and gives a detailed description of the country and its geographical character, as well as of its inhabitants, from an ethnographical as well as a social point of view. The natural resources and military power of Afghanistan are described, and particular attention has been paid to an account of the communication between India and Afghanistan, the lines of operation, and the numerous mountain passes. There are several illustrations and an excellent map. It is published by Hartleben, of Vienna.

IN a recent voyage from Melbourne to the Fiji Islands, the steamer *Ariel* called at Lord Howe Island, where twenty-five people in all were found. The island is mountainous, of volcanic origin, but well-wooded, about five miles long, and from one and a half to two miles broad, and is situated some 400 miles east of Sydney. The communication of the inhabitants with the outer world is nowadays very uncertain, as whalers but rarely visit them.

THE MUSICAL ASSOCIATION[1]

THE question, In what way does science enter into the subject of music? is one that by no means admits of an easy answer. If we were to put it to various persons interested in music in different ways we should find their opinions most vague and contradictory. A university scholar, or a physical lecturer, would make the science of music consist entirely in the doctrines of acoustics; while, on the other hand, we should find some of the most eminent musical professors telling us that these had nothing to do with music at all, but that science meant the study and application of the rules of musical composition. Or possibly it might even be held that a skilful manipulation of the violin, or an appropriate management of the voice in singing, or an intelligent phrasing of pianoforte passages, or other refinements of execution, constituted all the science that musicians need aspire to.

A quarter of a century ago such a question would have excited no interest. People in general were satisfied to take the art as they practically found it, and troubled themselves but little as to the principles on which it was based. But the march of knowledge has changed the aspect of the matter. Modern philosophical investigation has included music in the universality of its aims, and the musician, however conservative, must submit to a searching inquiry as to the real nature of the stuff in which he deals.

The great work of Helmholtz, published in 1863, gave the first real stimulus to scientific musical inquiry; and although many years passed before it became much known in this country it at length aroused attention, and some of the most intelligent students of the art began to see that there was really something to be inquired into—the first step towards accurate knowledge of any kind. They observed the beneficial operation of the learned societies, where papers on the subjects they embraced were brought forward; and the idea occurred to them that an association of a similar character for music would not only enable the scientific questions connected with it to be publicly discussed, but might also be made conducive to the welfare of the art in a practical point of view. The idea was mentioned to one of the most eminent men of science (now president of the Royal Society), who, warmly approving it, issued the following circular:—

"50, *Grosvenor Place, April* 8, 1874

"DEAR SIR,—It has been suggested by several leading persons interested both in the theory and practice of music, that the formation of a society similar in the main features of its organisation to existing learned societies would be a great public benefit. Such a musical society might comprise among its members the foremost musicians, theoretical as well as practical, of the day, the principal patrons of art, and also those scientific men whose researches have been directed to the science of acoustics and to kindred inquiries. Its periodical meetings might be devoted partly to the reading of papers upon the history, the principles, and the criticism of music, partly to the illustration of such papers by actual performance, and partly to the exhibition and discussion of experiments relating to theory and construction of musical instruments, or to the principles and combinations of musical sounds.

"With a view to ascertain the opinions of persons interested in these subjects, and to attempt a more precise definition of the objects and constitution of such a society, it is proposed to hold a meeting here, at which your presence is requested on Thursday, April 16, at 2.30 P.M.

"I am, dear sir, yours faithfully,
"(Signed) W. SPOTTISWOODE"

This led to the formation of the Association whose proceedings are mentioned at the head of this article. The rules were judiciously framed, so as to avoid the rocks on which former musical societies had been shipwrecked; and the society has now gone successfully through four sessions. We learn from the report just issued, at the commencement of the fifth year, that the finances are prosperous, that the meetings are well attended, that the officers are zealous and efficient, and that a series of good papers are forthcoming for the future; from all which it may be fairly inferred that the institution has taken a permanent position.

The character of the society is, of course, best displayed by the contents of its *Transactions*. We cannot pretend to review the thirty-six papers (some of them very elaborate) contained in the four volumes before us; it will be an easier course to indicate briefly, in the first instance, what are the "subjects connected with the art and science of music" which more especially deserve "investigation and discussion," and then to see how far the papers actually presented to the Association have fulfilled the object aimed at in its title.

Giving precedence to science, one may conceive that the "Principles and Phenomena of Acoustics" would claim attention. It is true, as has already been hinted, that some eminent practical musicians repudiate the relevancy of these inquiries, and discourage their study, on the ground that a knowledge of acoustics is unnecessary to the practical musician, whether composer or performer.[2]

But fortunately the general spread of education sufficiently disposes of arguments of this kind. There are, and no doubt always will be, persons who are satisfied with the minimum amount of knowledge to enable them to earn their daily bread, but it is to be hoped the number is decreasing every day. A man who lives by an art will, if his mind be properly constituted, be in no wise reluctant to learn all he can about it, even though the knowledge may not be immediately convertible into money. Musicians must, in spite of the disparaging opinion of some of their leaders, be treated as intelligent beings, who have minds capable of enlightenment and instruction, and surely there is nothing unreasonable in assuming that the philosophical principles on which their art depends must present some interest to them, if laid before them in an intelligible form. The doctrine that such knowledge should be confined to cultivated amateurs, and forbidden to professional musicians, is simply a libel on the intelligence of those to whom we owe enjoyment of so high an order. If, then, these principles are to be studied, the science of acoustics must necessarily form the basis of the study. The splendid

[1] Proceedings of the Musical Association for the Investigation and Discussion of Subjects connected with the Art and Science of Music. Vols. i. to iv. First Session, 1874-5; Second Session, 1875-6; Third Session, 1876-7; Fourth Session, 1877-8.

[2] It is a remarkable example of this view that in a new elaborate and voluminous English "Dictionary of Music," now in course of publication, the word *Acoustics* finds no place.

investigations of Helmholtz as to the nature of musical sounds and musical sensations form a fund of knowledge of the most interesting and instructive kind, and illustration and discussion of such topics would be by no means out of place before the society. We believe that the great fundamental fact of the compound nature of musical sounds, which now has become as firmly established as any physical fact can be, is hardly yet understood, or its great significance appreciated by the great mass of the persons who have to do with its effects every day of their lives.

It happens, however (no doubt for good and sufficient reasons), that the more abstract principles of acoustics have received but little attention in the society. We only notice three papers which come within this category, and these on quite subsidiary points, namely, "On our Perception of the Direction of a Source of Sound," by Lord Rayleigh; "On the Sensitiveness of the Ear to Pitch and Change of Pitch," by Mr. A. J. Ellis; and "On the Musical Inventions and Discoveries of the late Sir C. Wheatstone," by Prof. W. G. Adams.

But the science of acoustics is a very different thing from the theory of music. There is much misunderstanding on this point; many people confuse the two, whereas the former is in reality only the introduction to the latter. A student may be well acquainted with all the scientific facts and theories relating to the production and transmission of musical sounds, and yet know nothing of the mode in which these data bear on music itself. Helmholtz, who, with wonderful knowledge and sagacity, appears to have anticipated almost every possible view of the subject, has fully expressed this distinction not only in the substance of his great work, but in its very title-page. He calls it "Die Lehre von den Tonempfindungen *als physiologische Grundlage* für die Theorie der Musik," thereby declaring that the acoustical doctrines he so admirably lays down are not to be considered as forming of themselves a theory of music, but are merely intended to *serve as a basis* for such a theory. Starting from these data, it becomes necessary to consider the influence they have on the varied and complicated forms and rules which guide the structure of musical composition, as, for example, the construction of the ordinary scale, the nature of chromatic notes, tonality, the combinations and progressions of harmony, the rules of melodial counterpoint, musical form, and so on. A crowd of most interesting questions arise as to how far all these practical matters have been influenced by the physical properties of musical sounds, or how far they are the result of free artistic invention. Helmholtz devotes the second part of his work to the discussion of these and kindred questions, on which, aided by a competent knowledge of music, his great reasoning powers have enabled him to throw much new light. But this part of his labours has been hitherto almost a sealed book to musicians; it is difficult, often elaborate, and sometimes obscure, and the interpreters who have so ably popularised his acoustical researches have stopped short before venturing on what was to physicists a less familiar region. Yet this is by far the most important section of the work, from a musical point of view; it is, in fact, the real "Theory of Music," the true musical philosophy, in which the proper application of science to music is to be found; moreover, unlike abstract acoustics, it touches closely on the practice of the musical art, and the habits of thought of its professors. There are few teachers of musical composition who do not to some extent attempt to found their instruction on natural principles, or what they think to be such; but the theories thus propounded are for the most part crude, vague, and founded on merely empirical fancies, having no philosophical origin, and such as will not stand the test of scientific investigation or strict logical reasoning; and hence we can hardly wonder at the fact that they rather obstruct than aid the efficiency of musical instruction.

This subject, therefore, the "Application of Scientific Data and Scientific Reasoning to the Theory of Music," is one which offers every inducement for the higher order of musical study, and its discussion is eminently in place in such a society as that before us. The results of the modern investigations are so new, and in many respects so antagonistic to the ideas hitherto prevailing among musicians, that it is not to be expected they will be at once fully understood or favourably received. Already a considerable amount of opposition has been manifested to them; it is reasonable and proper that they should be fairly considered, and it is in the highest degree desirable that they should be clearly explained. The subject has not been neglected at the meetings of the Association, for, although no systematic treatment of it has yet been attempted, we find no less than eight papers on various points of theoretical detail. Four of these are on intonation and temperament (a favourite theme with musical mathematicians, but somewhat unpalatable to practical men, who consider the out-of-tune equal division of the octave "good enough for them"); a fifth aims at exposing the fallacies and inconsistencies of certain of the old theoretical systems; another treats of the philosophical nature of intervals and of the construction of the scale; another expounds some elementary views on harmony; and the eighth exhibits various numerical calculations on musical ratios, &c.

Another point that furnishes a most profitable topic of study is *musical history*. It is impossible to look far into music without becoming aware how largely modern form and structure are derived from what has gone before, and the careful examination of this clears up many points of theory for which no other sufficient explanation can be found. Nothing could be more in place for a "musical association" than historical papers, not as mere matters of antiquarian curiosity, but as bearing on the various changes of musical form. We only, however, find two historical papers, one, an instructive essay, by Sir Frederick Ouseley, on the "History of Ecclesiastical Music in Western Europe," the other an interesting monograph, by Mr. Cummings, on "Purcell."

The *construction of musical instruments* offers a large and varied source of interest, combining the laws of acoustics, the application of mechanical skill and invention, and the adaptation to practical musical use. There are six papers on this, relating to stringed and brass instruments, drums, and the voice.

Finally, there are abundance of topics connected with *the practice of the musical art* which admit of discussion in such a society; for although, in a scientific journal, it is our chief province to point to the subjects in which science takes part, yet it would be a misuse of the society to let these predominate to the prejudice of the more practical matters which come home more directly to professional men, and we consider it a good evidence of the flourishing condition and prospects of the society that these practical points have received so large a share of attention. By far the larger number of the papers have been of this practical kind, relating to musical notations and nomenclature, criticism, practical standards of pitch, the analysis of great musical works, pianoforte playing, the cultivation of sacred music, the connection of music with language, the laws of expression, modes of tuition, and musical libraries. A paper on the last-named subject led to a memorial to the British Museum, and elicited an answer explaining the facilities which that institution affords for musical reference and study.

The Association deserves the support and co-operation of every one interested in the cultivation of music either theoretically or practically, and we cordially wish it the permanent success it seems in a fair way to attain.

W. POLE

DISCUSSION OF THE WORKING HYPOTHESIS THAT THE SO-CALLED ELEMENTS ARE COMPOUND BODIES[1]

II.

Application of the above Views to Calcium, Iron, Lithium, and Hydrogen

Calcium

IT was in a communication to the Royal Society made now some time ago (*Proc.*, vol. xxii. p. 380, 1874), that I first referred to the possibility that the well-known line-spectra of the elementary bodies might not result from the vibration of similar molecules. I was led to make the remark in consequence of the differences to which I have already drawn attention in the spectra of certain elements as observed in the spectrum of the sun and in those obtained with the ordinary instrumental appliances.

Later (*Proc.* Roy. Soc., No. 168, 1876) I produced evidence that the molecular grouping of calcium which, with a small induction-coil and small jar, gives a spectrum with its chief line in the blue, is nearly broken up in the sun, and quite broken up in the discharge from a large coil and jar, into another or others with lines in the violet.

I said "another," or "others," because I was not then able to determine whether the last-named lines proceeded from the same or different molecules; and I added that it was possible we might have to wait for photographs of the spectra of the brighter stars before this point could be determined.

I also remarked that this result enabled us to fix with very considerable accuracy the electric dissociating conditions which are equivalent to that degree of dissociation at present at work in the sun.

In Fig. 3 I have collected several spectra copied from photographs in order that the line of argument may be grasped.

First we see what happens to the non-dissociated and the dissociated chloride. Next we have the lines with a weak voltaic arc, the single line to the right (W L

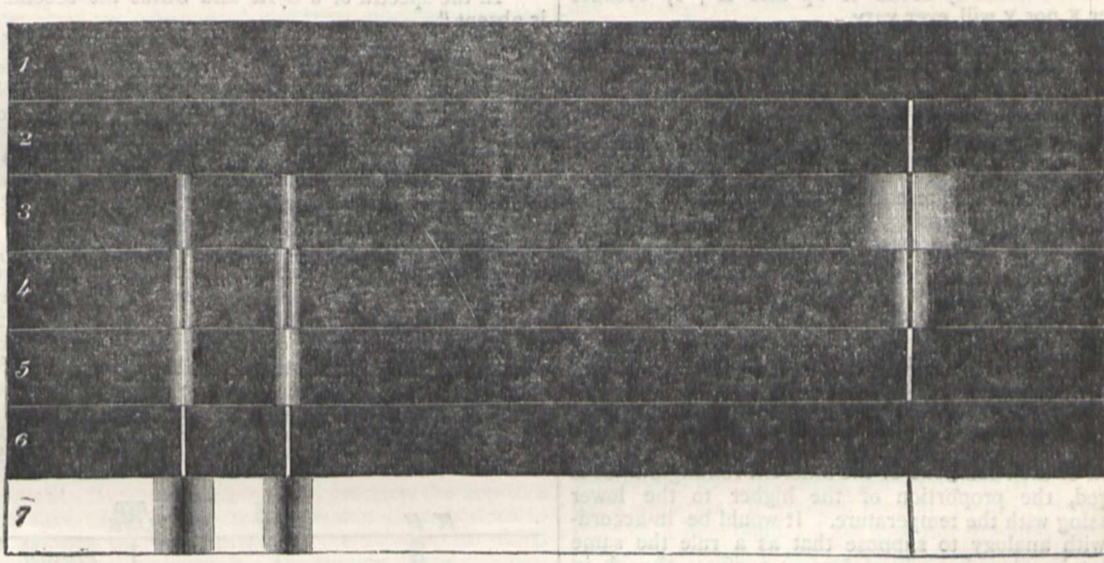

FIG. 3.—The blue end of the spectrum of calcium under different conditions. 1. Calcium is combined with chlorine ($CaCl_2$). When the temperature is low, the compound molecule vibrates as a whole, the spectrum is at the red end, and no lines of calcium are seen. 2. The line of the metal seen when the compound molecule is dissociated to a slight extent with an induced current. 3. The spectrum of metallic calcium in the electric arc with a small number of cells. 4. The same when the number of cells is increased 5. The spectrum when a coil and small jar are employed. 6. The spectrum when a large coil and large jar are used. 7. The absorption of the calcium vapour in the Sun.

4226·3) is much thicker than the two lines (W L 3933 and 3968) to the left, and reverses itself.

We have next calcium exposed to a current of higher tension. It will be seen that here the three lines are almost equally thick, and all reverse themselves.

Now it will be recollected, that in the case of known compounds the band structure of the true compounds is reduced as dissociation works its way, and the spectrum of each constituent element makes its appearance. If in 3 we take the wide line as representing the banded spectrum of the compound, and the thinner ones as representing the longest elemental lines making their appearance as the result of partial dissociation, we have, by hypothesis, an element behaving like a compound.

If the hypothesis be true, we ought to be able not only to obtain, with lower temperatures, a still greater preponderance of the single line, *as we do;* but with higher temperatures, a still greater preponderance of the double ones, *as we do.*

I tested this in the following manner:—Employing photography, because the visibility of the more refrangible lines is small, and because a permanent record of an experiment, free as it must be from all bias, is a very precious thing.

Induced currents of electricity were employed in order that all the photographic results might be comparable.

To represent the lowest temperature I used a small induction coil and a Leyden jar only just large enough to secure the requisite amount of photographic effect. To represent the highest, I used the largest coil and jar at my disposal. The spark was then taken between two aluminium electrodes, the lower one cup-shaped, and charged with a salt of calcium.

In the figure I give exact copies of the results obtained. It will be seen that with the lowest temperature only the single line (2) and with the highest temperature only the two more refrangible lines (6) are recorded on the plate.

This proved that the intensity of the vibrations was quite changed in the two experiments.

Perhaps it may not be superfluous here to state the reasons which induced me to search for further evidence in the stars.

[1] Paper read at the Royal Society, December 12, 1878, by J. Norman Lockyer, F.R.S. Continued from p. 201.

It is abundantly clear that if the so-called elements, or more properly speaking their finest atoms—those that give us line spectra—are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and therefore no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other atoms, like or unlike, at all lower temperatures. Hence association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature there may be no end.

That is the first point.

The second is this :—

We are justified in supposing that our "calcium," once formed, is a distinct entity, whether it be an element or not, and therefore, by working at it alone, we should never know whether the temperature produces a single simpler form or more atomic condition of the same thing, or whether we actually break it up into X + Y, because neither X nor Y will ever vary.

But if calcium be a product of a condition of relatively lower temperature, then in the stars, hot enough to enable its constituents to exist uncompounded, we may expect these constituents to vary in quantity; there may be more of X in one star and more of Y in another; and if this be so, then the H and K lines will vary in thickness, and the extremest limit of variation will be that we shall only have H representing, say X in one star, and only have K representing, say Y in another. Intermediately between these extreme conditions we may have cases in which, though both H and K are visible, H is thicker in some and K is thicker in others.

Prof. Stokes was good enough to add largely to the value of my paper as it appeared in the *Proceedings* by appending a note pointing out that "When a solid body such as a platinum wire, traversed by a voltaic current, is heated to incandescence, we know that as the temperature increases not only does the radiation of each particular refrangibility absolutely increase, but the proportion of the radiations of the different refrangibilities is changed, the proportion of the higher to the lower increasing with the temperature. It would be in accordance with analogy to suppose that as a rule the same would take place in an incandescent surface, though in this case the spectrum would be discontinuous instead of continuous. Thus if A, B, C, D, E denote conspicuous bright lines of increasing refrangibility in the spectrum of the vapour, it might very well be that at a comparatively low temperature A should be the brightest and the most persistent; at a higher temperature, while all were brighter than before, the relative brightness might be changed, and C might be the brightest and the most persistent, and at a still higher temperature E."

On these grounds Prof. Stokes, while he regarded the facts I mentioned as evidence of the high temperature of the sun, did not look upon them as *conclusive* evidence of the dissociation of the molecule of calcium.

Since that paper was sent in, however, the appeal to the stars to which I referred in it has been made, and made with the most admirable results, by Dr. Huggins.

The result of that appeal is that the line which, according to Prof. Stokes' view, should have prevailed over all others, as Sirius is acknowledged to be a hotter star than our sun, is that, if it exists at all in the spectrum, it is so faint that it was not recognised by Dr. Huggins in the first instance.

In Sirius, indeed, the H line due to one molecular grouping of calcium is as thick as are the hydrogen lines as mapped by Secchi, while the K line, due to another molecular grouping, which is equally thick in the spectrum of the sun, has not yet made its appearance.

In the sun, where it is as thick as H, the hydrogen lines have vastly thinned.

While this paper has been in preparation, Dr. Huggins has been good enough to communicate to me the results of his most important observations, and I have also had an opportunity of inspecting several of the photographs which he has recently taken. The result of the recent work has been to show that H and *h* are of about the same breadth in Sirius. In *α* Aquilæ while the relation of H to *h* is not greatly changed, a distinct approach to the solar condition is observed, K being now unmistakably present, although its breadth is small as compared with that of H. I must express my obligations to Dr. Huggins for granting me permission to enrich my paper by reference to these unpublished observations. His letter, which I have permission to quote, is as follows :—

"It may be gratifying to you to learn that in a photograph I have recently taken of the spectrum of *α* Aquilæ there is a line corresponding to the more refrangible of the solar H lines [that is K], but about half the breadth of the line corresponding to the first H lines.

"In the spectra of *α* Lyræ and Sirius the second line is absent."

Prof. Young's observations of the chromospheric lines, to which I shall afterwards refer, give important evidence regarding the presence of calcium in the chromosphere of the sun. He finds that the H and K lines of calcium are strongly reversed in every important spot, and that in solar storms H has been observed injected into the chromosphere seventy-five times, and K fifty times, while the blue line at W. L. 4226·3, the all-important line at the arc-temperature, was only injected thrice.

Further, in the eclipse observed in Siam in 1875, the H and K lines left the strongest record in the spectrum of the chromosphere, while the line near G in a photographic region of much greater intensity was not recorded at all. In the American eclipse of the present year the H and K lines of calcium were distinctly visible at the base of the corona, in which for the first time the observers could scarcely trace the existence of any hydrogen.

To sum up, then, the facts regarding calcium, we have first of all the H-line differentiated from the others

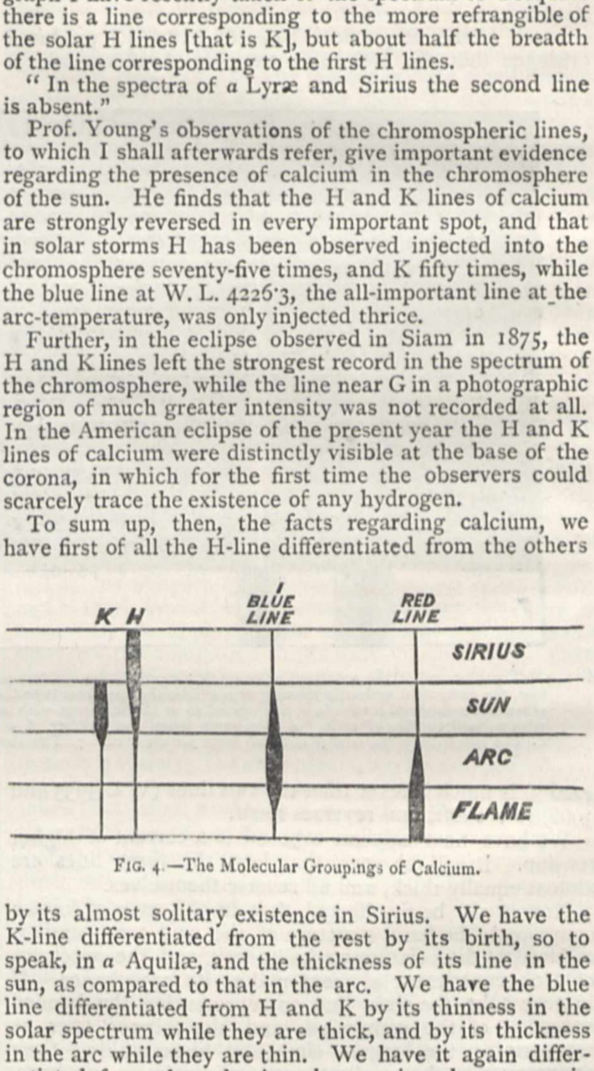

FIG. 4.—The Molecular Groupings of Calcium.

by its almost solitary existence in Sirius. We have the K-line differentiated from the rest by its birth, so to speak, in *α* Aquilæ, and the thickness of its line in the sun, as compared to that in the arc. We have the blue line differentiated from H and K by its thinness in the solar spectrum while they are thick, and by its thickness in the arc while they are thin. We have it again differentiated from them by its absence in solar storms in which they are almost universally seen, and finally, by its absence during eclipses, while the H and K lines have been the brightest seen or photographed. Last stage of all, we have calcium, distinguished from its salts by the fact that the blue line is only visible when a high temperature is employed, each salt having a definite spectrum of its own, in which none of the lines to which I have drawn attention appear, so long as the temperature is kept below a certain point.

Iron

With regard to the iron spectrum I shall limit my remarks to that portion of it visible on my photographic plates between H and G. It may be described as a very complicated spectrum so far as the number of lines is concerned in comparison with such bodies as sodium and potassium, lead, thallium, and the like, but unlike them again it contains no one line which is clearly and unmistakably reversed on all occasions. Compared, however, with the spectrum of such bodies as cerium and uranium the spectrum is simplicity itself.

Now among these lines are two triplets, two sets of three lines each, giving us beautiful examples of those repetitions of structure in the spectrum which we meet with in the spectra of almost all bodies, some of which have already been pointed out by Mascart, Cornu, and myself. Now the facts indicate that these two triplets are not due to the vibration of the same molecular grouping which gives rise to most of the other lines. They are as follows. In many photographs in which iron has been compared with other bodies, and in others again in which iron has been photographed as existing in different degrees of impurity in other bodies, these triplets have been seen almost alone, and the relative intensity of them, as compared with the few remaining lines, is greatly changed. In this these photographs resemble one I took three years ago, in which a large coil and jar were employed instead of the arc, which necessitated an exposure of an hour instead of two minutes. In this the triplet near G is very marked, the two adjacent lines more refrangible near it, which are seen nearly as strong as the triplet itself in some of the arc photographs I possess, are only very faintly visible, while dimmer still are seen the lines of the triplet between H and *h*.

There is another series of facts in another line of work. In solar storms, as is well known, the iron lines sometimes make their appearance in the chromosphere. Now, if we were dealing here with one molecular grouping, we should expect the lines to make their appearance in the order of their lengths, and we should expect the shortest lines to occur less frequently than the longest ones. Now, precisely the opposite is the fact. One of the most valuable contributions to solar physics that we possess is the memoir in which Prof. C. A. Young records his observation of the chromospheric lines, made on behalf of the United States Government, at Sherman, in the Rocky Mountains. The glorious climate and pure air of this region, to which I can personally testify, enabled him to record phenomena which it is hopeless to expect to see under less favourable conditions. Among these were injections of iron vapour into the chromosphere, the record taking the form of the number of times any one line was seen during the whole period of observation.

Now two very faint and short lines close to the triplet near G were observed to be injected thirty times, while one of the lines of the triplet was only injected twice.

The question next arises, Are the triplets produced by one molecular grouping or by two? This question I also think the facts help us to answer. I will first state by way of reminder that in the spark photograph the more refrangible triplet is barely visible, while the one near G is very strong. Now if one molecular grouping alone were in question this relative intensity would always be preserved however much the absolute intensity of the compound system might vary, but if it is a question of two molecules we might expect that in some of the regions open to our observation we should get evidence of cases in which the relative intensity is reversed or the two intensities are assimilated. What might happen does happen; the relative intensity of the two triplets in the spark photograph is grandly reversed in the spectrum of the sun. The lines barely visible in the spark photograph are among the most prominent in the solar spectrum, while the triplet which is strong in that photograph is represented by Fraunhofer lines not half so thick. Indeed, while the hypothesis that the iron lines in the region I have indicated are produced by the vibration of one molecule does not include all the facts, the hypothesis that the vibrations are produced by at least three distinct molecules includes all the phenomena in a most satisfactory manner.

Lithium

Before the maps of the long and short lines of some of the chemical elements compared with the solar spectra, which were published in the *Phil. Trans.* for 1873, "Plate IX.," were communicated to the Society, I very carefully tested the work of prior observers on the non-coincidence of the red and orange lines of that metal with the Fraunhofer lines, and found that neither of them were strongly if at all represented in the sun, and this remark also applies to a line in the blue at wave-length 4,603.

The photographic lithium line, however, in the violet, has a strong representative among the Fraunhofer lines.

Applying, therefore, the previous method of stating the facts, the presence of this line in the sun differentiates it from all the others. For the differentiation of the red and yellow lines I need only refer to Bunsen's spectral analytical researches, which were translated in the *Phil. Mag.*, December, 1875.

In Plate IV. two spectra of the chloride of lithium are given, one of them showing the red line strong and the yellow one feeble, the other showing merely a trace of the red line, while the intensity of the yellow one is much increased, and a line in the blue is indicated. Another notice of the blue line of lithium occurs in a discourse by Prof. Tyndall, reprinted in the *Chemical News*, and a letter of Dr. Frankland's to Prof. Tyndall, dated November 7, 1861. This letter is so important for my argument, that I reprint it entire from the *Philosophical Magazine*, vol. xxii. p. 472:—

"On throwing the spectrum of lithium on the screen yesterday, I was surprised to see a magnificent blue band. At first I thought the lithic chloride must be adulterated with strontium, but on testing it with Steinheil's apparatus it yielded normal results without any trace of a blue band. I am just now reading the report of your discourse in the *Chemical News*, and I find that you have noticed the same thing. Whence does this blue line arise? Does it really belong to the lithium, or are the carbon points or ignited air guilty of its production? I find there blue bands with common salt, but they have neither the definiteness nor the brilliancy of the lithium band. When lithium wire burns in air it emits a somewhat crimson light; plunge it into oxygen, and the light changes to bluish white. This seems to indicate that a high temperature is necessary to bring out the blue ray."

"POSTSCRIPT, Nov. 22, 1861.—I have just made some further experiments on the lithium spectrum, and they conclusively prove that the appearance of the blue line depends entirely on the temperature. The spectrum of lithic chloride, ignited in a Bunsen's burner flame, does not disclose the faintest trace of the blue line; replace the Bunsen's burner by a jet of hydrogen (the temperature of which is higher than that of the Bunsen's burner) and the blue line appears, faint, it is true, but sharp and quite unmistakable. If oxygen now be slowly turned into the jet, the brilliancy of the blue line increases until the temperature of the flame rises high enough to fuse the platinum, and thus put an end to the experiment."

These observations of Profs. Tyndall and Frankland differentiate this blue line from those which are observed at low temperatures. The line in the violet to which I have already referred, is again differentiated from all the rest by the fact that it is the only line in the spectrum of the sun which is strongly reversed, so far as our present knowledge extends. The various forms of lithium, therefore, may be shown in the following manner.

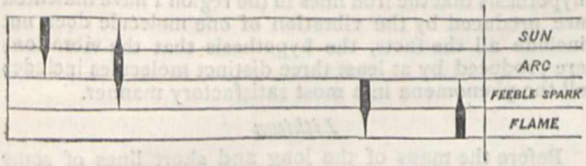

FIG. 5.—The Molecular Groupings of Lithium.

It is remarkable that in the case of this body which at relatively low temperature goes through its changes, its compounds are broken up at the temperature of the Bunsen burner. The spectrum, *e.g.* of the chloride, so far as I know, has never been seen.

Hydrogen

All the phenomena of variability and inversion in the order of intensity presented to us in the case of calcium can be paralleled by reference to the knowledge already acquired regarding the spectrum of hydrogen.

Dr. Frankland and myself were working together on the subject in 1869. In that year (*Proc.*, No. 112) we pointed out that the behaviour of the *h* line was *hors ligne*, and that the whole spectrum could be reduced to one line, F.

"1. The Fraunhofer line on the solar spectrum, named *h* by Ångström, which is due to the absorption of hydrogen, is not visible in the tubes we employ with low battery and Leyden-jar power; it may be looked upon, therefore, as an indication of relatively high temperature. As the line in question has been reversed by one of us in the spectrum of the chromosphere, it follows that the chromosphere, when cool enough to absorb, is still of a relatively high temperature.

"2. Under certain conditions of temperature and pressure, the very complicated spectrum of hydrogen is reduced in our instrument *to one line in the green* corresponding to F in the solar spectrum."

As in the case of calcium also, solar observation affords us most precious knowledge. The *h* line was missing from the protuberances in 1875, as will be shown from the accompanying extract from the Report of the Eclipse Expedition of that year:—

"During the first part of the eclipse two strong protuberances close together are noticed; on the limb towards the end these are partially covered, while a series of protuberances came out at the other edge. The strongest of these protuberances are repeated three times, an effect of course of the prism, and we shall have to decide if possible the wave-lengths corresponding to the images. We expect *à priori* to find the hydrogen lines represented. We know three photographic hydrogen lines: F, a line near G, and *h*. F is just at the limit of the photographic part of the spectrum, and we find indeed images of protuberances towards the less refrangible part at the limit of photographic effect. For, as we shall show, a continuous spectrum in the lower parts of the corona has been recorded, and the extent of this continuous spectrum gives us an idea of the part of the spectrum in which each protuberance line is placed. We are justified in assuming, therefore, as a preliminary hypothesis, that the least refrangible line in the protuberance shown on the photograph is due to F, and we shall find support of this view in the other lines. In order to determine the position of the next line the dispersive power of the prism was investigated. The prism was placed on a goniometer table in minimum deviation for F, and the angular distance between F and the hydrogen line near G, *i.e.*, Hγ, was found, as a mean of several measurements, to be 3′. The goniometer was graduated to 15″, and owing to the small dispersive power, and therefore relatively great breadth of the slit, the measurement can only be regarded as a first approximation. Turning now again to our photographs, and calculating the angular distance between the first and second ring of protuberances, we find that distance to be 3′ 15″. We conclude, therefore, that this second ring is due to hydrogen. We, therefore, naturally looked for the third photographic hydrogen line, which is generally called *h*, but we found no protuberance on our photographs corresponding to that wave-length. Although this line is always weaker than Hγ, its absence on the photograph is rather surprising, if it be not due to the fact that the line is one which only comes out at a high temperature. This is rendered likely by the researches of Frankland and Lockyer (*Proc. Roy. Soc.*, vol. xvii. p. 453).

"We now turn to the last and strongest series of protuberances shown on our photographs. The distance between this series and the one we have found reason for identifying with Hγ is very little greater than that between Hβ and Hγ. Assuming the distances equal, we conclude that the squares of the inverse wave-lengths of the three series are in arithmetical progression. This is true as a first approximation. We then calculated the wave-length of this unknown line, and found it to be approximately somewhat smaller than 3,957 tenth-metres. No great reliance can be placed, of course, on the number, but it appears that the line must be close to the end of the visible spectrum.

"In order to decide if possible what this line is due to, we endeavoured to find out both by photography and fluorescence whether hydrogen possesses a line in that part of the spectrum. We have not at present come to any definite conclusion. In vacuum tubes prepared by Geissler containing hydrogen, a strong line more refrangible than H is seen, but these same tubes show between Hγ and Hδ, other lines known not to belong to hydrogen, and the origin of the ultra-violet line is therefore difficult to make out. We have taken the spark in hydrogen at atmospheric pressures, as impurities are easier to eliminate, but a continuous spectrum extends over the violet and part of the ultra-violet, and prevents any observation as to lines. We are going on with experiments to settle this point.

"Should it turn out that the line is not due to hydrogen, the question will arise what substance it is due to. It is a remarkable fact that the calculated wave-length comes very close to H. Young has found that these calcium lines are always reversed in the penumbra and immediate neighbourhood of every important sun-spot, and calcium must therefore go up high into the chromosphere. We draw attention to this coincidence, but our photographs do not allow us to draw any certain conclusions.

"At any rate, it seems made out by our photographs that the photographic light of the protuberances is in great part due to an ultra-violet line which does not certainly belong to hydrogen. The protuberances as photographed by this ultra-violet ray seem to go up higher than the hydrogen protuberances, but this may be due to the relative greater length of the line."

In my remarks upon calcium I have already referred to the fact that the line which our observation led us to believe was due to calcium in 1875, was traced to that element in this year's eclipse. The observations also show the curious connection that, at the time when the hydrogen lines were most brilliant in the corona, the calcium lines were not detected; next, when the hydrogen lines, being still brilliant, the *h* line was not present (a condition of things which, in all probability, indicated a reduction of temperature), calcium began to make itself unmistakably visible; and finally, when the hydrogen lines are absent, H and K become striking objects in the spectrum of the corona.

To come back to *h*, then, I have shown that Dr.

Frankland and myself, in 1869, found that it only made its appearance when a high tension was employed. We have seen that it was absent from among the hydrogen lines during the eclipse of 1875.

I have now to strengthen this evidence by the remark that it is always the shortest line of hydrogen in the chromosphere.

I now pass to another line of evidence.

I submit to the Society a photograph of the spectrum of indium, in which, as already recorded by Thalén, the strongest line is one of the lines of hydrogen (*h*), the other line of hydrogen (near G) being absent. I have observed the C line in the spark produced by the passage of an induced current between indium poles in dry air.

As I am aware how almost impossible it is to render air perfectly dry, I made the following differential experiment. A glass tube with two platinum poles about half an inch apart was employed. Through this tube a slow current of air was driven after passing through a U-tube one foot high, containing calcic chloride, and then through sulphuric acid in a Wolff's bottle. The spectrum of the spark passing between the platinum electrodes was then observed, a coil with five Grove cells and a medium-sized jar being employed. Careful notes were made of the brilliancy and thickness of the hydrogen lines as compared with those of air. This done, a piece of metallic indium, which was placed loose in the tube, was shaken so that one part of it rested against the base of one of the poles, and one of its ends at a distance of a little less than half an inch from the base of the other pole. The spark then passed between the indium and the platinum. The red and blue lines of hydrogen were then observed both by my friend Mr. G. W. Hemming, Q.C., and myself. Their brilliancy was most markedly increased. This unmistakable indication of the presence of hydrogen, or rather of that form of hydrogen which gives us the *h* line alone *associated into* that form which gives us the blue and red lines, showed us that in the photograph we were not dealing with a physical coincidence, but that in the arc this special form of hydrogen had really been present; that it had come from the indium, and that it had registered itself on the photographic plate, although ordinary hydrogen persistently refuses to do so. Although I was satisfied from former experiments that occluded hydrogen behaves in this respect like ordinary hydrogen, I begged my friend Mr. W. C. Roberts, F.R.S., chemist to the Mint, to charge a piece of palladium with hydrogen for me. This he at once did, and I take this present opportunity to express my obligation to him. I exhibit to the Society a photograph of this palladium and of indium side by side. It will be seen that one form of hydrogen in indium has distinctly recorded itself on the plate, while that in palladium has not left a trace. I should add that the palladium was kept in a sealed tube till the moment of making the experiment, and that special precautions were taken to prevent the two pieces between which the arc was taken becoming unduly heated.

To sum up, then, the facts with regard to hydrogen; we have *h* differentiated from the other lines by its appearance alone in indium; by its absence during the eclipse of 1875, when the other lines were photographed; by its existence as a short line only in the chromosphere of the sun, and by the fact that in the experiments of 1869 a very high temperature was needed to cause it to make its appearance.

With regard to the isolation of the F line I have already referred to other experiments in 1869, in which Dr. Frankland and myself got it alone.[1] I exhibit to the Society a globe containing hydrogen which gives us the F line without either the red or the blue one.

[1] See also Plücker, *Phil. Trans.*, 1865, part 1, p. 21.

The accompanying drawing shows how these lines are integrated in the spectrum of the sun.

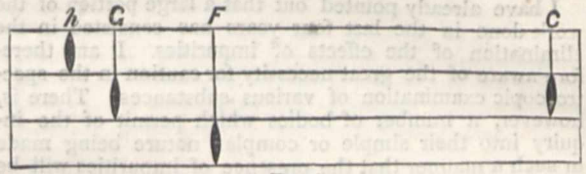

FIG. 6.

I have other evidence which, if confirmed, leads to the conclusion that the substance which gives us the non-reversed line in the chromosphere and the line at 1474 of Kirchhoff's scale, termed the coronal line, are really other forms of hydrogen. One of these is possibly more simple than that which gives us *h* alone, the other more complex than that which gives us F alone. The evidence on this point is of such extreme importance to solar physics, and throws so much light on star structure generally, that I am now engaged in discussing it and shall therefore reserve it for a special communication.

In the meantime I content myself by giving a diagram in which I have arranged the various groupings of hydrogen as they appear to exist, from the regions of highest to those of lowest temperature in our central luminary.

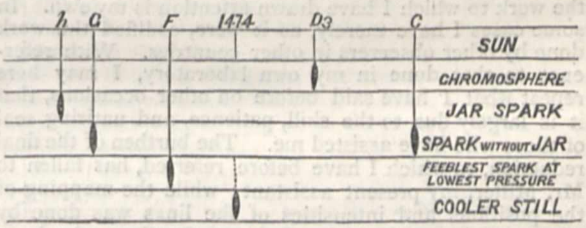

FIG. 7.

Summation of the above Series of Facts

I submit that the facts above recorded are easily grouped together, and a perfect continuity of phenomena established on the hypothesis of successive dissociations analogous to those observed in the cases of undoubted compounds.

The other Branches of the Inquiry

When we pass to the other possible evolutionary processes to which I have before referred, and which I hope to discuss on a future occasion, the inquiry becomes much more complicated by the extreme difficulty of obtaining pure specimens to work with, although I should remark that in the working hypothesis now under discussion the cause of the constant occurrence of the same substance as an impurity in the same connection is not far to seek. I take this opportunity of expressing my obligations to many friends who have put themselves to great trouble in obtaining specimens of pure chemicals for me during the whole continuance of my researches. Among these I must mention Dr. Russell, who has given me many specimens prepared by the lamented Matthiessen, as well as some of cobalt and nickel prepared by himself; Prof. Roscoe, who has supplied me with vanadium and cæsium alum; Mr. Crookes, who has always responded to my call for thallium; Mr. Roberts, chemist to the Mint, who has supplied me with portions of the gold and silver trial plates and some pieces of palladium; Dr. Hugo Müller, who has furnished me a large supply of electrolitically-deposited copper; Mr. Holtzman, who has provided me with cerium, lanthanum, and didymium prepared by himself; Mr. George Matthey, of the well-known metallurgical firm of Johnson and Matthey, who has provided me with magnesium and aluminium of marvellous purity; while to

Mr. Valentin, Mr. Mellor, of Salford, and other friends, my thanks are due for other substances.

I have already pointed out that a large portion of the work done in the last four years has consisted in the elimination of the effects of impurities. I am therefore aware of the great necessity for caution in the spectroscopic examination of various substances. There is, however, a number of bodies which permit of the inquiry into their simple or complex nature being made in such a manner that the presence of impurities will be to a certain extent negligable. I have brought this subject before the Royal Society at its present stage, in the hope that possibly others may be induced to aid inquiry in a region in which the work of one individual is as a drop in the ocean. If there is anything in what I have said, the spectra of all the elementary substances will require to be re-mapped, and re-mapped from a new standpoint; further, the arc must replace the spark, and photography must replace the eye. A glance at the red end of the spectrum of almost any substance incandescent in the voltaic arc in a spectroscope of large dispersion, and a glance at the maps prepared by such eminent observers as Huggins and Thalén, who have used the coil, will give an idea of the mass of facts which have yet to be recorded and reduced before much further progress can be made.

In conclusion I would state that only a small part of the work to which I have drawn attention is my own. In some cases I have merely, as it were, codified the work done by other observers in other countries. With reference to that done in my own laboratory, I may here repeat what I have said before on other occasions, that it is largely due to the skill, patience, and untiring zeal of those who have assisted me. The burthen of the final reduction, to which I have before referred, has fallen to Mr. Miller, my present assistant; while the mapping of the positions and intensities of the lines was done by Messrs. Friswell, Meldola, Ord and Starling, who have successively filled that post.

I have to thank Corporal Ewings, R.E., for preparing the various diagrams which I have submitted to the notice of this Society.

EXPERIMENTS IN ELECTRIC LIGHTING

MR. LOUIS SCHWENDLER, Superintendent Electrician of the Government of India, has been conducting a very careful series of experiments in London on electric lighting, with a view to decide upon the advisability of introducing this method of lighting into railway stations in India. He has just published a *précis* of his forthcoming report, and as the experiments were conducted on thoroughly scientific principles, and solely with a view to discover the most effective method, the results attained are extremely valuable, especially when so many systems are competing for public favour:—

First, with regard to quantity of light per unit of power, unit of speed, and unit of money (first outlay). To solve this question Mr. Schwendler tried four different dynamo-electric machines producing the electric current in *one* direction, viz., A, medium size, B, small; as supplied by Messrs. Siemens, Brothers, of London (construction:—Siemens, system:—Hefner von Alteneck). C, workshop pattern, as supplied by Messrs. Soutter and Lemonnier, of Paris (construction:—Gramme). D, with two sets of brushes as supplied by the British Telegraph Manufactory (construction:—Gramme). Mr. Schwendler finds these four machines all sufficiently practical for the production of the electric light, but, as a rule, the statements of their actual efficiency were not found to be in conformity with the results obtained from his own experiments. The quantity of light produced by these dynamo-electric machines had been over-rated, and the amount of power consumed underrated. But, notwithstanding this, he finds that the unit of light as produced in the electric arc (disintegration) by any of the four dynamo-electric machines is at least fifty times cheaper than the unit of light as produced by combustion, considering the expenditure of power only. This represents an enormous engineering margin in favour of the electric light.

Mr. Schwendler makes a most important proviso by stating that this relation only holds good as long as *one* dynamo-electric machine produces *one* electric light; he returns to this point in a subsequent part of his careful *précis*.

The three dynamo-electric machines B, C, and D, he found practically equal; the dynamo-electric machine A gives a much stronger light for a comparatively smaller expenditure of power. In round numbers it may be said that dynamo-electric machine A gives about double the quantity of light given by any of the other three machines, and that only about half as much power is expended to produce the unit of light. This favourable result, Mr. Schwendler states, is principally due to the comparatively small internal resistance of A, and its low speed.

Secondly, with reference to constancy and regularity of the electric light, Mr. Schwendler says that this appears to be still the weak point, and many improvements in this respect are possible and desirable. He has tried two different lamps:—*a*. The Serrin lamp, as supplied by Messrs. Soutter and Lemonnier, of Paris, and the British Telegraph Manufactory; *b*. The Siemens lamp, as supplied by Messrs. Siemens Brothers. The Serrin lamp, for any given adjustment, regulates the length of the arc only in *one* direction, *i.e.*, it diminishes that length. The actual consumption of the carbon points regulates the length of the arc in the other direction, *i.e.*, increases it. In the Siemens lamp the decrease of the length of the arc is effected exactly in the same manner as in the Serrin lamp, but the increase in the length of the arc is not only left to the consumption of the carbon points—a comparatively slow process—but is accelerated by the addition of a make and break arrangement, which separates the carbon points. Hence, from a theoretical point of view, the Siemens lamp is undoubtedly superior, since the length of the arc is rapidly adjusted in both directions, and consequently the working currents can increase to a very considerable degree without spoiling the dynamo-electric machine. But practically Mr. Schwendler finds the Siemens lamp somewhat difficult to manage, and although, when *once* well adjusted, it burns as regularly as the Serrin lamp, it is far more difficult to arrive at this adjustment. For practical use he prefers, therefore, the Serrin lamp, with those alterations and constructional improvements which his own experiments have suggested. A second cause of the irregularity of the electric light is the still imperfect state of the carbon points. Of late some great improvements have been made in the manufacture of artificial carbons, but much more is required, and this point deserves the closest attention. In his final report Mr. Schwendler will treat this subject in detail. To make the electric light more steady, Mr. Schwendler states, should be considered one of the most important questions to be solved.

Thirdly, Mr. Schwendler considers the question as to how to put up the light, its position, and mechanical details. Under this head he considers the method of dividing the electric light, *i.e.* producing by the *same electro-motor* a number of lights at different points of a given space. This method, which he does not consider as yet solved, appears to him impracticable from an engineering point of view. He refers to the immense loss of strength in thus dividing the light, increasing in enormous proportion with the increase of sub-division. Mr. Schwendler, after his careful, severe, and long-extended trials, comes to the decided conclusion that the electric light can alone compete with light produced by combustion, when produced of great intensity in *one* point by *one* dynamo-electric machine. Endeavours to cut up the electric light into a large number of small lights, although of great interest, must, he thinks, invariably result in engineering failure, as nobody could afford to pay for the luxury received. Thus, in the opinion of so competent a judge, all methods hitherto employed for using this method of lighting in public, are failures, involving a waste of power and money, with inadequate result. Having satisfied himself of the difficulty and impracticability of the division of the electric light, Mr. Schwendler tried diffusion, *i.e.*, a few large lights (each light produced by one machine) are placed at different points of the space, and by optical means the light is diffused over a large area. This method he finds perfectly practicable. There is naturally a large amount of light lost (by absorption), but, he states, this loss will bear a constant ratio to the total light produced, nay probably may decrease with the intensity. The actual plan by which Mr. Schwendler proposes to do it, and has done it during the trial, is to construct a silvered glass reflector in which a powerful electric light burns, throwing direct and reflected rays up to a white ceiling or any other convenient white surface. A number of such arrangements is to be put up in the most convenient places, and where they have the greatest effect. The form and size of each reflector will depend on the locality where it is to be used.

Any repairs required in course of time Mr. Schwendler believes can easily be effected by an ordinary mechanic. Only one man, he states, is actually required in each station, to take charge of the steam engine, dynamo-electric machine, lamps, and reflectors. Mr. Schwendler appends to this *précis* several details with reference to the adaptation of electric lighting to Indian stations, and also on some of the scientific results obtained by his experiments. Altogether his report is likely to be the most important contribution to a thorough knowledge of the public utilisation of electric lighting that has been made since the question has been resuscitated during the last year or two; and we should advise all interested in the subject to wait for his report before taking decisive steps to the adoption of any particular system.

NOTES

THE Emperor of Germany has approved of the election of Mr. Darwin and Prof. Owen as Foreign Members of the Berlin Academy of Sciences.

WE are glad to learn that M. Raoul Pictet is quite restored to health. The University of Geneva has conferred upon him the honorary degree of Doctor, and he has just been made a Chevalier of the Legion of Honour by the French Government, in recognition of his eminent services to science, and especially of his successful experiments in the liquefaction of gases.

A COMPETITIVE examination is going on at the Paris Conservatoire des Arts et Métiers, for the appointment of a professor of physics and meteorology at the National School of Agriculture. The examination has been conducted on a new principle by a jury presided over by M. Boussingault. Each of the seven candidates has expounded before the jury his programme of lectures to be delivered, and each of them has in turn delivered a lecture on physics, and another on meteorology, after a preparation of four hours. The competition is open to all without any condition of age, qualifications, and nationality; but the jurymen are instructed to attend, in giving their verdict, to the degrees obtained by candidates and their previous work or discoveries.

THE Municipal Council of Paris has voted a subvention of 2,000 francs to M. Joseph Vinot, editor of the *Ciel*, for a series of popular lectures on astronomy, to be delivered at the Salle des Ecoles, rue d'Arras. Admiral Mouchez, who was present at the last lesson, announced to the pupils, numbering from 400 to 500, that he will take measures to admit them to the observatory *seriatim*, in order to initiate them into the use of the large astronomical instruments so ably described by their professor. In one of the last reports read before the Paris Municipal Council it was stated that it would be necessary to establish somewhere in Paris an observatory of popular astronomy entirely devoted to the public exhibition of celestial phenomena, other establishments being entirely devoted to investigation.

PROF. S. P. THOMPSON has reprinted his valuable address on "Technical Education," given at the Social Science Congress last October. In this time of intense depression, when trade seems to be drifting from our shores, and people are wondering how it is that other nations are outstripping us in departments that used to be considered as peculiarly British, Prof. Thompson's remarks on the ignorance of our mechanics are peculiarly appropriate. One telling instance he gives of the lamentable want of intelligent skill that prevails among workmen and manufacturers in this country:—"I was recently informed by Prof. Graham Bell that he is about to return to America to resume his researches in telephony, his principal reason for quitting his native shores once more being that he found himself, in this country, unable to get his ideas carried out, unable to procure workmen capable of comprehending and carrying out new ideas, such workmen, in fact, as he was able to employ during his four years' residence in America. He pointed to the laboratory of Mr. Edison as an example of an institution to which there is no parallel in this country, though there are several in the States, a laboratory equipped with a staff of trained workmen, Americans, Germans, or Englishmen, whose business is not to work on old lines, but to carry out and put into practical form new and untried devices. No wonder inventions multiply when inventors have so powerful an aid as this to further their designs; and, mark this, Mr. Bell returns to set up a similar laboratory because he cannot find in his native country men whose technical training would qualify them for his particular work." In a note Mr. Thompson gives the following paragraph from a letter of Prof. Graham Bell to a friend in America which has been going the round of the American press:—"If you want to know why inventors are more numerous in America than they are here, come and live for six months in England. If you wish to know how it feels to be brimfull of ideas, and yet to be unable to have one of them executed, come to England. If you wish to know how it feels to have to wait for a month to have the simplest thing made, and then be charged a man's wages for two months, come to England. You will here be unable to see the interior of a workshop or to come into direct contact with your workmen, and the people seem incapable of working except in the ruts worn by their predecessors. They are absolutely incapable of calculating any new design without the most laborious oversight from the inventor, and their masters, instead of encouraging invention, do all they can to put a stop to it by refusing admission to the workshops and charging the most exorbitant prices for experimental work, avowedly because 'they don't want such kind of work,' 'it gives more trouble than it is worth;' and 'if you must have new things made you must expect to pay for them!' It is in vain that I say I am willing to pay anything to have my work done, and that what I object to is having to pay for not having it done. It is the same everywhere. Not only is your work not done, but you have to wait so long for the simplest things that your ideas cool, and you get quite exasperated at your inability to do anything." The moral of all this is obvious.

THE widow of the late Prof. E. Eichwald has presented the remarkable palæontological collections of her husband to the St. Petersburg University, which already possessed a part of them. These collections, collected by the late Prof. Eichwald since 1825, number no less than 30,000 specimens of fossils from the various formations of Western Europe, from the Petchora Land, from the Aleutian Islands, Siberia, Crimea, &c.; several European geologists as well as the late Mr. Peabody, have many times negotiated for the purchase of them.

ON December 22, Prof. Forster, Director of the Bern Observatory, gave to the Bern Economical Society a very interesting report on weather-warnings. After a sketch of the development of these warnings during past years in Europe, he pointed out the importance of the "Service Agricole," established between France and Austria, by which daily telegrams are sent, advertising the coming weather; 85 per cent. of these prognostications having been perfectly true, 7 per cent, approximately true, and only 8 per cent. untrue. As to the introduction of such a service in Switzerland, it would meet with great difficulties because of the great variety of topographical features of the country and of the incomplete knowledge of the local climatic conditions. These last having been, however, carefully studied since 1864, the first steps towards the establishment of such a service were recently made by the Swiss Meteorological Commission, some of whom wished to undertake immediately to introduce weather warnings into Switzerland, whilst the majority of the Commission was for the adjournment of them for one or two years. The Society expressed their desire for the introduction of weather prognostics as soon as possible, and we may hope that shortly Switzerland also will have its weather warnings.

The Association Scientifique de France will commence at the Sorbonne its series of lectures for 1879. On January 16 M. Milne-Edwards, president of the Association, will be in the chair. M. de Lesseps will lecture on Central Africa. The lecture will be illustrated by dissolving views made with drawings sent by Gordon Pasha from the newly-annexed Egyptian provinces.

THE *Reale Istituto Lombardo di Scienze e Lettere* has recently published a list of subjects for prizes to be awarded in this and following years. Among these subjects are the following:—Nosological geography of Italy; critical history of the telephone; on the nature of miasma and contagion; on the direction of balloons; is the generative material of hydrophobia a virulent principle or an organic germ? history of the progress of anatomy and physiology in the present century, especially with regard to the doctrine of Gall; illustration of some facts of the macro- or microscopic anatomy of the human brain; motor centres of the cerebral system; statistics of motor force, hydraulic and steam, in and around Milan.

THE Borough engineer of Liverpool has issued a not very encouraging report on the result of his visit to Paris for the purpose of examining the systems of electric lighting in use there. They are much more expensive than gas in Liverpool, and more than 50 per cent. of the light is absorbed by the globes used. Nevertheless he recommends a trial in Liverpool to find if no more economical method can be discovered. We recommend to him a perusal of Mr. Schwendler's report referred to elsewhere.

WE have received from Spain two numbers of a fortnightly scientific journal, published at Barcelona, which we welcome as a hopeful sign of progress in that country. The *Crónica Científica*, as the journal is called, while containing considerable extracts from foreign journals, and reports of foreign science, has a fair proportion of original contributions from Spanish investigators.

WE have received the number of the *Anales* of the Argentine Scientific Society for November. Among the papers is the continuation of M. Carlos Berg's monograph on the *Hemiptera argentina*, and a paper by Dr. D. Tomás Peron on the bark of Quebracho Blanco (*Aspidosperma quebracho*). Among the honorary members of this Society we notice the name of "Dr. Carlos Darwin;" while "Juan Lubbok, Londres," is a corresponding member.

IN connection with the Yorkshire Naturalists' Union, a "Grand Exhibition" of natural history specimens and scientific apparatus will be held from January 10 to 16 at the Mechanics' Institute, Leeds. On January 10 the Exhibition will be opened by the President of the Union, Mr. H. C. Sorby, F.R.S. The exhibition will be opened every succeeding day from 10 A.M. to 10 P.M., and will include in the various departments of natural history a large number of objects, including some of the finest private collections in the county, whilst the Physical Science Department will contain all the latest scientific novelties, including the apparatus for the liquefaction of oxygen, &c. There will also be a quantity of apparatus seldom exhibited to the public, together with instruments used in important original researches. Every evening a series of demonstrations and short addresses, and selections of music will be given. Microscopes and aquaria will be constantly on view. In the basement will be shown the process of manufacture of scientific apparatus. Although the announcement of this exhibition smacks a little too much of Barnum and the penny show, still the exhibition and lectures seem likely to be productive of permanent good in the district.

THE International Piscicultural Exhibition which will take place at Berlin in the spring of next year promises to be extremely interesting. A number of English, Russian, American, and even Japanese and Chinese exhibitors have already promised to take part in it. The Crown Prince of Germany has undertaken the protectorate of the exhibition and takes a great interest in its success.

HERR C. RECLAM has recently made a detailed report of the first cremation which took place at Gotha a short time ago. He calculates the cost of each cremation at about 4*l.*, which in case the furnace is in continual use, so that between two processes it has not time to cool, would be reduced to 3*l.* It is hoped in Germany that the example of Gotha will soon be followed by other cities so that Milan and Gotha will no longer be the only cities where cremation takes place.

PROF. ASA GRAY, we learn from *Harper's Weekly*, announces the detection, after the lapse of a hundred years, of a plant obtained by Michaux in the mountains of North Carolina, and known as the *Shortia galacifolia*, the re-discovery also having occurred in M'Dowell County, in North Carolina, in a region east of the Black Mountains.

Science News for December 15 contains some "Later Notes on Texan Birds," by George B. Sennett, the result of a second journey to south-western Texas made last spring. The Notes chiefly refer to nesting habits of the more peculiar forms. The region was on the banks of the Rio Grande within a few miles of its mouth, with Lomila Ranche as the central point.

"VÖGELBILDER aus fernen Zonen" is the title of an atlas of foreign birds just published by Fischer, of Cassel, under the care of Dr. Ant. Reichenow. The first part, before us, contains three beautifully coloured plates of birds, artistically grouped amid suitable surroundings, each plate having explanatory text, in which each species represented is pretty fully described, giving not only the scientific but also the native, German, French, English, and other names of the birds. The three plates are devoted to parrakeets and allied kinds. The same publisher sends us a series of large zoological wall-plates, admirably adapted, it seems to us, to teaching zoology in a thoroughly scientific manner. The plates sent us are mainly devoted to the lower forms, the specimens selected being enormously magnified, coloured, and exhibiting all details both of external and internal structure.

"THE Year Book of Facts in Science and the Arts," edited by James Mason (Ward, Lock, and Co.), is little better than a scrap-book of cuttings from various papers; we don't suppose it is seriously intended to represent the science of the past year.

MESSRS. HARDWICKE AND BOGUE have issued a second edition, "revised and corrected," of Mr. M. P. Edgeworth's work on "Pollen," noticed in our columns on its first publication (NATURE, vol. xvi. p. 499).

AT the last meeting, December 6, of the Russian Geographical Society, Col. Rykatchoff made a communication on the difficulty of organising observations on rain and storms.—Prof. Meller made a very interesting communication on the former connection between the Sea of Azov and the Caspian. The character of the geological formations on the tract between both seas proves without doubt that during the tertiary epoch the waters of both were connected; thus organic remains of Caspian origin are found within eighty-seven miles from the actual shores of the Sea of Azov.

AT the last meeting of the St. Petersburg Physical and Chemical Society, Prof. Beketoff made a communication on the atomic heat-capacity of hydrogen when mixed with palladium. He determined it as equal to 5·86, *i.e.*, very near to that of copper and silver.

THOSE of our readers who are in the habit of using chemical apparatus should get the Revised List just issued by Mr. Fletcher, of Warrington, who deserves credit for the very successful efforts he makes to introduce improvements into this department.

THE much-talked-of canal between Delaware and Chesapeake Bays, which will shorten the water-route from Baltimore to New York and Europe by 225 miles, is now at last to be constructed. Its length will be seventeen miles, and the cost is estimated at four million dollars. It will run through the Sassafras Valley and will have no locks.

THE additions to the Zoological Society's Gardens during the past week include a Punjaub Wild Sheep (*Ovis cycloceros*) from India, presented by Col. W. R. Alexander; two Californian Quails (*Callipepla californica*) from California, presented by Mr. William Turquand; seven Brown Tritons (*Geotriton fuscus*), South European, presented by Prof. H. H. Giglioli, C.M.Z.S.; a Feline Dowrocouli (*Nyctipithecus felinus*) from South America, purchased.

FURTHER RESEARCHES ON THE SCINTILLATION OF STARS

SINCE last we reviewed M. Montigny's valuable researches on the scintillation of stars (vol. xviii. p. 292) he has again published some highly interesting details. The researches now in question refer specially to the changes of colour which characterise the scintillation of the *red* and *orange* stars. M. Montigny tried to solve the question whether the changes of colour in scintillation follow certain definite laws; whether, for instance, their relative frequency expressed in numbers, shows differences which depend on the nature of the star's own light, on the star's elevation above the horizon, or on the condition of the atmosphere.

In order to solve this complicated question, it was divided into its several parts. First of all M. Montigny investigated the influence of the star's own light and that of the condition of the atmosphere upon those colours which characterise the scintillation of the stars of the so-called third type. Our readers will remember that these are the stars which show black lines as well as dusky bands in their spectra; they are generally of a red or orange colour, and mostly variable. There are not many fine stars in this class, the most remarkable ones are about thirty in number, and M. Montigny has examined only the following fifteen:—β Andromedæ, α Ceti, ρ Persei, Aldebaran, Betelgeuze, α Hydræ, Arcturus, δ Virginis, δ Coronæ, α Serpentis, Antares, α Herculis, γ Aquilæ, and β and ϵ Pegasi. The evenings of observation now number 476, and reach from October, 1870, to February, 1878.

The way in which the observations were made was the following:—After each evening of observation not only the values for the intensity of scintillation were entered for each star, reduced to a distance of 60° from the zenith, but each single colour observed in the circular image was also noted down. Further, the observations made in wet weather were noted separately from those made during dry weather. Finally the various colours were entered on a table divided into seven columns, respectively headed—red, orange, yellow, green, blue-green, blue, and violet. The sum total of any column thus indicates the number of times which the colour in question was observed in a certain star. Arcturus, for instance, in 131 observations during moist weather, showed the red colour 130 times and blue 118 times. These numbers thus express the absolute frequency of these two colours. If we compare the number 130 for red, with the sum total of all colours shown by Arcturus during rainy weather, which is 491, then we obtain the relative frequency of red, which is 0·265, or multiplied by 1000 = 265. Therefore in 1000 changes of colour which appeared in Arcturus during rainy weather, red occurred 265 times, and blue 240 times.

In the following table we give the average frequencies of the different colours for the fifteen stars of the third type enumerated above; line A shows the frequencies observed in rainy weather, and B those observed in dry weather. The total of observations was 800 for A and 368 for B; the totals of the changes of colour observed were 2,982 for A and 1,368 for B.

	Intensity.	Red.	Orange.	Yellow.	Green.	Blue-green.	Blue.	Violet
A ...	60	272	194	239	57	4	230	3
B ...	43	278	213	222	63	5	216	4
Average	52	275	204	230	60	5	223	4

We observe in this table that (1) the relative frequency of red is far greater than that of any other colour in rainy weather as well as in dry; (2) red, green, and particularly orange are seen more frequently in dry weather than in wet; (3) the frequency of yellow and blue is on the contrary greater in wet weather than in dry.

Although the differences in the frequency of one and the same colour, according to the state of the atmosphere, are rather limited, they nevertheless indicate an important fact. It is also remarkable that the numeric differences in the complementary colours red and green on the one hand, and blue and yellow on the other, lie in the same direction. It is further worthy of notice that the greater frequency of blue in rainy weather agrees well with the fact that blue greatly predominates during such weather in the image of the star as shown by the scintillometer. This predominance of blue has also been frequently observed a short time previous to rainy weather.

The following table will be found interesting, as it contains the changes of colour and intensity of scintillation of the six brightest stars of the third type. In line I. are those of Betelgeuze, which is orange coloured, and the spectrum of which shows numerous broad bands, dissolvable into lines; line II. gives those of Aldebaran, pale red, whose changing spectrum has many well-defined lines and dark bands; line III. represents Arcturus, yellow-orange, with numerous dark lines not united into bands in its spectrum; line IV. gives those of α Hydræ, a yellow star with very dark lines in the spectrum; line V. those of Antares, red, with wide bands and very distinct lines; and line VI. those of α Herculis, yellowish red, with black lines and dark bands.

	Intensity.	Red.	Orange.	Yellow.	Green.	Blue.	Violet.
I. Betelgeuze ..	65	255	190	234	106	202	13
II. Aldebaran ...	62	255	186	232	104	210	13
III. Arcturus ...	61	253	120	246	130	219	32
IV. α Hydræ ...	55	284	162	253	113	188	—
V. Antares ...	53	266	121	245	130	219	33
VI. α Herculis ...	47	275	225	232	51	217	—
Average	57	265	167	240	106	209	15

These values show that the relative frequencies of the three principal colours, red, orange, and blue, remain within narrow limits for the six stars. Yet red seems to increase in frequency in the three last stars, two of which are of a decided red tint, and whose intensity of scintillation is far smaller than that of the other three. With regard to the effect of the star's own colour it must be remarked that the relative frequency of that colour or of a nearly related one is often very great; thus, for instance, yellow is very frequent in the yellow star α Hydræ, and orange in the orange stars Betelgeuze and α Herculis. The total number of observations M. Montigny made of these six stars was 574.

For the sake of comparison M. Montigny has calculated the relative frequency of colours in two stars of the second type, to which, as our readers will remember, our own sun belongs too. The stars selected were Pollux, with a very characteristic spectrum, and Capella, which scintillates with great regularity. Both stars are yellow and their spectra show very thin dark lines. The average frequency of colours for these two stars is given separately, A in rainy weather, and B in dry weather, from a total number of 267 observations of Capella and 116 observations of Pollux.

	Intensity.	Red.	Orange.	Yellow.	Green.	Greenish blue.	Blue.	Violet.
A ...	88	281	88	280	86	7	250	8
B ...	63	299	41	304	122	26	194	14
Average	76	290	65	292	104	17	222	11

If we compare these results with the former ones we find the frequency of red and particularly that of yellow to be considerably greater in Capella and Pollux than in stars of the third type, while the frequency of orange is very much less, having decreased from 204 to 65. The influence of the weather was equally apparent in these stars; in dry weather red was more frequent, and in rainy weather blue. The relative values of green and violet are greater in these two stars than in the fifteen stars of the third type; probably these colours will become more important when the observations are extended in a larger measure to the stars of the two first types.

It is therefore proved beyond doubt, by the results above mentioned, that the changes of colour which characterise the scintillation of stars, are subject to general laws which are quite as regular and fixed as those which govern the changes in the intensity of the phenomenon as we pass from one type to another, or under the influence of rain and fine weather.

In one of the last numbers of the *Bulletin* of the Brussels Royal Academy of Sciences M. Montigny publishes the results of some researches concerning the influence of the aurora borealis on the scintillation of stars. We may return to this subject at a future date.

NOTES FROM NEW ZEALAND

MR. T. H. POTTS, of Ohinitahi, N.Z., sends us the following notes:—

Which Species of Pinus have Cones really Sessile?—In working up the habits of a collection of pines one has felt a difficulty in understanding why certain cones should be termed sessile; for example, the cone of *P. tuberculata* is described by Gordon in his "Pinetum;" also by Broun, in his "Forester," as "quite sessile." Why? Can such a description be correct at its early stage of life? It is then perched on a scaly foot-stalk, well developed; months elapse, its increasing bulk is protected with needle-pointed scales, its foot-stalk becomes curved, but is plainly visible; the mature cone, grey and glossy, clings tightly to its stem, it can scarcely be removed therefrom without tearing off a shell of bark adhering to the nasal scales; when wrenched off it shows a portion of its curved foot-stalk that has been embedded in the growing stem.

A very similar habit may be observed in *P. insignis*. Should not the cones of *P. tuberculata* and those of other species showing a similar habit, be described rather as *apparently sessile* than as "quite sessile"?

It may be mentioned that here *P. tuberculata* bears cones not only on the stem and main branches, but also on the soft green shoots of the outer branches, this would in part account for the foot-stalks becoming embedded in the growing bulk of the shoot.

P. insignis here bears cones of longer dimensions than those given by the authors before named; five specimens measure rather over seven inches in length, with a circumference of eleven inches.

Heredity.—One of my sons returning from a visit to the Chatham Isles, brought back with him a young pup of a famous colly breed. As soon as it was grown enough to run about it displayed an unusual excitement in the presence of horses by jumping upwards repeatedly towards their heads. As this trick or vice was unknown or unpractised by any of our dogs, it was, of course, soon remarked. On inquiry of a Chatham Island settler, I found this was a common trick in the colly family "Bell" sprang from; so Bell faithfully held to the habits of her race.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE following programme of the Natural Science Courses in Trinity College, Dublin, may be of interest to our readers:—In the Junior Sophister year natural science is represented by two courses—one in zoology, the other in botany. Students attending a course of lectures in natural science are examined three times during the term on the subjects of the lectures, and no student is allowed credit for his attendance who does not answer sufficiently. In the Senior Sophister year natural science is represented by one course in geology. In each term examinations are held of those students in the Junior and Senior Sophister classes who are qualified to become candidates for honours. At the Michaelmas examination in the Junior Sophister year prizes of 4*l.* and of 2*l.* are awarded by the Board, on the recommendation of the Honour Examiners, to the best answerers among the candidates. At the Hilary and Trinity examinations of the Junior and Senior Sophister years honours without prizes are awarded—of which honours there are two ranks. At the Michaelmas examination of the Senior Sophister year, examinations for Moderatorships are held. The Senior Moderators receive gold medals, and the Junior Moderators silver medals, which are given to them publicly before the University, by the Chancellor, at the commencements, when they are admitted to their degrees. The First Senior Moderator receives a large gold medal if specially recommended by the Court of Examiners. The subjects of examination for the Moderatorships in natural science are the following, each of which has equal weight:—1. Physical Geography, Geology, and Palæontology. All the ordinary and honour courses of the Sophister years. A limited course to be announced each year. Course for 1879:—The Silurian Period. 2. General and Physiological Anatomy. A limited course to be announced each year. Course for 1879:—Circulation and Respiration. The Circulatory and Respiratory Organs in Mammals. The Respiration and Assimilation in Plants. 3. Zoology and Botany. All the ordinary and honour courses of the Sophister years. A limited course to be announced each year. Zoological Course for 1879:—The Non-Placental Mammals. Botanical Course for 1879:—The Fucoids. The Professors of Zoology, Botany, and Geology, give each a course of demonstrations and a course of lectures in each term, especially meant for Junior Sophisters. The Museum of Comparative Anatomy and Zoology is open, under the superintendence of the professor, to all students, to whom every facility is given for the prosecution of their studies. For the purposes of study fresh specimen of plants are, under the superintendence of the professor, to be had by the student from Mr. Frederick Moore, at the College Botanical Gardens, at Lansdowne Road; and a large number of mounted specimens of cryptogamic plants are also to be seen and examined in the herbarium. The Museum of Geology and Palæontology is open to the students attending the course.

THE Science and Art School of St. Thomas' Charterhouse Institution, Goswell Road (the largest in the United Kingdom), under the direction of the vice-chairman of the London School Board, commenced a new term on Monday evening the 6th inst. Since the commencement of the present session upwards of 600 of our elementary school teachers of London have taken advantage of the privileges offered by the classes. The attendance at the classes for experimental work in chemistry and physics has been very large.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, November, 1878.—Some time ago Prof. Thurston invented apparatus for re-determining the coefficients of friction of lubricated surfaces, and the laws governing such friction, for a wide range of temperatures, pressures, and velocities. The machines have been in use about five years, and have furnished much useful information, some of which is here embodied in the opening paper.—Mr. Cooper investigates the driving power of leather belts.—Prof. Chase gives a series of verifications and confirmations of astronomical hypotheses and predictions, and a brief statement of the principles on which they are based.

Journal de Physique, November, 1878.—In a contribution to the theory of vowels, M. Bourseul, after pointing out that there are really as many distinct vowels as there are pitches of sound proper to the mouth, enumerates the vowels (ten in number) which he uses in speaking French. On examining the sounds of the mouth-cavity which correspond to them (apart from absolute pitch), he finds they fall into two divisions, one being in perfect accord with *do* major (*mi, do, sol, mi, do*), the other with *re* (*fa, si, re, fa, si*). This confirms in an unexpected way a principle discovered by the philologist, M. Rœhrig, in 1848. He noted the marked difference, or rather antagonism, of certain consonants and certain vowels, and he arranged the latter in two distinct classes, one comprising *a, o, ou*, the other *é, eu, u*. When studying the Tartar and Finnish tongues, he remarked that the vowels in a word of Tartaro-Finnish idiom were invariably of the same class. The derived languages have undergone alterations, whereby the traces of this original principle gradually disappears, still it may be recognised.—With regard to vibratory forms of liquids on circular metallic plates, M. Decharme finds that the widths of the striæ are inversely proportional to the square roots of the numbers of vibrations of the corresponding sounds.—Prof. Crova explains his important mode of comparison of the radiations emitted by calorific and dark sources; and in their continued paper of experimental researches on magneto-electric machines, MM. Mascart and Angot take up mixed machines, *i.e.*, those composed of magnets and electro-magnets.

December, 1878.—M. Deprez here describes the way in which he solves a problem relating to the work of steam in locomotive cylinders, viz., to trace, at a distance, a curve whose abscissæ and ordinates are respectively proportional to the path traversed by the piston and the pressure exercised by the steam on the piston. The solution rests on two principles—(1) Giving the sheet of paper (for the curve) a motion rigorously proportional to that of the piston; (2) Measuring at a certain point and *instantaneously*, the pressure of the steam on the piston.—M. Terquem communicates a paper on the use of plane liquid sheets (from bars and connecting threads) for experimental demonstration and measurement of the superficial tension. The determination in this way agrees with that by observation of the ascent of liquid in a capillary tube, whereas the process of counting drops gives perceptibly higher numbers.—M. Macé du Légimay studies mathematically the subject of potential in electrodynamics and electromagnetism.

Verhandlungen des naturhistorischen Vereines der preussischen Rheinlande und Westphalens, thirty-fifth year, vol. v. part 1. (Bonn: Max Cohen and Son, 1878.)—Dr. Franz Leydig, herpetological reminiscences of Roesel von Rosenhof. Roesel was born in Nürnberg, 1705, and died there in 1759, and he wrote on amphibian reptiles, insects, crustacea, and spiders, worms and polyps, rotifers and infusoria.—Dr. Förster, a short monograph of some parasitical hymenoptera, in which many new genera and species are described.—P. Hesse, contribution to the molluscan fauna of Westphalia.—Dr. Theodor Wolf, on Cotopaxi and its last eruption on June 26, 1877, with two plates.—Dr. A. von Lasaulx, contribution to a knowledge of the igneous rocks (Eruptiogesteine) in the districts of the Saar and the Mosel, with two plates.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xi. fasc. xvi, xvii.—This contains reports connected with the award of prizes, announcement of subjects for new prizes, &c.

Fasc. xviii.—On the integration of algebraico-differential equations of the first order and degree by means of linear functions, by Prof. Casorati.—On the dominant diseases of vines, by Drs. Garovaglio and Cattaneo.

SOCIETIES AND ACADEMIES

London

Royal Society, December 19, 1878.—"On the Chemical Composition of Aleurone Grains," by S. H. Vines.

In the first part of the paper an account is given of some experiments confirming those of Weyl (*Zeitschr. f. physiol. Chemie*, Bd. i.), which show that the reserve-proteids are stored up in the seeds of the blue lupin in the form of globulins (vitellin and myosin), and that the conglutin extracted from them by Ritthausen is a product of the alteration of these globulins by the reagents used in the process of extraction. Mr. Vines' micro-chemical observations further show that these globulins constitute the aleurone-grains.

In the second part Mr. Vines points out that, in addition to the globulins, the aleurone-grains contain a proteid which is soluble in distilled water. Such a solution of this substance does not become turbid in boiling; it gives a precipitate on the addition of a drop of nitric acid, soluble in excess; it gives the xanthoproteic and Millon's reactions; it gives an immediate precipitate with acetic acid and potassic ferrocyanide; and it gives a bright pink colour when treated with excess of strong caustic soda on the addition of a drop of dilute solution of cupric sulphate. This substance does not dialyse. These properties show that it belongs to the group of the peptones, and that it most resembles the *a* peptone of Meissner, or, adopting Kühne's nomenclature, hemialbumose, a name which may be provisionally applied to this substance also. The discovery of this substance is of interest in connection with the researches which have been made during the last few years into the existence of peptic ferments in the seeds and other parts of plants.

Linnean Society, Dec. 19, 1878.—Prof. Allman, president, in the chair.—A note on South African orchids, by W. Mansell Weale, was read by the secretary, and a series of drawings illustrating the above, and also other plants from the same region were likewise exhibited on the part of Mr. Weale. He points out that the supposed generic characters of *Mystacidium* and *Polystachyon*, founded on the "two-legged" caudicles of the pollinia, are fallacious.—In a short paper consisting of a description of some rare shells, by Mr. Sylvanus Hanley, *Melania Limborgi*, from British India, and *Leptomya gravida*, of uncertain habitat, were specially referred to as being unusual in several respects.—An interesting communication on the relations of *Rhabdopleura* was made by the president.—Messrs. F. M. Campbell, J. L. Hamilton, and J. J. McAndrew were elected Fellows of the Society.

Entomological Society, Dec. 4, 1878.—Mr. H. W. Bates, F.L.S., F.Z.S., president, in the chair.—The following elections took place: Mr. T. P. Newman as a Member, and Mr. J. Walker, R.N., as a Subscriber.—Mr. Stainton exhibited a series of specimens of *Glyphipteryx schœnicolella* taken by Mr. Threlfull near Witherslack.—Mr. Wood Mason exhibited and made remarks upon a stridulating beetle belonging to the *Rutelidæ*.—Prof. Westwood exhibited some insects and diagrams illustrative of so-called monstrosity, and contributed remarks thereon.—Mr. McLachlan exhibited a series of cases of the larvæ of Trichopterous insects forwarded to him by Dr. Fritz Müller of Santa Catharina, Brazil, one of which, Dr. Müller stated, had the peculiar habit of living on trees in the water that collects between the leaves of *Bromeliæ*, in which tadpoles, the larvæ of dragon-flies and other aquatic animals were also to be found. Mr. Bates stated that rain-water collects at the bases of the leaves of these plants and remains there for nine months out of the twelve. Dr. Müller had also forwarded a photograph of a number of cases which he considered to belong to some species of *Hydropsychidæ*, and in his own words, "make a funnel-shaped entrance to their houses with a net of which no spider need be ashamed." In reference to the opinion of Dr. Müller as to the homologies which appeared to exist between the neuration of various *Lepidoptera* and that of *Trichoptera*, Mr. McLachlan expressed his own belief that in a linear arrangement the orders *Lepidoptera* and *Trichoptera* should not be widely separated.—The Rev. A. Eaton exhibited a piece of "Kungu cake" from Lake Nyassa district, where, according to Livingstone and others, it is used extensively as food by the natives, who manufacture it from large quantities of a minute insect, conjectured to be a species of *Ephemeridæ*. From an exhaustive examination, however, Mr. Eaton found it to be a minute representative of the *Culicidæ*, probably belonging to the genus *Corethra*. In connection with the subject of insect-food as used by man, Mr. Distant remarked, he had learned from Mr. Chennell that *Erthesina fullo*, a very common eastern hemipterous insect was largely eaten by the Naga Hill tribes of North-eastern India.—Mr. Meldola in reply to some queries forwarded to him, as to the chemical composition of the bodies of insects, remarked that the chitine, which comprised the horny external portion of the bodies of insects had been shown by analysis to contain about 6 per cent. of nitrogen; and as regards phosphates, Mr. Wm. Cole had burned some insects and found phosphoric acid in the ash.—Mr. Waterhouse forwarded for exhibition a living *Curculio*, found in an orchid house at Windsor, which was identified as one of the *Calandridæ*.—The Secretary read the report of the sub-committee appointed to consider the communication from the Board of Trade regarding the ravages of *Anisoplia austriaca* at Taganrog.—Mr. Butler communicated a paper on a collection of Lepidoptera from Cachar, North-east India.

Photographic Society, December 10, 1878.—James Glaisher, F.R.S., in the chair.—Papers were read, by Henry Cooper on a really reliable dry-plate process, by L. Warnerke on a case of the destruction of the latent image on washed emulsion, and its restoration, by W. Willis, jun., notes on the platinotype process, and by Edwin Cocking on the subjective and objective of pictorial photography.—Mr. Willis, in demonstrating his new platinum process, stated that ferric oxalate is sensitive to light, and then becomes ferrous oxalate; this, when dissolved in a hot solution of potassic oxalate, reduces the metal from chlorides and other salts of platinum. A sheet of paper is coated with a solution of ferric oxalate and potassic chloro-platinite, and then exposed to light under a negative; this produces a visible brownish ferrous image; it is then floated for a few seconds upon a hot solution of potassic oxalate and potassic chloroplatinite, the ferrous image becomes dissolved, and the combination thus formed reduces the platinum salt and forms the ultimate picture in metallic platinum.

VIENNA

Imperial Academy of Sciences, November 14, 1878.—The following among other papers were read:—On a meteoric stone which fell at Dhulia, Hindostan, in November, 1877, by Dr. Brezina.

November 21, 1878.—On the behaviour of halogen derivatives of aromatic bodies towards water and lead oxide, by Professors Lippmann and Schmidt.—On the Clintonite group, by Prof. Tschermak and Herr Sipörz.—On the meteorite fall of Tieschitz, by Prof. Tschermak.

PARIS

Academy of Sciences, December 30, 1878.—M. Fizeau in the chair.—The following papers were read:—Reply to M. Berthelot, by M. Pasteur. M. Trecul made some observations on the subject.—Borings undertaken by M. Roudaire, in view of the formation of an interior African sea, by M. de Lesseps. M. Roudaire writes on December 11, 1878, that having reached a depth of 18 metres, nothing but sand and water had been met with (no rocks). An explanation is offered of the exceptional tide of 2·50m. in the Gulf of Gabes.—M. Daubrée presented a map of the itinerary of Prof. Nordenskjöld in the glacial sea of Siberia, from August 7 to 27 last.—M. Cahours presented the three first vols. of the fourth edition of his treatise on general elementary chemistry. He indicates the additions and alterations made.—Report on the diplograph of M. Recordon, and his apparatus for use of the blind. This diplograph enables the blind person to produce ordinary writing and a seeing person to produce mechanically the characters the blind person can understand. It consists chiefly of two discs carrying respectively the signs and characters, and which are simultaneously applied to two sheets of paper, impressing the letter recognised by touch or by sight. M. Recordon is making a musical diplograph.—Harmotome and stilbite, by M. Gaudin. This is a study in atomic composition and arrangement (the two minerals contain 179 and 175 atoms respectively).—On electrochemical actions under pressure, by M. Bouvet. The decomposition of water by a current is independent of pressure. The quantity of electricity necessary to decompose a given weight of water is sensibly the same, whatever the pressure at which decomposition occurs. Oxygen and hydrogen, whatever the pressure, are liberated with equal facility, and there are no secondary phenomena causing recomposition, &c.—On the decomposition, at ordinary temperature, of an alkaline silicate by a salt of alumina (artificial hydrophane), by M. Monnier.—Determination, by M. Eylden's method, of the motion of the planet 103 Hera, by M. Callandreau.—On an interpretation of the imaginary values of time in mechanics, by M. Appell.—On an intuitive law according to which is distributed the weight of a solid circular disc, supported by an elastic horizontal base, by M. Boussinesq. The charge supported by each element of the base is that which would be directly over this element if we supposed the total charge distributed uniformly over the convex surface of a hemisphere, having the same base as the disc.—M. Joubert acknowledged M. Becquerel's priority in the experiment of magnetic rotation of the plane of polarisation under the earth's influence. The account had escaped his notice.—On a very precise way of observing the contact between the mercury and the ivory point in the basin in Fortin's barometer, by M. Goulier. The author has adopted a method similar to that indicated by M. Le Chatelier many years ago.—On the use of the telephone and microphone for scientific researches, by M. Hughes. This shows how delicate a means of observation these two instruments afford in researches relating to very weak currents, such as those from movement of a magnet before a helix. Several experiments are detailed.—On a new electric lamp, by M. Ducretet. The chief feature of this is the use of a column of mercury in which are immersed one or several crayons; the difference of density produces a thrust, which brings the crayons constantly and regularly to their point of application in proportion as they are consumed. One part of them becomes incandescent. An equal resistance in the circuit is insured, whatever the length and consumption of the crayons.—On the existence and conditions of formation of oxide of nickel, Ni_3O_4, by M. Baubigny.—On the nitrates found in beets and some other roots, by M. Barral. The greatest quantity of nitre per cent. of dry matter is found in the largest beets, and also in those that have least sugar. Beet is thus often given injuriously to cattle. In carrots, potatoes, and hay, 1 cc. (at the most) of bioxide of nitrogen was got in treating 5 to 10 grammes of dry matter, whereas for various beets the quantity never came below 14 cc.—Inertia of derivatives of chromium compared with the action of vanadium on salts of aniline in presence of chlorates in printing with aniline black, by M. Witz.—Analysis of raw sugars and saccharine matters; determination of water and all salts with mineral bases and organic acids, by M. Laugier.—On the harmlessness of borax in conservation of meat, by M. de Cyon. In M. Jourdes' process the borax is sprinkled lightly on the surface, and the meat retains its nutritive value. Prof. Panum, of Copenhagen, has proved the innocuity of borax and boric acid in meat-preserving.—Researches on the physiological action of *maté*, by M. Couty. It excites only, or at least primarily, the sympathetic system in those organs that are most independent of the nerve-centres; such as the intestines, the bladder, the accelerating nerves of the heart.—Poison of serpents, by M. Lacerda. The poison of certain serpents contains figured ferments showing remarkable analogies to bacteria.—On the function of chlorophyll in green Planaria, by M. Geddes. The gases they give off in sunlight contain 45 to 55 per cent. of oxygen, the rest nitrogen; hardly any carbonic acid.—Geological observations on Majorca and Minorca, by M. Hermite.

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