THURSDAY, OCTOBER 14, 1880

THE INDIAN FAMINE COMMISSION

THE recently issued Report of the Government Commission appointed some time ago to inquire into Indian Famines is of great practical value and full of suggestiveness as to the lines which further inquiry should pursue. This first part of the Report relates to Famine Relief, and bears evidence that the Commission have done their work with great thoroughness and breadth of view, and the results are recorded with clearness and method. On the question as to what measures of relief would be the most effectual to adopt, we need not touch here; no doubt they will receive attention in the proper quarter. The discussion of the various questions involved is prefaced by an excellent concise sketch of the geography, population, and climate of British India. Here also some important information is given as to the degree in which each part of the country is exposed to famine. This is followed by a statement of the measures which, in the opinion of the Commission, it would be advisable to adopt for famine relief, and a very complete and instructive review of past famines and the measures adopted to meet them. The immensity of the problem with which the Commission had to deal may be learned from the fact that the total area of British India is about one and a half million square miles with a population of 240 millions. Of this, 900,000 square miles, with a population of 190 millions, is under direct British rule, the remainder belonging to the native States. The great bulk of this population belongs to the classes on whom the dire effects of famine are sure to fall, so that the responsibility of our government in the matter cannot be magnified; they are bound to leave no means untried either to prevent the recurrence of famines or to meet them effectually if they do occur. The Commission, of course, could not but come to the conclusion that the devastating famines to which the provinces of India have from time to time been liable are in all cases to be traced to the occurrence of seasons of unusual drought, the failure of the customary rainfall leading to the failure of the food crops on which the subsistence of the population depends. The Commission have therefore justly conceived it to be an important part of their inquiry to ascertain what can be known as to the periodicity of rainfall throughout the year, and over periods of greater extent if possible. The yearly periodicity of rainfall in India and other tropical countries is well known. In India a strongly marked yearly periodicity is everywhere observed, the chief fall occurring, with few exceptions, in the summer months, between May and October, in the season commonly known as the south-west monsoon. On a part of the Madras coast, on the east of the peninsula, heavy rain falls after the cessation of these summer rains, in the months of November and December, at the beginning of what is termed the season of the north-east monsoon. In the more northern provinces, again, a well-marked season of winter rain occurs, commencing about Christmas and extending to February, but its effects hardly reach south of the tropic, and it has no sensible influence on the agriculture of Southern India. The main agricultural operations of the country correspond with these principal seasons of rain, and their relative importance is in a great degree dependent on the local distribution of the rainfall at the various seasons of the year, as the period and amount of rain differ much in the several provinces of India.

A most valuable feature of the Report is the numerous excellent maps which accompany it, and which are a great assistance to understanding the results of the inquiry. One map, for instance, shows the general features of the distribution of annual rainfall. The fall on the Western Ghats and on the tract between them and the sea is very heavy, being from 70 to 100 inches at the sea level, and as much as 250 inches on the mountain face exposed to the south-west rain-bearing winds. Along the east coast of the Bay of Bengal, and in the eastern districts of the Bengal Province, as also along the foot and outer slopes of the Himalaya throughout its whole extent, the rainfall is also extremely heavy, reaching 100 inches or more. Subject to these exceptions, it may be said generally that the portion of India east of the 80th meridian has a rainfall of more than 40 inches, while the portion west of the same meridian has less than 40 inches. The region in which the fall is less than 30 inches includes almost the whole of the Punjab, a considerable part of the North-West Provinces, a large part of Rajputana and Kathiawar, as well as almost the whole of the Deccan and Mysore. In Sindh and in the southern portion of the Punjab and most western part of Rajputana the rainfall is extremely small and irregular, being less than 15 inches. Of the area in which the rainfall is below 15 inches, it may be said that it is either actual desert or that agriculture is impossible without artificial irrigation; and hence it has followed that where the rain is least copious the population has made itself in a great degree independent of the local rainfall. In the opposite direction it is also generally true that where the rain is most abundant, exceeding 40 or 50 inches, the occurrence of such drought as will cause serious scarcity is rare. The region in which the average rainfall is between 20 and 35 inches is that which suffers most from droughts. Here, though on the average of years the rain is sufficient to support an agricultural population, the greater deficiencies which reduce the quantity below what is essential, as well as the smaller which seriously damage the crops, are so frequent as to lead to repeated seasons of scarcity of greater or less severity. From this it can easily be ascertained what are the parts of the country most subject to drought. These are (1) the western and southern parts of the North-Western Provinces and that portion of the Punjab territory which lies east of the Satlej; (2) the western and northern States of Rajputana and of the central plateau which border on the North-Western Provinces; (3) the districts of Bombay above the Western Ghats, and the districts of Madras above the Eastern Ghats, together with the southern and western region of Hyderabad and all Mysore, except the strip lying close along the Western Ghats; (4) the districts of Madras along the east coast and at the extremity of the peninsula. The more detailed account of the known droughts of the past hundred years, which are given, show how frequently the region whose total rainfall is from 20 to 35 inches has been subject to severe scarcity, and that within it have occurred the great famines of 1837-38 in the North-West Provinces, of 1868-69 in Rajputana, and of 1876-77 over nearly the whole of the peninsula of Southern India. These droughts were mainly due to the failure of the south-west monsoon. The drought of 1865-66, and some of the earlier scarcities in Madras, arose from failures of the rain of the north-east monsoon on the east coast, a failure which in 1865-66 extended into Western Bengal. The famine of 1873-74 in Northern Bengal was exceptional, and is an instance of a great scarcity suddenly arising in a region of abundant average rainfall. This drought arose from a premature cessation of the rain, apparently due to an abnormal extension to the eastward of the margin of the comparatively dry area of North-Western India.

The Report touches briefly on a part of the subject which we deem of the greatest importance, namely, the supposed periodicity of fluctuations in the rainfall from year to year. These, the Report states, are in all parts of the country very considerable, variations of as much as 50 per cent. on either side of the average being often registered. The Commission refer to the opinion of those "qualified by their scientific knowledge to judge of such matters that there is evidence of these fluctuations being in some measure synchronous with those periodical variations in the condition of the sun which are indicated by the varying extent or number of sun-spots; and the recurring cycle of about eleven years, with which prolonged observation has shown that the period of sun-spot variation on the average accords, has been thus considered to correspond to the annual variations of the rainfall, the maximum and minimum of the one approximating in period to those of the other."

Of course the Commission, in the present unsettled state of this all-important question do not feel themselves justified in recommending any anticipatory measures to be taken in view of the probable recurrence of famine, on the basis of this theory. The subject, it is admitted, is scarcely advanced enough to warrant such recommendations. What they do recommend, however, demands the serious attention of the Indian Government. They state that the subject "is one deserving of careful investigation, and that it does not seem contrary to reasonable expectation that some relation should be established between the variations of the rainfall from year to year and those of the conditions of the sun's surface, on the heat derived from which, unquestionably, all terrestrial meteorological phenomena closely depend. For various reasons India is a country in which the investigation of this matter may be carried out with especial facilities, and for this reason (though other grounds are not wanting) we would urge that, as the expense of such researches would be small, the measures which have recently been taken by the Government of India to carry them out should be continued, and even extended in the future."

"As at present no power exists of foreseeing the atmospheric changes effective in producing the rainfall, or of determining beforehand its probable amount in any season, such as would admit of timely precautions being taken against impending drought, the necessity becomes the greater for watching with close attention the daily progress of each season as it passes, for ascertaining with accuracy and promptitude the actual quantity of rain in all parts of the country, and for forming the best and earliest judgment possible from the facts as they occur, whether the supply will be sufficient or otherwise. For the present at least, so far as the rainfall directly affects the subject under consideration, these are the only precautions that appear possible. Within the last few years a very satisfactory system of meteorological observations has been established all over British India, and in our opinion it is of primary importance that it shall be maintained in complete efficiency, and shall so far be strengthened and improved as to insure the early and punctual supply of information to the executive governments, and to the officials in all departments concerned with the agriculture of the country or the preparations required to meet famines, as to the actual progress of the periodical seasons of rain in all parts of the provinces for which those governments or officers are respectively responsible. So far as it may become possible, with the advance of knowledge, to form a forecast of the future, such aids should be made use of, though with due caution.

"We are also satisfied of the importance of the diffusion of more sound and accurate knowledge of the causes and mode of occurrence of the periodical rains, on which the well-being of India is so largely dependent, not only among the officers of the Government, but also among all classes of the community. Any measures which the Government may find possible with a view to the publication and diffusion of such knowledge cannot fail to be highly beneficial."

We shall look with interest for the further information on this subject, which is promised in the appendix to the Report. We need not add anything in support of the strong recommendation of the Commission. The Government would certainly not have appointed them at all unless it meant to take action upon their recommendations, and surely no line of inquiry is more promising, or could be fraught with more useful results. If the laws (for there can be no doubt that such exist) which regulate the periodicity of droughts can be clearly ascertained, it would reduce to the limit of simplicity the measure to be adopted either to prevent the occurrence of famines or to be prepared long beforehand to prevent their natural consequences.

Other recommendations of the Commission are quite in keeping with that to which we have just referred. They advocate the introduction of a more scientific method into administration and statistics, the institution of a separate agricultural department, and the need of improved agricultural, vital, and economical statistics.

Besides the map already mentioned, there are others showing the extent and comparative severity of the famines in various districts of India, from the beginning of the century downwards. Altogether the Commission have faced their task in a thoroughly business-like and scientific method; while they have sought information from every quarter likely to yield useful results, they have never lost sight of the object they had in view, and their Report is likely to be of permanent value. We shall look for the further record of their proceedings with the greatest interest.

GAMGEE'S "PHYSIOLOGICAL CHEMISTRY"

Physiological Chemistry of the Animal Body. By Arthur Gamgee, M.D., F.R.S., Brackenbury Professor of Physiology in the Owens College, Manchester. Vol. i. (London: Macmillan and Co., 1880.)

THE title of this book, since it seems to indicate that the work treats of a division or kind of chemistry, suggests the question whether it ought not to have been written (and reviewed) by a chemist rather than by a physiologist. And indeed there was a time when the view that the chemistry of living beings was a kind of chemistry distinct from the ordinary chemistry had some measure of support, and when consequently the phrase physiological chemistry had a very definite meaning. At the present time however all or nearly all are ready to admit that the chemical events which take place in living bodies are in reality of the same kind as and subject to the same chemical laws as those which take place in lifeless things; and hence physiological chemistry has come to mean the same thing as chemical physiology. The study of the chemical phenomena of animals and plants may be undertaken either by the chemist who understands physiology or by the physiologist who knows chemistry. The day must sooner or later come,—may its advent be more speedy than the present outlook promises!—when the chemist will be able, on the strength of his general knowledge, to foretell with sureness and precision the varied chemical events of the animal body; but hitherto and as yet, each chemical twist and turn of the vital machine has to be worried out by direct observation and experiment, so that physiological chemistry really means at present the physiological investigation of the chemical phenomena of living beings, and thus naturally falls into the hands of the physiologist.

For some years past there has been a great want of an adequate English treatise on the subject, a treatise which should deal with the matter much more fully and completely than could possibly be done in the text-books of physiology or chemistry. The preparation of such a treatise, however, is a task of great labour, and Prof. Gamgee assumed a heavy responsibility when he undertook to bring out the work, the first volume of which is now before us. But we believe that we may congratulate him and his readers on the accomplishment, so far, of his task.

The first instalment comprises, besides a preliminary chapter on proteids, an account of the chemistry of blood, pus, lymph, and of the elementary tissues, contractile, nervous, connective, and epithelial. About 200 pages are devoted to blood alone, and these not only contain a full description and discussion of the phenomena of coagulation, of the chemistry of the serum, and of the red corpuscles, both of their stroma and their hæmoglobin, but include a special chapter "on the changes which the blood undergoes in disease," and a section on the "characters presented by the blood of invertebrate animals." Prof. Gamgee's object has been apparently threefold, viz., (1) to give the chemical data as fully and as exactly as possible, with abundant references to original memoirs and other authorities; (2) to explain even in detail the methods by which the data are determined, and in this the reader will have at once his attention arrested and his progress assisted by the illustrations of apparatus, spectra, &c., the number and excellence of which form a very striking feature of the work, distinguishing it in a most marked manner from its predecessors; and (3) to point out and discuss the physiological bearings of the data expounded. Thus under the heading of "Oxy-hæmoglobin" will be found a description of the various methods of preparation of this substance (some eight special methods being given in detail in small print), followed by an account of its elementary composition, crystalline form, general reactions, and absorption-spectra. The physiological properties of hæmoglobin are in large measure postponed to the chapter on respiration; but the *technique* of spectroscopic examination is fully described, including the method of recording absorption-bands in wave-lengths; and hæmatin, with other derivatives and allies of hæmoglobin, as well as the action of carbonic oxide and other gases, are treated at length. The account of blood ends with a "description of certain methods of research not described in preceding sections," such, for instance, as the determination of the specific gravity of blood, the quantitative estimation of its various constituents, normal and abnormal, the extraction and measurement of the gases of blood, the measurement of the total quantity of blood in the body, &c., &c.

The other parts of the book are written in a similar fashion, and as far as we have at present, from the sections which we have subjected to a more detailed examination, been able to judge, the author has spared no pains to insure accuracy in his facts and statements, as he has certainly shown judgment in his selections, while his descriptions are remarkably clear and easy to understand.

The prominence given to methods, and the richness in illustrations, make the book one of great value to the student. There are books, some of them professedly written for students, which, though of much worth in other respects, are from the student's point of view practically useless: books of which the student's own judgment is that "he cannot find what he wants" in them. We venture to think that it will be the student's own fault if he cannot find what he wants in Prof. Gamgee's work; that is to say, if he wants what he ought to want. If he seeks in it a compendium which will give him just that amount of knowledge which may be required for an examination, so prepared as to be most easily absorbed and retained for the few weeks which precede his ordeal, he will very probably be disappointed. But if he desires to understand the chemistry of the animal body he will find it an admirable guide, and especially a most valuable book of reference. Throughout the whole of physiology, and at least no less in the chemical than in other parts, the value of the data and the trustworthiness of the conclusions founded on them depend very largely on the methods employed; and no student can form an intelligent judgment on the chemical phenomena of the body who has not understood and appreciated the methods by which the various investigations have been carried out. Hence we lay especial stress on this feature of the book before us as most important for the student.

Prof. Gamgee has gone largely into detail and even into controversy; and in this point too we think he is right. The outlines of physiological chemistry are already present in the various text-books of physiology; what was emphatically wanted was a history and discussion of details to give shape and fulness to the more meagre accounts found elsewhere. Doubtless many will say that the work contains a great deal more than can possibly be wanted by the student of medicine or even of physiology. We will not presume to answer the difficult question, How little physiology a medical student may know without his educational status being considered "mean"; but this we may say, that there is not a page in this work, the study of which will not prove profitable not only to the medical student, but even to the medical practitioner.

We trust that the author will as soon as possible be able to complete a work of which the first part will increase his already high reputation, and certainly must be regarded as a most noteworthy addition to English physiological literature. M. FOSTER

PEAT-MOSSES

The Sphagnaceæ or Peat-Mosses of Europe and North America. By R. Braithwaite, M.D., F.L.S. (London: David Bogue, 1880.)

THE peat-mosses are a peculiarly interesting group of cryptogamic plants, which has attracted the attention of even ordinary observers from a very early period. No group of plants is more clearly defined in structure, in family likeness, and by the localities in which they are found. The wanderer over our moorlands, the sportsman in pursuit of game, are as familiar as is the botanist with their dense green or ruddy-coloured tufts, now covering over some damp spot or filling up some bog hole with a vast mass of vigorous vegetation. Nor is there wanting to them an economic value, and that of too great an importance to be overlooked by even the most careless, for it is past generations of these bog-mosses which form the vast deposits of peat, for which as an article of fuel no substitute is in many parts of Europe attainable. The name sphagnus was first used, by writers like Theophrastus and Pliny, to indicate some of the spongy lichens, but was restricted to a genus of mosses by Dillenius more than a century and a half ago, "which were like none of the terrestrial mosses, but were produced always in bogs and marshes."

Dr. Braithwaite, in the volume before us, gives a most excellent sketch of the literature of the genus, tracing it from Dillenius, Linneus, Hedwig, to Müller, Wilson, Sullivant, Schimper, Lindberg, and others. For a long time Prof. Schimper's work was the best on the subject, and Dr. Braithwaite mentions it as very complete in its details of structure, both descriptive and pictorial, and as leaving hardly anything to be desired. Of works more especially relating to the development and minute anatomy of the group, allusion is made to the important memoirs of von Mohl, Carl Nägeli, Dozy, Hofmeister, Russow, Piré, and Rozé. He then proceeds in a second chapter to some general observations on collecting, preparing, and on the points to be observed in the determination of a species.

In a third chapter the vegetative system of the group is discussed. To our mind this chapter might well have been extended. The details given of the germination of the spores are too few, nor is the following chapter on the reproductive system free from the same defect; and as to the illustration of these two chapters, it will suffice to mention that it is confined to a single plate. As the charming plates illustrating the descriptive portion of the work are, we trust, likely to serve for more than one edition of it, we would suggest that, in the event of a second edition, some half-dozen supplementary plates might be given, on which would be represented the embryology of the group.

Between fifty and sixty species of Sphagnum are known, of which about one-third are tropical. They are most abundant in the north and south temperate zones, in the higher latitudes of which they often cover over a large expanse of surface. Dr. Braithwaite describes twenty species as found in Europe and North America, that is about one-third of all the known species. Of the others, seven species are described as from Brazil, seven from Central America, four from Guadaloupe, seven from Australia and New Zealand, four from the Eastern Archipelago, two of these, *S. sericeum*, C. Müll., and *S. Holleanum*, Dozy and Molk, known only in a barren state, but remarkable for having the stem leaves precisely like the branch leaves in form and structure, their hyaline cells being without fibres, but with a single apical pore. The only species from tropical Africa is *S. Africanum*, Duby.

Dr. Braithwaite points out that the range of variability in the species is in this group most extensive, so that in their determination one must rely on minute anatomical distinction for their essential characters, as in many cases size, colour, direction of leaves, habit, presence or absence of fibres in the hyaline cells of the stem leaves, will all alike fail. In the separation of the Sphagninæ as a subclass from the Bryinæ or frondose mosses, Dr. Braithwaite follows the earlier views of the illustrious Schimper. He groups the species described in nearly the same manner as Lindberg, adopting his three sections—Eusphagnum, Hemitheca, and Isocladus. The European species are all located in the first section. The descriptive details are very clearly given. The synonymic lists are evidently made out with great care, and the varieties which in many of the species are, as is well known, very marked, are not only described, but in several cases figured. The twenty-eight beautiful coloured plates illustrating the species and varieties are all from drawings by the author, and they contain complete anatomical details of the stem and leaf structures. The work is brought out in a style worthy of the subject, and we trust will find its way not only into the hands of the botanist, but, as it well deserves to do, into the possession of all who take an intelligent pleasure in studying our native mosses.

OUR BOOK SHELF

Vox Populi: a Sequel to the "Philosophy of Voice." By Charles Lunn. (London: W. Reeves, 1880.)

WE are told in the preface that "the present work is a reprint of articles that appeared in the *Orchestra*," and that "now it has been discovered Galen (A.D. 180), 'the father of physicians,' as he is called, advanced the same physical views as those for which I (Mr. Lunn) have contended, my controversial work is ended:—it is scarce worth while to re-write." Was it then worth while to re-print? In the introduction the author tells us that his articles were written "to clear up some ambiguous points in my (Mr. Lunn's) 'Philosophy of Voice,'" and that "this without the former work is incomplete, as that without this." Some time ago the present writer honestly endeavoured to understand Mr. Lunn's "Philosophy of Voice," and utterly failed in his attempts. He cannot find any assistance towards understanding it in the present little tract (pp. 88) of loose writing, wonderful reasoning, and jumping exposition. Let us hope that Mr. Lunn's teaching is better than his preaching. His axioms are however rather startling, especially the second (p. 7), "All voices are naturally beautiful. All ugliness in vocal tone is the result of transferred habits acquired by the artificial use of voice in speech." If this use is "artificial," what use is "natural"? But attempts to understand and criticism are all thrown away. Notwithstanding Mr. Lunn's initial confession that he is a mere follower of Galen, he declares in his introduction (p. 1): "It is a *fait accompli*. *I* have founded a New Profession standing midway between the Musical and the Medical worlds, with Art on its one side, Science on the other; firm and irrefutable." In this state of suspension, like Mahomet's coffin, "midway between" two "worlds," and belonging to neither Science nor Art, which seems fitly to describe the nature of the book, we are content to leave it to the happy conviction of the author that what he says (of course when others can find out what it is) is "firm and irrefutable."

Practical Plane Geometry and Projection for Science Classes, Schools, and Colleges. By Henry Angel. Vol. I., Text; Vol. II., Plates. Collins's Advanced Science Series. (London and Glasgow, 1880.)

A VERY practical and useful book by an experienced teacher: it is designed to meet the requirements of students at the Royal School of Mines, at the Royal Military Academy, at Cooper's Hill, and elsewhere, and embraces great part of the two higher stages of the Science and Art Department syllabus. There is no great scope for absolute novelty in such a work, and our author acknowledges his indebtedness to the works of many, if not most, of his well-known predecessors, but the arrangement appears to be judicious, and the constructions good and clearly enunciated. In the Practical Geometry (six chapters) the student is taught the use and construction of scales, of triangles and polygons, and there are numerous problems on areas, on circles in contact, and on other plane curves with their tangents and normals. The orthographical portion treats of the projection of the five regular solids, of other simple solids, of flat and curved surfaces, intersected by cutting planes, and of solids inscribed in, or circumscribed to, the surfaces of other solids; of the interpenetration of solids, of the projection of shadows, on isometric projection, on the solution of the spherical triangle, and on horizontal projection—a very extensive and varied bill of fare. In addition there are numerous questions for practice, many of which are taken from examination papers, and the text is illustrated by several clearly-drawn figures. Part ii. contains eighty-one large-page plates to further illustrate the constructions. The two parts together ought to enable any painstaking student to take a creditable place in his examination and to acquire a solid acquaintance with the subject.

Teorica delle Forze Newtoniane e sui applicazioni all' Elettrostattica e al Magnetismo del Prof. Enrico Betti. 365 pp. (Pisa, 1879.)

IN the session 1863-64 Prof. Betti delivered at Pisa a course of lectures, subsequently (1865) printed in the *Nuovo Cimento* under the title "La Teorica delle Forze che agiscono secondo la legge di Newton e sua applicazione alla elettricità statica"; the volume before us is what may be looked upon as its greatly enlarged second edition. It consists of an introduction and three chapters. The first chapter, in twenty-three sections, treats of Potential Functions and of Potentials (§ 11 gives Green's theorem and some others due to Gauss; § 12 Stokes's theorem for transforming a double integral into a simple integral, and the properties of a surface which has on one face a stratum of attracting, and on the opposite face an equal stratum of repulsive, matter; the other sections appear to contain nearly all the known properties of these functions). Chapter II., on Electrostatics, in sixteen sections, discusses several cases of electrostatical distribution, the method of images (Sir W. Thomson's theory) and condensers; Chapter III., on Magnetism, is divided into ten sections (on p. 304 Prof. Betti announces the theorem, "Se la superficie di un corpo è semplicemente connessa ed ha un numero finito di poli, questo numero sarà sempre pari," an advance upon Gauss, who has shown that if there be three poles there must also be a fourth).

Kalkül der Abzählenden Geometrie. Von Dr. Hermann Schubert. (Leipzig: Teubner, 1879.)

DR. SCHUBERT in this work gives us, in the form of a treatise of 359 pages, the principal results as yet arrived at in the "Numerical Geometry," a branch of mathematics originated by M. Chasles and subsequently studied by Zeuthen, Sturm, Halphen, Klein, and in this country by Dr. Hirst ("On the Correlation of Two Planes," vol. v.; "Correlation in Space," vol. vi.; "Note on the Correlation of Two Planes," vol. viii.; London Math. Soc. *Proceedings*). The book closes with a full historical and bibliographical list in the form of notes to the several chapters.

LETTERS TO THE EDITOR

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

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

The Spectrum of Hartwig's Comet

THE spectrum of this comet was examined here on the evening of October 7 with a spectroscope having a single prism of 45°, and was found to consist of three bright bands and a continuous spectrum corresponding to the nucleus. The middle and brightest band was compared with the band at W.L. 5198 in the spectrum of a vacuum tube containing alcohol vapour, and three micrometer measures gave the position of the less refrangible edge of the comet band at W.L. 5184, 5215, and 5204 tenth metres respectively. The breadth of the band was about 40 tenth metres. These measures would indicate that the principal comet-band is coincident with the band at W.L. 5198 of the vacuum-tube spectrum of carbon-compounds, and not with that of the Bunsen-flame at W.L. 5165. The observations however were made under unfavourable circumstances, the comet being low, and involved in haze and cloud. The positions of the other two bands were not determined.

W. H. M. CHRISTIE

Royal Observatory, Greenwich, October 11

Wire Torsion

I HOPE you will allow me to seek information, through your aid, on a subject which is perplexing me a good deal at present. I am engaged in studying a gravimeter designed by the late J. Allan Broun, in which gravity is balanced by the torsion of a single wire; or is intended to be so. As the function of the instrument depends largely on the law of torsion in wires, I have been making experiments to satisfy myself on some points. It is in the results of one of these that I have met with my difficulty. I was using thin brass wire (diam ·02), and after stretching it till it broke, twice, I supposed it to be at or near its maximum elasticity, and proceeded to use it in the intended way. At each end of a 6-foot plank I inserted into the edge a 2-inch screw. The wire was fastened upon these so as to get a strain by turning them. The wire was in two pieces, attached to opposite sides of a ring in the middle. By turning this ring the two wires were severally twisted in opposite directions. A straight thick wire passed through the ring, the weight of which afforded a ready means of varying the force necessary to balance the torsion of the wire. My first object was to prove that the force of the latter was, at any rate to some considerable extent, independent of the tension. Suppose that with this arrangement, the wire being horizontal, a balance has been effected when the ring has been turned about the wire as an axis three or four times. What will happen when the wire is further strained? I think it would be a natural expectation (apart from special knowledge) that the weight will rise; on the other hand, a knowledge of the law of torsion teaches (?) that there is no increase of the force sustaining the weight, which therefore will *not* rise. But who would suppose that, on the contrary, it would sink? Such, nevertheless, is what takes place. I continued increasing the strain, and the weight continued to sink. I had to go on lessening the weight again and again (by shifting the balancing cross-rod), in order to restore the horizontality of the ring; until at last there was scarcely any force of torsion left! To repeat the experiment of course the ring had to receive three or four fresh turns. I did so several times, always continuing, as I thought, to increase the strain. All the time the wire was absorbing the torsion, and did not break. I then thought to try the effect of a high initial torsion. But I did not seem to get any such by turning the ring more than five or six times. I then thought to see how much twisting the wire would bear. Expecting it every instant to break, I counted up to 100 half turns. *By this time the wire was quite slack!* I added *another hundred* half turns. The wire was now half an inch longer, without any strain having been kept on it except just enough to keep it straight. I went on twisting. At 218 one wire broke. The other then had only sixteen half-turns of twist in it, out of the 230 or more received. I afterwards went on twisting, mending each time that the wire broke, till the twist (quite visible under the microscope) amounted to sixteen turns per inch. The length kept on increasing. After breaking, the wire always untwisted one turn in four inches.

I feel myself here in presence of laws of which I know *nothing;* and my object in writing this short experience is to ascertain whether it is sufficiently in accord with what *is* known to cause no surprise to any one but myself. In that case I shall be greatly obliged to any one who will tell me where I can learn all about it. J. HERSCHEL

Collingwood, October 4

I forgot to say that in no case did slackening of the strain reverse the sinking of the weight due to increase of strain.

The Magnetic Storm

BY the mail just arrived from Australia I have received copies of the photographic traces produced by the declination magnetograph at the Melbourne Observatory during the magnetic storm of August 12 to 14, kindly forwarded by Mr. Ellery, the Government astronomer there.

A comparison of these curves with those from the Kew instrument for the same period shows that the disturbance commenced and ended at both places at the same time.

It is not easy however to trace much similarity in the two sets of curves, as the individual excursions of the magnet east and west of the normal position which form the record of the magnetic storm, cannot be at all times followed in both curves, but the periods of greater disturbance seem to have been simultaneous. For example, the commencement of the disturbance was well marked at August 11d. 8h. 10m. p.m. at Melbourne, which corresponds to 11d. 10h. 33m. a.m. G.M.T., whilst here (*vide* Mr. Ellis's letter in NATURE, vol. xxii. p. 361) it commenced at 10h. 30m. a.m.; then again the large deviation to the eastward noted in the Rev. S. J. Perry's letter in NATURE, which occurred here between 12d. 11h. 30m. a.m. and 12h. 30m. p.m., seems to have had its effect, as a movement of the needle at Melbourne to the westward between 12d. 9h. 15m. p.m. and 10h. 30m. p.m. The maximum deflection which exceeded the limits of registration of the instrument, I estimate to have taken place at 10 p.m. The corresponding G.M. times for the above are 12d. 11h. 38m. a.m., 12h. 53m. p.m., and 12h. 23m. p.m.; the maximum deflection recorded here seems to have been at 12h. 25m. p.m.

The disturbed period may be considered to have died out at Kew at 14d. 8h. a.m. G.M.T., and at Melbourne at about 14d. 7h. a.m., but there is no very distinctive movement which would enable us to fix this limit with accuracy.

These interesting comparisons are extremely satisfactory, for it is but recently that the Government of Victoria was considering the advisability of discontinuing the system of photographic registration of the magnetometers at Melbourne, and consulted the Kew Committee upon the subject.

A circular was accordingly issued to the leading physicists of Europe, and their replies being almost unanimously in favour of the continuance of the recording system, the Government erected a new magnetic observatory, and decided upon carrying on the work.

Mr. Ellery has also forwarded a month's curves for the purpose of assisting in the international comparison of magnetograms now being prosecuted by the Kew Committee.

The preliminary results of their investigations have been already indicated by Prof. Adams in his recent speech at Swansea (NATURE, vol. xxii. p. 416). G. M. WHIPPLE

Kew Observatory, October 2

Coral Reefs and Islands

I HAVE been greatly interested in Mr. John Murray's paper on coral reefs and islands published in NATURE, vol. xxii. p. 351. I hope you will allow me space to draw scientific attention to the fact that as early as 1857 I published a paper on the Formation of the Peninsula and Keys of Florida (*Am. Jour.* vol. xxiii. p. 46), in which I maintain that the theory of Darwin, although so beautifully (as I thought) explaining the phenomena of the Pacific reefs, *wholly fails to explain those of the Florida coast.*

In 1851 I spent the months of January and February on the Keys of Florida, assisting Prof. Louis Agassiz in his investigations on the growth of reefs and formation of keys in this region. An abstract of these investigations and their results was published in the Report of the United States Coast Survey for 1851, p. 145 *et seq.*[1]

In this report Agassiz shows that the Keys and nearly the whole Peninsula of Florida have been formed by the growth of successive reefs, one beyond the other from north toward the south. In my paper above alluded to, and also in my "Elements of Geology," p. 152, I state further, that the reefs of Florida, if we accept Darwin's theory, are entirely peculiar. For according to Darwin barrier-reefs are formed *only by subsidence*, while on the Florida coast we have well-marked barriers with channels 10–40 metres wide where there cannot be any subsidence, for continuous increase of land is inconsistent with subsidence. Again, according to Darwin barriers and atolls always show a *loss of land*, only a small portion of which is recovered by coral and wave agency; while on the Florida coast, on the contrary, there has been a continuous growth of the Peninsula by coral accretion, until a very large area, viz., about 20,000 square miles, has been added.

I have attributed the formation of *successive* reefs from north toward the south to the successive formation of the depth-condition necessary for coral growth; and this latter, in the absence of any evidence of elevation, to the steady building up by sedimentary deposit, and extension southward, of a submarine bank within the deep curve of the Gulf Stream. The formation of barriers instead of fringes on a coast which has certainly not subsided—for continuous land-growth negatives the idea of subsidence—I attribute to the shallowness and muddiness of the bottom along this coast. Only at a distance of twenty to forty miles, where the depth of twenty fathoms is reached, and where, therefore, the bottom is no longer chafed by the waves, the conditions necessary for coral growth would be found, and here a line of reefs would be formed, limited on one side by the depth and on the other by the muddiness of the water.

In brief then, according to my view, the Peninsula and Keys of Florida were formed by the co-operation of several agents:— 1. The Gulf Stream building up and extending a submarine bank within its loop. 2. Corals building successive barriers on the bank as the latter was pushed farther and farther southward. 3. Waves beating the reefs into lines of islands. 4. *Débris* from the reefs and keys on the one side and the already formed mainland on the other filling up the successive channels and converting them first into swamps and finally into dry land.

Whether this view is true in all its parts or not, there can be

[1] This report has been recently published in full as one of the memoirs of the Harvard Museum of Comparative Anatomy, but I have not yet seen it.

no doubt that the southern coast of Florida affords exceptional advantages for the successful study of the formation of coral reefs. JOSEPH LECONTE

Berkeley, California, September 18

Geological Climates

THE dilemma into which Dr. Houghton thrusts the rigid uniformitarian school is one which was enlarged upon some years since, when reef-building corals were asserted, upon the evidence afforded by fossils, to have existed during the Miocene and Oligocene ages in seas where Tasmania now exists in the south and Hampshire in the north. There are no instances of large masses of reef-building corals in corresponding latitudes at the present day, and the range of these surface-living, high-temperature-requiring zoophytes is well known.

Uniformitarians may take comfort, however, and slip under the horns which Dr. Houghton so ably presents for their transfixment. Where I now write, on the Bagshot sands and gravels of Cooper's Hill, facing the cold north with a touch of the east, there is a patch of bamboo canes in full leaf. They were in full leaf at this time last year. The plant survived out of doors the extreme frost and fogs of last winter and other evidences of a temperate climate, and it has been in beautiful leaf all this summer.

Now everybody knows that in torrid India the bamboo grows. Therefore if the palæontologist of the year A.D. 18800 should dig up the Cooper's Hill stalks and leaves, and should have the opportunity of examining in some future Kew the bamboos of the hot parts of the earth, he would logically, geologically, palæontologically, but somehow unreasonably, come to the conclusion that Cooper's Hill and India enjoyed corresponding and intensely tropical climates in 1880, during the geological age when the earth's polar axis was certainly inclined nearly $23\frac{1}{2}°$ to the plane of the ecliptic. P. MARTIN DUNCAN

Royal Engineering College, Cooper's Hill, Staines, October 9

The Yang-tse, the Yellow River, and the Pei-ho

I HAVE been much interested in the paper on the above rivers, published in NATURE, vol. xxii. p. 486. To the extent of the writer's personal observations the calculations appear to have been careful and accurate, and as near the truth as the observations of a single year are likely to be. A reference to Sir Charles Hartley's observations of the Danube, extending over *ten* years, shows that the mean maximum discharge of that river for one year exceeded the minimum by 3 to 1.

It is however to the use of *one* observation of the Yellow River made in 1792 by Sir Geo. Staunton that I feel compelled to enter a protest, firstly, because *one* observation is misleading in drawing general inferences, and, secondly, is especially to be suspected when it is at variance with other well-authenticated examples.

According to the writer of the paper, the mean discharge of the Yang-tse is 770,397 cubic feet per second, carrying to the sea 6,428,800,000 cubic feet of sediment per year, but the Yellow River having only a mean discharge of 116,000 cubic feet per second delivers, according to Sir George Staunton, 17,520,000,000 cubic feet of sediment per year into the Gulf of Pe-Chili. With Dominie Sampson we may well exclaim "prodigious!" It has struck me as an explanation of this anomaly that Sir George Staunton probably measured the deposit from "the gallon and three-quarters" of the Yellow River water as *wet mud.*

If so this will at once account for the excessive amount of it. The deposit of Nile mud in the reservoirs of the Cairo waterworks often amounts to 1 inch in 10 feet of water,[1] or $\frac{1}{120}$ part of the bulk. Dr. Letheby's analyses show that in August the proportion by weight of sediment (dried) being the maximum of the year, in Nile water is $\frac{1}{670}$[2]: thus taking the specific gravity of the dry mud at 1·9, the measurement of the wet deposit by bulk exceeds the dry about $10\frac{1}{2}$ times.

If the 80 grains to the pint of the Yellow River water be divided by $10\frac{1}{2}$, we arrive at between 7 and 8 grains per pint of dry sediment, corresponding closely with the proportion given by the writer for the Pei-ho and Yang-tse.

I would also point out that the discharge of the River Plate as given in the table is not the *mean*, which has not yet been ascertained, but the *dry veather flow.*[1] Still another little error, for which the writer is in no way responsible, being a quotation from Huxley's "Physiography." The discharge of sediment by the Thames is a calculation by Prof. Geikie on an *hypothesis*, not on observation; and instead of 1,865,000 should be 18,650,000—this printer's error has been copied from Geikie's original paper by writer after writer without discovery.

I should feel obliged if the writer would explain why the surface-current of the Yang-tse and Pei-ho should vary so in velocity with the same average depth of water. It seems anomalous. T. MELLARD READE

Blundellsands, Liverpool

[1] "Mediterranean Deltas." *Edin. Review*, January, 1877.

[2] "Egyptian Irrigation." Second Report, January, 1876. By John Fowler, engineer to the Khedive.

[1] Report by James Bateman, C.E.

Miller's Elements of Chemistry—Part III. Organic Chemistry

IN his notice of the new edition of this work, by Mr. Groves and myself, which appears in NATURE, vol. xxii. p. 530, Mr. Muir refers to an obvious omission at p. 933. May I request those who possess the book to insert at the top of the page the words "Probably, however, the most weighty objection that can be raised to the" . . . Although in the revise, by some strange mischance this line has been dropped in printing off.

HENRY E. ARMSTRONG

Swiss Châlets

IDENTICAL suggestions to those of Mr. George Henslow with regard to the connection in descent of modern Swiss châlets with ancient pile lake-dwellings will be found expressed in Dr. J. J. Wild's "At Anchor" (Marcus Ward and Co.), p. 106, and with some detail in my "Notes by a Naturalist on the *Challenger*" (Macmillan and Co.), p. 399. Dr. Wild, who is a native of Switzerland, and I arrived at the same conclusions independently, as we only found out on reading one another's books, from the study of the modern pile dwellings of the Malay Archipelago during the voyage of the *Challenger*, and we both amongst other conclusions identified the balcony of the châlet with the ancient platform, as does Mr. Henslow. H. N. MOSELEY

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

Spectre of the Brocken at Home

HAVING occasion ten days ago to go into my garden about half past ten o'clock at night I found there was a thick white fog, through which, however, a star could be seen here and there. I had an ordinary bedroom candlestick in my hand with the candle lighted, in order to find the object I wanted. To my great surprise I found that the lighted candle projected a fantastic image of myself on the fog, the shadow being about twelve feet high, and of an oddly distorted character, just as the spectre of the Brocken is said to be. It is of course usual on going into the open air to use a lantern with a solid back for any light that may be wanted, and with this, of course, such a shadow would not be seen; but in this charmingly foggy valley of the Thames, and in these days of "Physics without Apparatus," the effect I saw can probably be seen only too often. May not the gigantic spirits of the Ossianic heroes, whose form is composed of mist, through which the stars can be seen, be derived from the fantastic images thrown upon the mountain fogs from the camp fires of the ancient Gaels? In a land where mists abound a superstitious people might very readily come to consider a mocking cloud-spectre to be supernatural, though it was really their own image magnified. If it be true that in our earlier stages of development we resemble more nearly the past forms of life and thought, I may mention in this connection that, thinking to amuse a little child of three, I threw a magnified shadow of her on the wall with a candle, and then, by moving it in the usual way, made the figure suddenly small. Instead of the changing shadow giving the pleasure intended, the child was terrified, as the warriors of Morven may have been when they saw their shadows on the clouds. J. INNES ROGERS

Putney, October 8

Ice under Pressure

THERE is a point in Dr. Carnelley's letter (NATURE, vol. xxii. p. 435) which I have been hoping to see cleared up by subsequent letters. He says, "In order to convert a solid into a liquid the *pressure* must be *above* a certain point," and goes on to describe some experiments with ice, implying that ice is in this respect a typical substance. Now our text-books speak of the behaviour of water in freezing and melting as exceptional. For instance, Prof. Balfour Stewart says ("Heat," p. 89): "If a substance expands in congelation, its melting-point is lowered by pressure, but if a substance contracts in congelation, its melting-point is raised by pressure." And (p. 91): "Bunsen found that the melting-points of paraffin and spermaceti, both of which contract when freezing, were raised by the application of pressure."

Do the new results tend to overthrow the generally received opinions on the subject? or is there some way of reconciling these seeming contradictions?

I have more interest in these matters than knowledge of them, and must apologise if I am asking a question which I ought to have been able to answer. C. A. M.

October 2

Mr. Haddon's Marine-Zoology Class

OWING to misconceptions which have arisen from the notice in NATURE, vol. xxii. p. 517, relative to my marine-zoology class, I should like to state that this class was formed solely for the purpose of the practical study of marine zoology, and without any idea of founding a zoological station. I would also like to take this opportunity of acknowledging my great indebtedness to Prof. Dohrn's magnificent institution at Naples.

Zoological Museum, Cambridge ALFRED C. HADDON

Landslips

I READ with great interest the article on landslips in NATURE, vol. xxii. p. 505. It is no doubt familiar to many that the salt districts of Cheshire, in the neighbourhoods of Northwich and Winsford, are subject to landslips of a peculiar kind. The beds of rock salt occupying the position of the Triassic salt lakes are the centre of an extensive underground drainage. The fresh water on reaching the salt proceeds to dissolve it and becomes brine. This brine is pumped up and manufactured into white salt. As the fresh water keeps constantly dissolving and eating away the solid salt, the superincumbent earths keep sinking, and on the surface deep furrows, like the dried beds of rivers, mark the course of the underground waters. At times enormous masses of earth sink bodily, leaving cavities of a funnel shape. A short time since a mass of at least 60,000 tons of earth suddenly disappeared. When these subsidences are near rivers they become filled with water, and large lakes over 100 acres in extent have been formed. Although houses are not overwhelmed they are very frequently destroyed, and this destruction of property is so serious that the sufferers are now about to appeal to Parliament for assistance.

The district of the salt manufacture presents phenomena both curious and interesting, and is well worth visiting. A fortnight ago the whole of the water in one of these subsidences of over five acres in extent disappeared, leaving a chasm or abyss in many places forty or fifty feet deep. The action of water on soluble rocks can be seen here in great perfection.

Brookfield House, Northwich THOS. WARD

LIQUEFACTION OF OZONE

AT a recent meeting of the French Academy, MM. Hautefeuille and Chappuis announced that they had liquefied ozone. These chemists have been able to ozonise oxygen to a greater extent than has hitherto been done, by passing the silent discharge through the oxygen at a low temperature. The tube containing oxygen was immersed in liquid methylic chloride, which boils at -23°. After being submitted to the electric discharge for fifteen minutes at this temperature, the oxygen was conducted into the capillary tube of a Cailletet's apparatus, the temperature of which was maintained at -23°.

After a few strokes of the pump the gas in the tube appeared azure blue; as pressure increased the depth of colour likewise increased, until under a pressure of several atmospheres the ozonised oxygen appeared dark indigo blue. The pressure was increased to ninety-five atmospheres, and was then suddenly removed, whereupon a mist, indicating liquefaction, appeared in the capillary tube.

The stability of a mixture of oxygen and ozone rich in ozone appears to be chiefly dependent on the temperature. If such a mixture be rapidly compressed at ordinary temperatures, a considerable amount of heat is evolved and the gas explodes.

Ozone, say MM. Hautefeuille and Chappuis, is therefore to be placed in the category of explosive gases.

Berthelot has shown that the transformation of oxygen into ozone is attended with absorption of heat: the stability of products of endothermic reactions is as a rule increased by decreasing temperature.

Ozone is much more easily liquefied than oxygen; the latter must be compressed under 300 atmospheres at about the temperature of -29° before sudden removal of pressure succeeds in producing liquefaction.

We have thus the existence through a large range of temperature and pressure of two allotropic forms of the same element; each with distinctly marked chemical and physical properties. We know that the molecule of oxygen has a simpler structure than that of ozone; the substance of simpler molecular structure is capable of existing through a much more extended range of temperature and pressure than that of more complex structure. Under special physical conditions it seems possible that new allotropic modifications of various elements might be produced.

The marked differences in colour, and in temperature of liquefaction, between oxygen and ozone, furnish another illustration of the close connection which exists between the "chemical structure" and physical properties of substances; a different "linking," even of similar atoms, being evidently associated with distinctly different physical properties.

MM. Hautefeuille and Chappuis will doubtless soon be able to furnish more details of the properties of this most interesting substance, liquid ozone. M. M. P. M.

THE UNIVERSITY OF NEW ZEALAND

THE University of New Zealand, with which, since 1874, the University of Otago has been affiliated, has, we are glad to find, adopted a quite modern schedule of subjects for its degree of B.A.

The subjects of examination for the B.A. degree are:—1. Greek Language and Literature. 2. Latin Language and Literature. 3. English Language and Literature. 4. Modern Languages and Literature. 5. General History and Political Economy. 6. Jurisprudence and Constitutional History. 7. Mathematics. 8. Physical Science, any two of the following branches: (*a*) Sound and Light, (*b*) Heat and Radiant Heat, (*c*) Electricity and Magnetism, (*d*) Astronomy and Meteorology. 9. Chemistry. 10. Natural Science, any one of the following branches: (*a*) Geology and Mineralogy, (*b*) Zoology, (*c*) Anatomy and Physiology, (*d*) Botany. 11. Mental Science. No candidate shall be approved by the examiners unless he show a competent knowledge of at least five of the above subjects of examination, of which two must be Latin and Mathematics. The examination may be passed in two sections. Either two or three subjects of examination, one of which must be either Latin or mathematics, shall constitute the first section, which may be taken at the end of the second or any subsequent year, and the remaining subjects shall constitute the second section, which may be taken at the end of the third or any subsequent year; or, at the option of the candidate, all the subjects may be taken together at the end of the third or any subsequent year.

In this curriculum the physical and natural sciences seem to have a fair share allotted to them, and the same is also the case in the courses for the senior scholarships and honours—which latter cannot be competed for until the end of one year after the candidate takes his B.A. degree. A Bachelor of Arts obtaining honours can have his M.A. degree without special examination; all others have to pass an M.A. examination.

It is probable that for some time to come there will be great differences of opinion as to how the natural sciences should be taught and examined in in our universities. Some incline to limit the courses in botany and zoology, and to require a good sound knowledge of the prescribed work; others imagine that the effect of limiting a course is to produce a specialist, which, they argue, is to spoil a student; but the mean appears to us to be not so hard to find. A sound general knowledge of development and of physiology might certainly be demanded of all students, and the field of biology being too large for any human being to work over, the student might, as to details of structure, &c., be limited to the study of some defined class. It is in this direction evidently that Prof. Hutton has framed the schedule of zoology and botany, a schedule which, while we acknowledge it to be excellent from a general point of view, is, we are firmly persuaded, longer and more profound than is expedient in a new country, where the teaching power is not great. We are fully aware that there is a tendency in classical and mathematical teachers to believe that the study of natural science is something quite easy; but those able to judge have long agreed that not only does this study call for all the best talents, but that the student too often approaches it long after the impressionable period of his life: a little Latin, perhaps less Greek, a schoolboy knows; arithmetic, algebra, and geometry he is fairly familiar with; but the natural sciences and the how he lives, moves, and has his being, of these he is fain to exclaim, But who are ye? The professors of natural science must bide their time; it is no doubt coming, for biology is now somewhat taught in our schools, and may be will be taught on the mother's knee; but in the meanwhile let them not exact too much from candidates for B.A. degrees or honours; let them progress surely, even though they be accused of progressing slowly. As to the New Zealand University, we shall follow its progress with pleasure, and trust it may soon fulfil the great expectations that we have of it.

DOCTORED WINES

THE French Government have just passed a most salutary measure, which will have for effect the diminution, if not the complete suppression, of the process known as *plâtrage*, now become an almost constant custom through most of the wine districts of France, and which, from having at first been performed on a very moderate scale, has lately enormously increased, till it has developed into a crying abuse. The *plâtrage* is carried on during the fermentation, and consists in merely sprinkling the grapes, as successive baskets of them are emptied into the fermentation vats, with plaster of Paris—calcium sulphate—(French *plâtre*), mineralogically known as gypsum, or selenite, in fine powder. Now the grape-juice contains several salts of potash, among which the most abundant are the tartrate and bi-tartrate, and these decompose when placed in contact with the calcium sulphate, forming calcium tartrate—an insoluble salt—and potassium sulphate.

In the case of potassium bi-tartrate, potassium bi-sulphate is formed. Now besides the salts of potash above named, the juice of the grape contains grape-sugar, a nitrogenous fermenting principle and an astringent principle—to which latter new red wines owe much of their harshness—and also a red colouring-matter, with which the astringent principle is intimately associated. The fermentation splits up the grape-sugar, as it is well known, into carbonic acid, which escapes with effervescence, and alcohol, which remains dissolved. In pure undoctored wines, in proportion to the development of alcoholic strength, and as the wine by age tends to become more acid, potassium bi-tartrate separates as a crystalline precipitate, forming the chief constituent of the deposit in the casks known as *lees*, or, when it forms in bottled wines, as the *crust*.

Now the astringent principle which in the red grape is, as we have explained, intimately combined with the colouring-matter, seems to be held more or less in solution by the tartrates, and as these subside with age the wine grows less harsh, losing at the same time much of its colour, and is said to ripen or grow mellow. As the astringent principle however disappears, the wine, if it be one of the weaker French wines, tends to run to the acetous fermentation, and this is why we frequently find a wine become sour and unpalatable shortly after it has mellowed with age and arrived at its maximum of perfection. Many a bin of valuable claret or Burgundy has thus suddenly surprised and disappointed its possessor, changing in the short space of a few months from fine mellow wine to undrinkable vinegar.

Now, as stated above, calcium sulphate (*plâtre*) decomposes the potassium tartrates, and by withdrawing them and substituting the potassium sulphate, tends to prevent much of the colouring and astringent matter from passing into solution, so that this so-called *plâtrage* is nothing more than a means employed by the Bordelais and Burgundians for giving to their wines a fictitious effect of age, and they naturally defend a practice which enables them to bring their wines sooner into the market, economising their outlay in casks, and diminishing the chances of loss entailed by keeping a large stock of wine on hand. Further the process lends itself to fraud, permitting the wine merchants of Bordeaux and Burgundy to import the strong harsh wines of the north of Spain and the south-east of France, which, when blended with the small, poorer wines of the hill-districts of their own country, and then being *plâtrés* (that is agitated with powdered calcium sulphate), become mild and palatable. Thousands of hogsheads of wines thus blended and doctored are annually sold, and too often at the high rates commanded by pure vintage wines. Under the provisions of the new act no wine is allowed to be brought into commerce if it contains over two grammes of potassium sulphate per litre. Even this proportion is too large, *plâtrage* should be entirely prohibited; but when we consider that wines are now often sold with five or six grammes of this salt to the litre, it was time indeed that some measures should be taken. The merchants defend themselves on the basis of the practice being innocuous, and that while it promotes the keeping qualities of the wine, even four grammes of potassium sulphate could do no harm. It is the greatest possible mistake to fancy that *plâtrage* makes wine keep; for, no the contrary, it withdraws from it the astringent principle, a most potent means of its preservation. For a Bordeaux merchant to contend that forty grains of potassium sulphate to the pint of wine is not or cannot be unwholesome, is a thesis which may be agreeable to his pocket, but certainly ought to be discouraged, for, to say the least, it would surely be prejudicial to the stomachs of delicate or dyspeptic consumers. Not very many years since a case occurred of actual death by poisoning from the administration of a comparatively small dose of potassium sulphate, and this salt is well known in medicine as a drastic and dangerous purgative. We should then be most sincerely grateful to the French Minister of Commerce for the prudent forethought with which he has protected the consumers of French wines from a practice which had grown into a crying abuse, and for giving us one more guarantee for the purity of these wines, justly ranked as the most esteemed that the world produces.

MULTIPLE SPECTRA [1]

III.

I HAVE endeavoured to show in the previous articles that there are many facts which justify the conclusion that the same elementary substance in a state of purity can under different conditions give us spectra different in kind. To those spectra to which special reference is now made the names of *lined* and *fluted* have been given to mark their chief point of difference, which is that in lined spectra we deal with lines distributed irregularly over the spectrum; while in fluted spectra we deal with rythmical systems.

This was the first point, and I showed that the idea was suggested that the lined and fluted spectra, though produced by the same substance, were produced by that substance in a different molecular condition.

I have pointed out that both in lined and fluted spectra taken separately there was evidence of still further complication, that is, that a complete lined spectrum of a substance and a complete fluted spectrum of a substance, was the result of the vibration not of one kind of molecule only, but probably of several.

So that in this view we have to imagine a series, in some cases a long series, of molecular simplifications brought about by the action of heat, and ascribe the spectral changes to these simplifications.

To understand my contention, and one objection which has been taken to it, in the clearest way, let us suppose that there is a substance which gives us, under different conditions, three spectra, which we will term *a*, *b*, and *c*. My view is that these spectra are produced by three distinct molecular groupings brought about by successive dissociations. On the other hand, it is objected that they are produced by *one and the same molecule* struck, as a bell might be struck, *in different ways* by the heat waves or the electric current passing among the molecules.

In my memoir entitled "Discussion of the Working Hypothesis that the so-called Elements are Compound Bodies," I remarked as follows :—

"I was careful at the very commencement of this paper to point out that the conclusions I have advanced are based upon the analogies furnished by those bodies which, by common consent and beyond cavil and discussion, are compound bodies. Indeed, had I not been careful to urge this point, the remark might have been made that the various changes in the spectra to which I shall draw attention are not the results of successive dissociations, but are effects due to putting the same mass into different kinds of vibration or of producing the vibration in different ways. Thus the many high notes, both true and false, which can be produced out of a bell with or without its fundamental one, might have been put forward as analogous with those spectral lines which are produced at different degrees of temperature with or without the line, due to each substance when vibrating visibly with the lowest temperature. To this argument, however, if it it were brought forward, the reply would be that it proves too much. If it demonstrates that the *h* hydrogen line in the sun is produced by the same molecular grouping of hydrogen as that which gives us two green lines only when the weakest possible spark is taken in hydrogen inclosed in a large glass globe, it also proves that calcium is identical with its salts. For we can get the spectrum of any of the salts alone without its common base, calcium, as we can get the green lines of hydrogen without the red one.

"I submit, therefore, that the argument founded on the over-notes of a sounding body, such as a bell, cannot be urged by any one who believes in the existence of any compound bodies at all, because there is no spectroscopic break between acknowledged compounds and the supposed elementary bodies. The spectroscopic differences between calcium itself at different temperatures is, as I shall show, as great as when we pass from known compounds of calcium to calcium itself. There is a perfect continuity of phenomena from one end of the scale of temperature to the other."

Not only is what may be termed the bell hypothesis opposed to the law of continuity, as I endeavoured to show in the last paragraphs quoted, but it appears never to have struck the objectors that it is also opposed to the theory of exchanges as it is generally enunciated, on which the whole of our supposed knowledge of extra-terrestrial matter depends. If vapours, when relatively cool, do not absorb the same wave-lengths which they give out when relatively hot, what becomes of some of the most noted exploits of our nineteenth-century science?

Take the case of sodium. Three distinct spectra have been mapped for it. There is first the yellow line seen in a Bunsen flame, then the green line seen alone in a vacuum tube when the vapour is illuminated by an electric glow, and again there is the fluted absorption spectrum, without any lines, seen when sodium is gently heated in hydrogen in a glass tube. If we have here the same molecule agitated in different ways, I ask which is the true spectrum of sodium? And what right have we to say that sodium exists in the sun because the yellow line is represented? Why do we not rather say that sodium does *not* exist in the sun because the fluted spectrum is *not* represented.

It is not necessary to enlarge upon this point because the difficulty in which the theory of exchanges is thus landed is obvious, while, if we acknowledge different molecular groupings in the vapours of the same chemical substance, and apply the theory of exchanges *to each grouping*, then the teachings of that theory become more numerous and important than before.

It is of course of the highest importance to see whether there is any *experimentum crucis*—any mode of inquiry—by which the theory can be settled one way or the other.

I submit that the results of experiments based on the following considerations ought to be accepted as throwing light on the question.

1. At different temperatures the brilliancy of the spectral lines of the same substances as ordinarily observed changes enormously. See if these changes can be produced *at the same temperature* by employing those experimental conditions which will be most likely to bring about different molecular conditions if such exist.

2. At a low temperature some substances give us few lines while at a high one they give us many. Vapours, therefore, already glowing with few lines at a low temperature, say in a flame, should give us all their lines when the vapour is suddenly subjected to a high one, say by the passage of a high tension spark. On the bell hypothesis the spectrum should change with the mode of striking. On the dissociation hypothesis this should only happen for the lines of those molecular groupings which are *from other considerations* held to be more simple. If the flame has brought the substance to its lowest state, the passage of the most powerful spark should not cause the flame spectrum to vary.

Now what are the "other considerations" above referred to? This necessitates a slight digression.

In the *Phil. Trans.* for 1873 [1] I gave an historical account, showing how, when a light source such as a spark or an electric arc is made to throw its image on the slit of a spectroscope, the lines had been seen of different lengths, and I also showed by means of photographs how very definite these phenomena were. It was afterwards demonstrated that for equal temperatures chemical combination or mechanical mixture gradually reduced the

[1] Continued from p. 312.

[1] *Phil. Trans.*, 1873, p. 254.

spectrum by subtracting the shortest lines, and leaving only the long ones.

On the hypothesis that the elements were truly elementary, the explanation generally given and accepted was that the short lines were produced by a more complex vibration imparted to the "atom" in the region of greatest electrical excitement, and that these vibrations were obliterated, or prevented from arising, by cooling or admixture with dissimilar "atoms."

Subsequent work, however, has shown[1] that of these short lines *some* are common to two or more spectra. These lines I have called basic. Among the short lines, then, we have some which are basic, and some which are not.

The different behaviour of these basic lines seemed, therefore, to suggest *that not all of the short lines of spectra were, in reality, true products of high temperature.*

That some would be thus produced and would therefore be common to two or more spectra we could understand by appealing to Newton's rule: "Causas rerum naturalium non plures admitti debere quam quæ et veræ sint et earum phænomenis explicandis sufficiant," and imagining a higher dissociation. It became, however, necessary to see if the others would also be accounted for.

Now if not all but only *some* of the short lines are products of high temperature, we are bound to think that the *others* are remnants of the spectra of those molecular groupings first to disappear on the application of heat.

At any particular heat-level, then, some of the short lines may be due to the vibrations of molecular groupings produced with difficulty by the temperature employed, while others may represent the fading out of the vibrations of other molecular groupings produced on the first application of the heat.

In the line of reasoning which I advanced a year ago,[2] both these results are anticipated, and are easily explained. Slightly varying Fig. 2 of that paper, we may imagine furnace A to represent the temperature of the jar spark, B that of the Bunsen burner, and C a temperature lower than that of the Bunsen burner (Fig. 1).

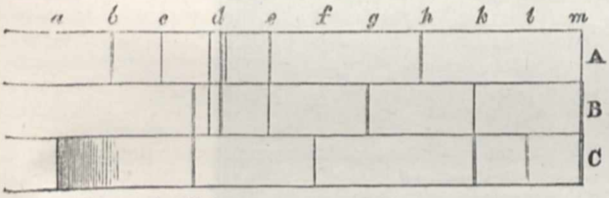

FIG. 1.—A. Highest temperature. C. Lowest temperature.

Then in the light of the paper the lines *b* and *c* would be truly produced by the action of the highest temperature, *c* would be short and might be basic, while of the lines *h* and *m*, *m* would be short and could not be basic, because it is a remnant of the spectrum of a lower temperature.

So much then by way of explanation; it is clear that to make this reasoning valid we must show that the spark, or better still the arc, provides us with a summation of the spectra of various molecular groupings into which *the solid metal which we use as poles* is successively broken up by the action of heat.

We are not limited to solid metals; we may use their salts. In this case it is shown in the paper before referred to[3] that in very many cases the spectrum is one much less rich in lines.

The experimental work has followed two distinct lines. I shall refer somewhat in detail to the results obtained along each. The first relates to the extraordinary and beautiful phenomena and changes observed in the spectra of vapours of the elementary bodies when volatilised at different temperatures in vacuum tubes. Many of the lines thus seen alone and of surpassing brilliancy, are those seen as short and faint in ordinary methods of observation, and the circumstances under which they are seen suggest, if we again apply Newton's rule, that many of them are produced by complex molecules.

In this case the appeal lies to the phenomena produced when organic bodies are distilled at varying temperatures; the simplest bodies in homologous series are those volatilised at the lowest temperatures; so that on subjecting a mixture of two or more liquids to distillation, at the beginning a large proportion of the more volatile body comes over, and so on.

The novelty of the method consists in the use of the luminous electric current as an explorer and not as an agent for the supply of the vapours under examination; that is to say, the vapours are first produced by an external source of heat, and are then rendered luminous by the passage of the current. The length and bore of the tube therefore control the phenomena to a certain extent.

A form of apparatus which I have found to answer very well is shown in the accompanying woodcut (Fig. 2).

A is the tube or retort containing the metal experimented on in its lower extremity, and having a platinum wire sealed into it at a distance of about two inches from the lower end, the other end being drawn out and connected by a mercury joint to an ordinary Geissler tube, which is connected by another mercury joint to the Sprengel pump C.

Another form of tube which I have used is prepared by inserting two platinum poles into a piece of combustion tubing sealed at one end, and after inserting the metal to be experimented on, drawing out the glass between the platinums to a capillary tube.

I have also tried inserting the platinum pole at the end of the retort, so that the spark passes from the surface of the metal, but this arrangement did not answer at all.

Some other modifications have been tried, but the first form I have described is that which I have found to answer best, so far as the trials have yet gone.

D is the spectroscope.

E is the lens used for focussing the image of the Geissler tube on the slit.

F is the spirit lamp for heating the retort.

H is the battery.

K and L are the wires connected with the coil.

In the second cut (Fig. 3) the method of observing the spectrum of the vapours close to the surface of the metal is indicated; the same letters apply, D′ being, however, in this case a direct-vision spectroscope, which was sometimes employed for convenience.

For determining the exact positions of the lines in the spectrum of the vapour in any part of the retort, a larger spectroscope, with its illuminated scale, was used in the place of the direct-vision spectroscope.

The secondary wires of the coil were connected, one with the pole in the upper bulb at B, and the other with the platinum at A.

B is an ordinary Geissler tube with two bulbs separated by a capillary tube. The great advantage of this arrangement is that this capillary portion can be used for ascertaining what gases or vapours are carried over by the pump without any interference with the retort, both wires being connected with the Geissler tube. If, for example, we are working with sodium which contains an impurity of hydrocarbon, the moment at which it begins or ceases to come off can be found by examining the spectrum of this capillary tube.

I now give an account of the phenomena observed when we were working with sodium, in order to show the kind of phenomena and the changes observed.

After a vacuum has been obtained the retort is heated gradually. The pump almost immediately stops clicking,

[1] *Proc. R.S.*, vol. xxviii. p. 159. [2] *Proc. R.S.*, vol. xxviii. p. 162. [3] *Phil. Trans.*, 1873, p. 258.

and in a short time becomes nearly full of hydrogen. The spectrum of the capillary then shows the hydrogen lines intensely bright. After some time the gas comes off far less freely, and an approach to a vacuum is again obtained. Another phenomenon now begins to show itself: on passing the current a yellow glow is seen, which gradually fills the whole space between the pole in the retort and the metal; its spectrum consists of the lines of hydrogen and the yellow line of sodium, the red and green line being both absent until the experiment has gone on for some time.

As the distillation goes on, the yellow glow increases in brilliancy, and extends to a greater distance above the pole, and the red and green lines presently make their appearance as very faint lines.

The upper boundary of the yellow is quite sharp, the lines and fluted spectrum of hydrogen appearing above it.

After the yellow glow-giving vapour (which does not attack the glass) has been visible for some time, the pump is stopped and the metal *heated more strongly*. On passing the current a little while afterwards, a very brilliant leaf-green vapour is seen underlying the yellow one, and connected with it by a sap-green vapour. The spectra then visible in the tube at the same time are—

Leaf-green ...	Green and red lines of sodium and C of hydrogen; D absent.
Sap-green	Green, red, and yellow sodium lines of equal brilliancy and C of hydrogen.
Yellow	D alone and C.
Bluish-green ...	C and F and hydrogen structure.

To observe the green sodium line alone it is necessary to point the direct-vision spectroscope just above the surface of the metal where the green is strongest. It is also necessary to guard against internal reflections from the glass, as this may sometimes cause the D line to be seen by reflection from the surface.

This method of inquiry has been tried also with potassium, calcium, and some other metals, and with metallic salts.

With potassium and calcium we get the same inversion of phenomena, the yellow-green lines of potassium being seen without the red; while in the case of calcium the blue line alone was seen.

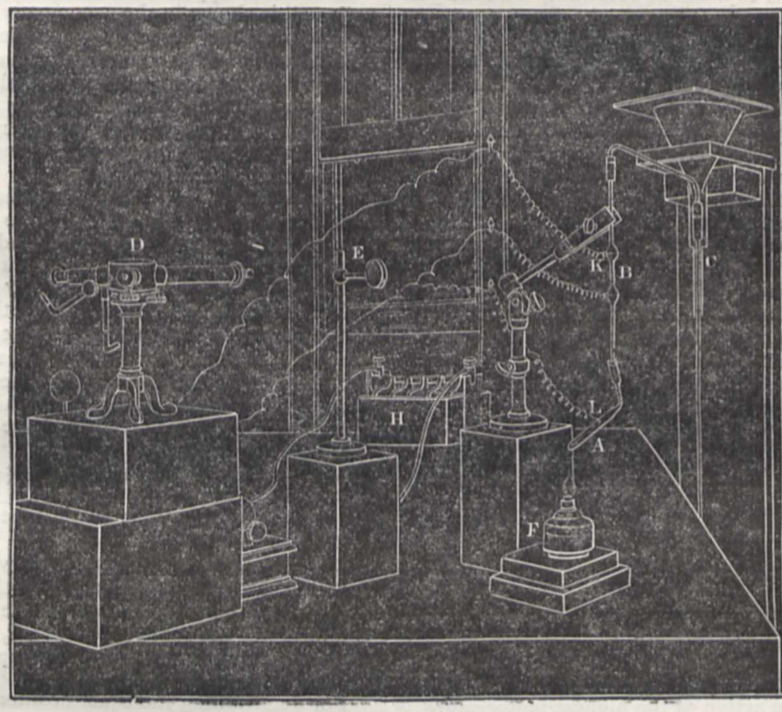

FIG. 2.—Distillation Apparatus.

The fact that in these experiments we get, as before mentioned, vapours which at one and the same time exhibit different colours and different spectra at different levels in the tube, at once suggests the phenomena of fractional distillation.

It is also suggested, as a result of the application of this new method, that in the case of a considerable number of chemical substances not only the line spectrum is compound in its origin, as I suggested many years ago, but that a large number of the lines is due to molecular groupings of considerable complexity, which can be kept out of the reaction by careful low temperature distillation.

So much then for one method. Now for the other.

In this I have attempted to gain new evidence in the required direction by adopting a method of work with a spark and a Bunsen flame, which Col. Donnelly suggested I should use with a spark and an electric arc. This consists in volatilising those substances which give us flame spectra in a Bunsen flame and passing a strong spark through the flame, first during the process of volatilisation, and then after the temperature of the flame has produced all the sim; lification it is capable of producing.

The results have been very striking; the puzzles which a comparison of flame spectra and the Fraunhofer lines has set us find, I think, a solution; while the genesis of spectra is made much more clear.[1]

To take an instance, the flame spectrum of sodium gives us, as its brightest, a yellow line, which is also of marked importance in the solar spectrum. The flame spectra of lithium and potassium give us, as their brightest, lines in the red which have not any representatives among the Fraunhofer lines, although other lines seen with higher temperatures are present.

Whence arises this marked difference of behaviour?

[1] I allude more especially to the production of triplets, their change into quartets, and in all probability into flutings, and to the vanishing of flutings into lines, by increasing the rate of dissociation.

From the similarity of the flame spectrum to that of the sun in one case, and from the dissimilarity in the other, we may imagine that in the former case—that of sodium—we are dealing with a body easily broken up, while lithium and potassium are more resistant; in other words, in the case of sodium, and dealing only with lines recognised generally as sodium lines, the flame has done the work of dissociation as completely as the sun itself. Now it is easy to test this point by the method now under consideration, for if this be so then (1) the chief lines and flutings of sodium should be seen in the flame itself, and (2) the spark should pass through the vapour after complete volatilisation has been effected without any visible effect.

Observation and experiment have largely confirmed these predictions. Using two prisms of 60° and a high-power eyepiece to enfeeble the continuous spectrum *of the densest vapour produced at a high temperature*, the green lines, the flutings recorded by Roscoe and Schuster, and another coarser system of flutings, so far as I know not yet described, are beautifully seen. I say largely, and not completely, because the double red line and the lines in the blue have not yet been seen in the flame, either with one, two, or four prisms of 60°, though the lines are seen *during volatilisation* if a spark be passed through the flame. Subsequent inquiry may perhaps show that this is due to the sharp boundary of the heated region, and to the fact that the lines in question represent the vibrations of molecular groupings more complex than those which give us the yellow and green lines. The visibility of the green lines, *which are short*, in the flame, taken in connection with the fact that they have been seen alone in a vacuum tube, is enough for my present purpose.

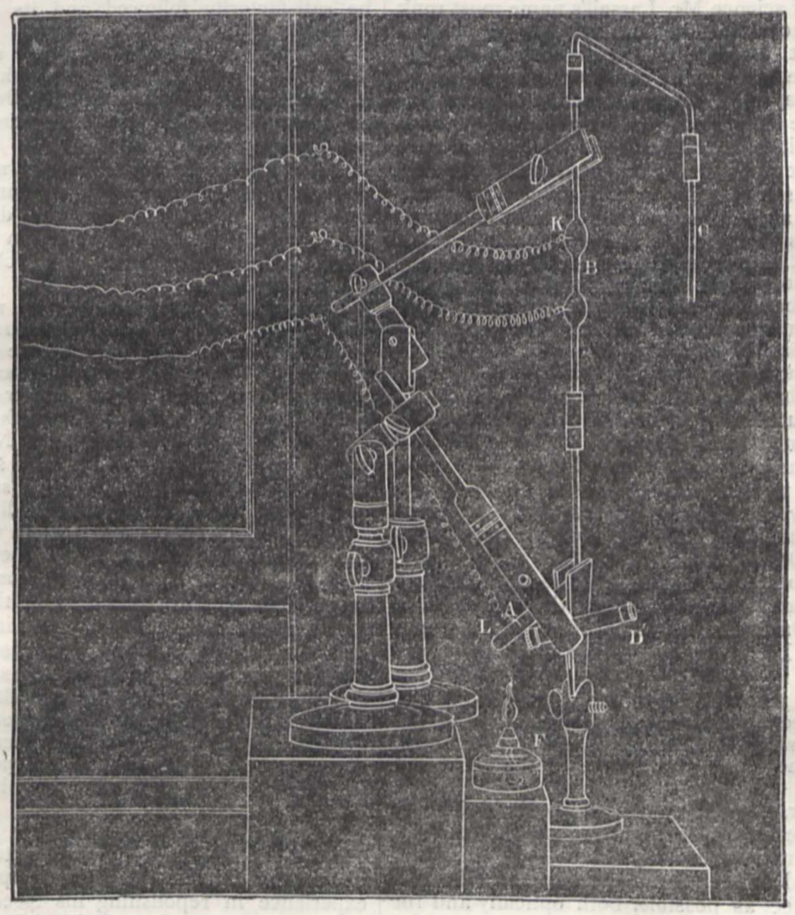

FIG. 3.—Position of Spectroscope for discovering Vapours close to the Metal.

With regard to the second point, the passage from the heat-level of the flame to that of the spark after volatilisation is complete, produces no visible effect, indicating that in all probability the effects heretofore ascribed to *quantity* have been due to the presence of the molecular groupings of greater complexity. *The more there is to dissociate, the more time is required to run through the series, and the better the first stages are seen.*

J. NORMAN LOCKYER

(*To be continued.*)

WILLIAM LASSELL, LL.D., F.R.S.[1]

THE scientific world will receive with deep regret the intelligence of the death of this distinguished astronomer. The smaller circle of those who knew Mr. Lassell personally will deplore the loss of a friend of rare worth. Mr. Lassell passed away without suffering soon after five o'clock on the morning of Tuesday, October 5, in the eighty-second year of his age, full of years and greatly honoured and respected.

In the words of Sir John Herschel, Mr. Lassell "belonged to that class of observers who have created their own instrumental means, who have felt their own wants and supplied them in their own way." The qualities which enabled Mr. Lassell to do all this made him what he

[1] Based on an obituary notice written by the present writer for the *Times*.

was. The work was the revelation of the man. He felt precisely where lay the difficulties and wants which met him in his work, because he was sensitive and sympathetic. He could deal successfully with these difficulties, and supply these wants, often in a masterly and original way, because he could think for himself cautiously and yet boldly. He could work out his conceptions in new and difficult directions to a successful issue, because the constancy of his character showed itself here in concentration of thought and perseverance of action. These qualities, sensitive sympathy, wise prudence, constancy, were those which pre-eminently characterised him as a man and a friend.

In the history of science Mr. Lassell's name must rank with those of Herschel and the late Lord Rosse in connection with that essentially British instrument, the reflecting telescope, whether we consider the genius and perseverance displayed in the construction of these instruments, or the important discoveries which have resulted from their use. About 1820 Mr. Lassell, then in his twenty-first year, began to construct reflecting telescopes for himself. It is perhaps to circumstances which Mr. Lassell at the time considered most unfavourable that science is indebted for much that Mr. Lassell has accomplished. At that time he did not possess sufficient means to enable him to purchase expensive instruments, and besides "his business avocations were such as most men consider of an engrossing nature." The value to him in his subsequent work of the energy and power of resource which were in this way so strongly developed in his character at an early age it is difficult to appraise. His success with the two first instruments which he attempted simultaneously, a Newtonian of 7-inch diameter and a Gregorian of the same size, encouraged him to make a Newtonian of 9-inches aperture. The several mirrors made for this instrument were of great excellence. The observatory note-books of the late Mr. Dawes, which are in the writer's possession, bear record to the delicate tests for figure to which these mirrors were put on the occasions of the visits of Mr. Dawes to the observatory of his friend at Starfield, near Liverpool, where the instrument was erected.

The instrument may be said to form an epoch in the history of the reflecting telescope, in consequence of the successful way in which Mr. Lassell, on a plan of his own, secured to it the inestimable advantage of the equatorial movement.

About 1844 Mr. Lassell conceived the bold idea of constructing a reflector of 2 feet aperture and 20 feet focal length, to be mounted equatorially on the same principle. Mr. Lassell spared neither pains nor cost to make this instrument as perfect as possible, both optically and for the mechanical side. As a preliminary step he visited the late Earl of Rosse at Birr Castle, and commenced the specula for this instrument with a machine similar in construction to that employed by that nobleman. After some months work he was not satisfied with this apparatus, and was led, in consequence, to contrive a machine for imitating as closely as possible those motions of the hand by which he had been accustomed to produce perfect surfaces on smaller specula. "The essential difference of these constructions," to use the words of Sir George Airy, "as regards the movements of the grinder is this: that in Lord Rosse's apparatus every stroke is very nearly straight, while in Mr. Lassell's apparatus there is no resemblance to a straight movement at any part of the stroke." This is not the place to describe the many new contrivances in the mode of support of the mirror, in the equatorial mounting, and in the polishing machine, which enabled Mr. Lassell to bring this instrument to a high degree of perfection. I must not omit to notice, to use Sir John Herschel's words, "that in Mr. Nasmyth he was fortunate to find a mechanist capable of executing in the highest perfection all his conceptions, and prepared by his own love of astronomy and practical acquaintance with astronomical observation, and with the construction of specula, to give them their full effect." Mr. Lassell was very successful in the great brilliancy and permanency of polish of his metal. Within the last few years the writer has been shown specula by Mr. Lassell which had not been polished for more than twenty years, and which appeared as bright as if but just removed from the polishing machine.

With this fine instrument he discovered the satellite of Neptune. This minute body was first seen on October 10, 1846, but it was not until the next year that it could be satisfactorily followed and its existence fully confirmed.

The superiority of the telescope and the vigilance and skill of the observer were further shown by the discovery in 1848, simultaneously with Prof. Bond in America, of an eighth satellite of Saturn, of extreme minuteness, which was named Hyperion.

In 1851, after long and careful search, he discovered two additional satellites of the planet Uranus (Umbriel and Ariel), anterior to the two discovered by Sir W. Herschel in 1787. In the autumn of 1852 he took his 20-foot telescope to Malta, and observed through the winter of that year.

A most careful drawing of the nebula of Orion and drawings of several planetary nebula will be found in vol. xxiii. of the *Memoirs* of the Royal Astronomical Society. With respect to the planets, to use his own words, "his discoveries were rather negative than otherwise," for he was satisfied that without great increase of optical power no other satellite of Neptune could be detected. With regard to Uranus he says, "I am fully persuaded that either he has no other satellites than the four, or if he has they remain yet to be discovered."

Mr. Lassell's energy and zeal in the cause of science did not permit him to remain content with this magnificent instrument. His last work was a much larger telescope, four feet in aperture and thirty-seven feet focus, mounted equatorially. This grand instrument was erected at Malta in 1861, and the work done with it, with Mr. Marsh's assistance during the next four years, is fully described in vol. xxxvi. of the *Memoirs*. This work consists of numerous observations of nebulæ and planets and a catalogue of the places of 600 new nebulæ discovered at Malta. It is not possible to suppress a feeling of regret that this magnificent instrument no longer exists.

After his return from Malta Mr. Lassell purchased an estate near Maidenhead, and erected in an observatory his equatorial telescope of 2-feet aperture. Mr. Lassell's experience in repolishing his 4-feet mirrors suggested to him some alterations in his polishing machine. After his return he was able to carry out these experiments in a workshop erected at Maidenhead, and succeeded in constructing an improved form of polishing machine, which is described in the *Transactions* of the Royal Society for 1874. In 1839 Mr. Lassell was elected a Fellow of the Royal Astronomical Society, received its gold medal in 1849, and in 1870 was elected its president, which office he held for two years. He became a Fellow of the Royal Society in 1849, and received one of its gold medals in 1858. Among other honours conferred upon him may be mentioned an honorary degree from the University of Cambridge, and the honorary Fellowships of the Royal Societies of Edinburgh and Upsala.

The numerous papers by Mr. Lassell to be found in the *Monthly Notices* and the *Memoirs* of the Royal Astronomical Society bear abundant record to his industry and skill, and make us feel that in Mr. Lassell's death we have to deplore the loss of one who contributed largely to the advancement of the science of his age.

WILLIAM HUGGINS

NOTES

ANOTHER brilliant synthesis has recently been accomplished in the domain of organic chemistry; Messrs. Grimaux and Adam have succeeded in building up citric acid from glycerin. We shall give full details next week. Curiously enough, in the last number of the Berlin *Berichte*, Kekulé announces that he has been working at the same subject, but by a totally different method. Kekulé's work is not sufficiently advanced for him to say positively that his method of synthesis is successful, but he feels justified in saying that very probably the process adopted by him has resulted in the formation of citric acid.

THE death is announced of Dr. Hofrath von Wagner, Professor of Technological Chemistry in the University of Würzburg, and the author of several works on that science, chief of which is "Jahresberichte über Chemische Technologie" and "Handbuch der chemischen Technologie," translated into English by Mr. Crookes. He was born at Leipsic in 1823, and first taught in Nüremberg.

THE credit of the invention of binocular glasses has usually been assigned to a certain Bohemian friar, Father de Rheita, who died at Ravenna in 1660. His treatise, which bears the quaint title of "Oculus Enochi et Eliæ," was published at Antwerp in 1645. In 1677 there appeared at Paris a volume entitled "La Vision parfaite," by another ecclesiastic, Père Cherubin of Orleans, which contained an account of some improvements on de Rheita's discovery, illustrated by excellent copper-plate engravings. Lately however Signor Govi has unearthed in the Bibliothèque nationale a printed document which proves the antiquity of binocular glasses to be a little more remote. This document is a placard by one D. Chorez of Paris, who lived on the island of Nôtre-Dâme, at the sign of the "Compas." The placard is in old French, and is headed "Av Roy"; it states that the "admirable lunettes" it describes, and which are represented by accompanying figures, were invented by Chorez and dedicated to the king in 1625.

IN the placard of the optician Chorez referred to, the address actually printed was "la ruë de Perigneur aux Marais du Temple"; but these words have been struck out with a pen, and above is written "Lisle nostre Dame." The incident is curious as showing that two centuries and a half ago the same quarters of Paris were frequented as now by the instrument-maker. M. Salleron is in the Rue Parée du Marais; M. Lemaire a little farther north, just out of the *Quartier du Temple*, in the Rue Oberkampf. On the island of Nôtre-Dâme the opticians elbow one another in shoals, not to omit M. Bregnet's modest shop. The only district of Paris which can, indeed, compete with these being the *Quartier Latin*, where instrument-makers of all kinds abound.

WITH regard to the announcement in an enterprising provincial contemporary of a projected "Natural History Survey of India," the general conduct of which is to be intrusted to Dr. George King of the Botanical Gardens, Calcutta, we believe that those most concerned know nothing which affords any foundation for the statement. The notion is intrinsically improbable, inasmuch as the "Flora of British India," which is in process of preparation at Kew, and of which the third volume is now in course of publication, covers the same ground. It would be inexpedient for the Government to take any step of the kind as far as botany is concerned till the material collected by Indian botanists since the beginning of the century has been fully worked up, and this is being rapidly proceeded with under the direction of Sir Joseph Hooker, assisted by Mr. C. B. Clarke of the Bengal Education Department, who has been detached on duty at Kew for the purpose.

THE case recently reported in the newspapers of poisoning by American tinned beef is calculated to arouse much alarm in the minds of those who use tinned meats. According to the newspaper report of the inquest, no direct evidence was given that poisoning was actually due to zinc or other metallic poison; in the present state of knowledge the explanation referred to by Mr. Dyer in his letter to the *Daily News*, viz., that it was due to the unwholesome state of the meat itself, and not to metallic poison absorbed from the tin, seems the most probable. Nevertheless, a series of well-conducted experiments, undertaken by some of the companies whose trade in these meats is so large, on the action of meat juices on tin and on solder, might do much to allay suspicion, and at the same time to advance our knowledge of natural facts.

IN the last session of the United States Congress at Washington, May 24, 1880, the "Committee on Naval Affairs" reported a bill in support of a proposed International Commission to agree upon standard tests for colour-blindness and visual power in navies and merchant marines, and standard requirements of these faculties. Resolutions in recommendation of this Commission have been passed by the American Ophthalmological Society at their Newport meeting, the Ophthalmological Section of the British Medical Association at Cambridge, and the International Congress of Ophthalmology at Milan. The next United States Congress will act on this bill to initiate the Commission. Dr. R. Joy Jeffries, 15, Chestnut Street (Beacon Hill), Boston, Mass., U.S. America, intimates that he will be greatly indebted for any public or private statistics or information in relation to this subject which any one can send him.

MR. ADAM SEDGWICK, who was elected a Fellow of Trinity College, Cambridge, on Saturday last, the 9th inst., graduated in the First Class of the Natural Sciences Tripos of 1877, when he was especially distinguished for his knowledge of zoology and comparative anatomy, human anatomy and physiology. Those of our readers who are interested in the study of the principle of heredity may be glad to know that this gentleman is the great-nephew and eldest male representative of the illustrious geologist whose name he bears.

IN reference to our note (NATURE, vol. xxii. p. 541) upon the awards of the juries of the Exhibition of Agriculture and Insectology at Paris, wherein we observed that a suggestion had been put forward to arrange the electric light as an insect-catcher, a correspondent writes that in experimenting for other purposes with a Browning electric light upon a roof at Charing Cross, besides innumerable flies and moths, single individuals of two species of sphinxes were attracted, probably from considerable distances.

THE Freedom of the City of London was conferred on Sir Henry Bessemer on Wednesday last week. Sir Henry in his address indicated the vast improvements which his process had introduced in the manufacture of steel, and at the dinner in the evening he sketched the early progress of iron manufacture.

THE French Minister of Public Instruction has caused an edition of Mr. Herbert Spencer's work on Education to be published for gratuitous distribution in France.

A PRACTICAL experiment was, on Wednesday last week, tried with the air-engine at Woolwich, designed by Col. Beaumont, Royal Engineers, and which has been for some time running on the short lines of the Royal Arsenal. Although weighing but 10 tons, it has proved capable of hauling a burden of 16 tons up a fair incline, and arrangements were made to try its powers in a more extended run, such as engines of the kind would have to encounter on the London railways and tramways. The air-reservoir, which contains only 100 cubic feet of air, was charged at the torpedo pumping-house, up to pressure of 1,000 lb. to the square inch, and with this store of energy it was proposed to run to and from Dartford, about 16 miles. The chief feature of Col. Beaumont's method is the introduction of an almost imperceptible supply of steam, by which the air, as it is admitted to the cylinder from the reservoir, is largely heated, and thereby greatly increased in force. The engine is driven by six cylinders and a double set of machinery at one end, and, having no smoke-stack, resembles in appearance a locomotive tender rather than a locomotive. It runs on four wheels, and in size is less than that of an ordinary omnibus. It left the Royal Arsenal at Plumstead Station at 12.22 p.m., with a full charge of 1,000 lb. to the inch, passed Abbey Wood Station at 12.27 with 940 lb. on the guage; Belvedere at 12.33, with 8.60 lb.; and Erith at 12.36, with 760 lb., arriving at Dartford at 12.50, with a remaining energy of 540 lb. on the square inch. Shunting about at the station reduced this pressure somewhat, and at 1·35 the return journey commenced with a store of 510 feet. Although the minimum for effective working is considered to be a pressure of 200 feet, Plumstead station was reached again at 2.10, but the engine was nearly pumped out, having a pressure of barely 80 lb. remaining. It was stated that another engine was being constructed, much more powerful; capable, in fact, of travelling double the distance with a single charge. The operation of pumping in the compressed air occupies about fifteen minutes, and it is calculated that an air engine on this principle as large as the usual steam locomotive of 50 tons weight would be considerably more powerful than any locomotive yet made. The objection to steam, that it frightens horses, cannot apply to this system, as there is no escape of steam visible or audible, and the only noise to be distinguished is a rumbling sound like the rattle of the street traffic.

ONE of the most satisfactory reports on the progress of cinchona cultivation and the harvesting of bark in the Government plantations in Bengal, has just been issued by Dr. King. A summary of the work of the year 1879-80 shows that the plantation was extended by about three-quarters of a million of young trees, a crop of 361,590 lbs. of dry bark was harvested, a new kind of cinchona, namely, that which yields the Carthagena bark of commerce, was brought into cultivation, and the nursery stock was maintained at a sufficiently high level for the supply of young plants for the present year. In the details of the year's planting it is shown that as in former years the species most largely planted was *C. succirubra*, and of this as many as 644,222 were put out. Of the valuable *C. calisaya* and hybrid plants a comparatively large number has been planted; of the hybrid species as many as 39,400 at Mungpoo, and 36,680 at Sittong, and of *C. calisaya* 12,782 at Sittong. The yield of bark during the year amounted to 361,590 lbs. of dry bark. Dr. King further reports that in accordance with the orders of Government arrangements were made towards the end of the year for sending a quantity of the Calisaya bark, which had accumulated in the factory store-room, to London for sale, and since the expiry of the year, part of this bark has actually been despatched: further it is stated that the amount of febrifuge used in substitution of quinine in Government hospitals and dispensaries during the past year was 5,400 lbs. Taking the average price in Calcutta of quinine for the year at Rs. 90 per lb. (a low estimate), the saving effected by this substitution has been ery nearly four lakhs of rupees. The saving in former years from the substitution of the febrifuge having amounted to seven and three quarter lakhs of rupees, the total saving up to the end of last year therefore reaches eleven and three quarter lakhs, which is quite a lakh and a half more than the plantations (including compound interest at 4 per cent.) have cost since their commencement. This is a most satisfactory statement, added to which the introduction through Kew of the valuable species of cinchona, yielding Carthagena or Columbian bark, and the prospect of its successful propagation, makes Dr. King's present report one of very great interest and satisfaction.

WE have received several numbers for the current year (vol. v.) of the *Botanical Gazette*, a Paper of Botanical Notes published at Crawfordsville, Indiana. The *Gazette* appears to have a large circulation in Western America, which, as far as we can judge from the specimens before us, it well deserves. We quote the following interesting and sensible remarks from the editorial notes:—"A new school of botanists is rapidly gaining ground in this country, and we are glad to see it. While the country was new and its flora but little known, it was very natural for systematic botany to be in the ascendency. It is a very attractive thing to most men to discover new species; but when the chance for such discovery becomes much lessened, there is a turning to the inexhaustible field of physiological botany. Systematists are necessary, but a great number of them is not an essential thing, and it is even better to have but a few entitled to rank as authorities in systematic work. But in studying the life-histories of plants or their anatomical structure, we cannot have too many careful observers. This, at the present day, seems to be the most promising field, and one botanist after another is coming to appreciate it. As microscopes are becoming cheaper, and hence commoner, the workers in the histology of plants are becoming more numerous, and it is to such that the *Gazette* would now address itself." We noticed especially some remarkable observations by a correspondent of the *Gazette* on the carnivorous habits of the honey-bee of South America. These would appear, however, to require confirmation before they can be accepted without hesitation.

THE *Valley Naturalist* is the title of a small monthly journal published in St. Louis, U.S. It contains contributions in various departments of natural science; it would be of more value, we think, if it confined itself more strictly to contributions on local natural history, and had fewer miscellaneous items from foreign journals.

WE noticed a few months ago that an international metrological office had been established at Breteuil (near St. Cloud) at the expense of all the civilised nations except England. A part of the duties of this office is to deliver to the associated nations approved standard metres and kilograms for the ulterior construction of other standards, and practical verification of the usual metres and kilograms. The standards intended for France being ready, the Minister of Public Instruction appointed the French national committee, which is composed of MM. Dumas, St.-Claire Deville, Hervé-Mangon, Mascart, and a few others. It may be noted that M. Tresca, who designed the pattern of the international metre adopted by the International Commission, is not one of the new committee.

THE Nineteenth Century Building Society has done a commendable thing in resolving, that as, in their opinion a course of lectures at the Parkes Museum of Hygiene on House Sanitation would be most valuable to the members of building societies (who to a very large extent own the house they live in), the secretary of the society be requested to ask the Committee of the Museum whether such a course of lectures could not be given gratuitously during the ensuing winter.

THE Municipal Council of St. Petersburg is at present deliberating on a proposition made by the Electrotechnik, a Russian society recently established, for illuminating with Siemens lamps the Newsky Prospect, whose length is 7,000 metres.

THE Algerian *Akhbar* says, in one of its last numbers, that the corpses of two European travellers, who according to all probability have died from want of water, have been discovered lying in the desert fifty miles southward of Wargla, the most remote oasis occupied by the French in the Algerian Sahara.

The names and nationality of these two unfortunate travellers have not been ascertained yet, according to our contemporary.

FROM a Japan paper we learn that at the Botanical Garden in Aichi *ken*, an Indian tea-plant, has been planted as an experiment. The leaves have lately been gathered and treated in the same manner as the Uji tea, and it has been found that the product of dried tea is greater in proportion to the quantity of leaves used than in the case of Japanese plants. Tea-growers are, in consequence, said to be devoting their attention to the new plant.

THE additions to the Zoological Society's Gardens during the past week include a Plantain Squirrel (*Sciurus plantani*) from Java, a Smooth Snake (*Coronella lævis*) from Hampshire, presented by Mr. D. Tober; a Plantain Squirrel (*Sciurus plantani*) from Java, presented by Mrs. Elliot; a Common Spoonbill (*Platalea leucorodia*), European, presented by Mr. W. H. St. Quintin; a Common Kestrel (*Tinnunculus alaudarius*), European, presented by Mr. J. Young; two Central American Agoutis (*Dasyprocta isthmica*) from Central America, a Variable Squirrel (*Sciurus variabilis*), a Common Boa (*Boa constrictor*) from South America, two West African Pythons (*Python sebæ*) from West Africa, a European Pond Tortoise (*Emys europæus*), European, two Glass Snakes (*Pseudopus pallasi*), a Lacertine Snake (*Cœlopeltis lacertina*), a Common Snake (*Tropidonotus natrix—var.*), South European, deposited; a Fraser's Squirrel (*Sciurus stramineus*) from Ecuador, a Ring-tailed Coati (*Nasua rufa*), a Cayenne Lapwing (*Vanellus cayennensis*) from South America, three Californian Quails (*Callipepla californica*), purchased; two Gayals (*Bibos frontalis*) from Assam, two Sumatran Porcupines (*Hystrix longicauda*) from Sumatra, an Indian Crocodile (*Crocodilus palustris*) from India, received in exchange.

OUR ASTRONOMICAL COLUMN

HARTWIG'S COMET.—Prof. Winnecke, in a circular issued from Strassburg on October 5, expresses the opinion that it is highly probable the comet discovered by Dr. Hartwig on September 29 was observed in the year 1506, and at his request Dr. Hartwig has submitted the point to calculation, using the first approximation to the orbit which we gave last week. Laugier computed elements of the comet of 1506, from the rough accounts left by European chroniclers and one in the Chinese annals, but his places were necessarily very arbitrarily fixed in this case, as may be seen on referring to his communication presented to the Academy of Sciences at Paris on January 26, 1846. It has not been consequently from any striking similarity between the orbits that Prof. Winnecke has been led to conjecture the identity of the comets, but rather, it would appear, from a general resemblance of track, allowance being made for the somewhat later appearance in the year of the comet of 1880. The Chinese observations do certainly in some cases enable us to make reliable approximations to the orbits of comets, as, for instance, in 568 and 1337; indeed for the latter comet they furnish a remarkably good outline of its apparent path, considering the difficulties which in many cases attend the interpretation of the Chinese accounts: nevertheless for the great majority of comets recorded in their annals the descriptions are unfortunately totally insufficient for this purpose, one very common failing being the omission of dates corresponding to the positions given, as for the comet of A.D. 178, which must have passed very near the earth from the long track it described in the heavens.

As regards European observations of the comet of 1506, Pingré tells us (on the authority of the Chronicles which, according to his excellent custom, are named in his margins), that a comet was seen in the month of August in the north, or between the north and east, or lastly between the west and north, and as the comet was not distant from the Pole, so that it appeared in the evening after sunset, and in the morning before sunrise, it may have had at different hours of the night the various positions mentioned by the historians. It had a long and bright tail which extended "between the fore and hind-wheels of the chariot." On August 8 a Polish historian, an eye-witness, says it was seen near the Pole above "the seven stars or the stars of the great chariot;" on the following night it was situated amongst the same stars, and later, on several nights, it was seen below them; declining by the signs Cancer, Leo, and Virgo, it attained the northern part of the horizon and disappeared on August 14. Some writers limit its appearance to eight days; others say it was visible for three weeks, or even a month.

With respect to Chinese observations, Pingré quotes from Gaubil's manuscript, of which he made so much use, which was preserved in the Depôt de la Marine at Paris in his time, but since understood to be lost, and from Mailla and Couplet. We have now the fuller translations by Biot and Williams. We read that in the first year of the epoch Ching Tih, in the reign of Woo Tsung, on the day Ke Chow of the 7th moon (1506, July 31), a star was seen to the west without the boundary of Tsze Wei (the circle of perpetual apparition). . . . After some days it had a short tail. It was seen between the sidereal divisions Tsan (determined by δ Orionis) and Tsing (by μ Geminorum), the Chinese sidereal divisions, it must be remembered, being intervals of right ascension with wide limits of declination reckoned from the determining star of the division, which we have here taken from Biot. It gradually lengthened, extending in a north-westerly direction towards or to Wan Chang (θ, ν, ϕ Ursæ Majoris). On August 10 it was bright, and moved to the south-east, it lengthened to about 5° and swept the upper of the stars Hea Tae (ν, ξ, Ursæ Majoris), and entered within the space Tae Wei Yuen (Biot's *Thai-Wei*), a space between stars in Leo and Virgo, to which, as also to Tsze Wei, the circle of perpetual apparition mentioned above, constant reference is made in the Chinese cometary observations. For the limits of this space Williams may be consulted. Biot and he substantially agree in their translations. Dr. Hartwig assumes the perihelion passage in 1506 to have occurred on July 1, old style, and with the elements of 1880 finds a track of which it is remarked, "Die Uebereinstimmung des so gefundenen Laufes mit dem wirklich beobachteten ist eine vollständige." The track is thus given:—

	R.A.	Decl.		R.A.	Decl.
	°	°			°
July 19 ...	97·1 ...	+39·3	Aug. 18 ...	250·1 ...	+54·5
29 ...	106·6 ...	61·3	28 ...	258·1 ...	37·0
Aug. 8 ...	201·9 ...	77·9			

We should incline to characterise the presumed identity of the comets of 1506 and 1880 as one rather of possibility than of high probability.

From observations at Strassburg on September 29 and October 1, and one at Leipzic on October 3, Mr. Hind has deduced the following elements:—

Perihelion passage, September 6·9182 G.M.T.

	° ′ ″	
Longitude of perihelion	81 1 37	App. Eq.
,, ascending node ...	44 19 47	Oct. 1.
Inclination of orbit	38 28 11	
Logarithm of perihelion distance ...	9·558048	

Motion—retrograde.

As regards position the comet may be observed for many weeks, but the brightness will be rapidly declining. Since it was not detected till three weeks after perihelion passage, it is desirable that observations should be continued as long as practicable, if the character of the orbit is to be decided at this appearance.

GEOGRAPHICAL NOTES

THE newly-published volume of the Geographical Society's *Journal* contains some useful and even valuable contributions to geography. The veteran traveller, Capt. R. F. Burton, furnishes a memoir respecting the new map of Midian constructed by the officers of the Egyptian General Staff. Capt. Burton however, as might be expected, supplies geographical information beyond that given by the Egyptian officers. He also contributes a second paper of a different character on the subject of a visit to Lissa and Pelagosa. Even more valuable than Capt. Burton's first paper is Lieut. R. C. Temple's account of the country traversed by the second column of the Tal-Chotiali field-force in the spring of 1879, with his sketch-map of part of the country passed over by it between Candahar and India. This memoir has evidently been drawn up with elaborate care, and embodies a mass of important information. The notes upon some astronomical observations made in Kordofan and Darfur by Major H. G. Prout of the Egyptian Staff are also of value, and are accompanied by a map of routes in the two provinces, constructed by the Society's draughtsman from the reconnaissances of various officers in the service of the Khedive. Mr. E. Colborne Baber, lately our Consular representative at Chungking in Western China, also communicates through the Foreign Office some brief remarks under the heading of "Approximate Determination of Positions in South-Western China," to which are appended a number of tables of observations for latitude, &c.

IN the middle of last January Mr. W. S. Jerdan and a small party started from the Elderslie station on the Diamantina River, in Western Queensland, for the purpose of exploring the Mackinlay Ranges for gold. Leaving the Booker-Booker Mountain, with its dark fringe of gidya scrub, on their left, and Mount Munro on the right, they travelled up the Diamantina over splendidly grassed downs, and as they advanced up the river they found that the grasses became even finer and herbs more plentiful. After eight days' marching the party reached the neighbourhood of the Mackinlay River, and they report that the country passed over for some time previously was principally level plain, and just at that season perfectly bare, with the exception of a few tussocks. After about another week they got out of the low country and obtained their first good view of the Mackinlay Ranges, which they describe as presenting a very picturesque appearance in the distance, with their numerous pinnacles, peaks, and flat-topped mountains. The country along the ranges is covered with granite boulders, or else consists of decomposed granite flats infested with spinifex, with numerous sandy creeks running through it in all directions. The party spent about two months in searching for gold, but met with little success.

SIGNOR BIANCHI has reported to the Milan Society, which sent him out to Shoa and other parts of North-Eastern Africa for the purpose of making commercial explorations, that he has been able to make some corrections in the position of places as given in our existing maps. Antotto he places in 8° 53' N. lat., 36° 15' E. long., instead of its present position further north. Fanfinni is really north-north-east and not south of Antotto, while the Salala Mountains are fifty kilometres from Fanfinni, and not close to it. Lake Zouay he has not met with, though his route ought to have taken him to it, according to the map.

THE United States Navy Department have received through the Russian Government a letter from the Captain of the Arctic Exploration steamer *Jeannette*, dated from Cape Serdze Kamen, August 29, 1879, which reports the arrival of the *Jeannette* at that place on the afternoon of the above date. The letter states that the members of the expedition were all well, and that they expected to sail that night for Wrangell Land, by way of Kaliutchin Bay. This news has taken more than a year to reach America. The *Corwin* has arrived at San Francisco, and is reported to have searched all the region between Point Barrow and Herald Island, without finding any trace of the expedition. Still he thinks there is no reason yet to give up hope.

The new number of *L'Exploration* is an improvement on previous ones. We have a good article on the commercial relations between France and Russia; information as to the progress to their destination of MM. Revoil and Crevaux; an interesting analysis of an article on Ausland, on the country of Muata Yanvo, a letter from Dr. Quintin on a former expedition to the Upper Niger, and letters from Matteucci on the progress of his expedition in the Sudan. The notes are also much better edited.

CAPELLO and Ivens have furnished to the Portuguese Government a detailed account of their African explorations, a great number of drawings, and a comprehensive map containing an important part of Portuguese Africa, and also the adjacent territories. Next year Capello and Ivens will return to Africa to finish their explorations, and make a complete chart of the province of Angola.

THE death has just taken place at Pitminster, near Taunton, of Capt. Hobson, of the Royal Navy, who in his earlier days took an active part in the search for the remains of the late Sir John Franklin, and was the discoverer of the records which afforded the clue to the lamented explorer's fate. He was second in command, then holding the position of lieutenant to Capt. McClintock, who, in the year 1845, sailed in the *Fox* to search for the Franklin Expedition. Hobson was the leader of one of the parties which went in search of traces of Franklin, and he succeeded in finding the brief record which only too clearly set at rest the conjectures which the public entertained as to Sir John Franklin's fate.

THE expedition which left France on October 5 for the exploration of the country between the Upper Senegal and the Niger, though mainly for military and commercial purposes, is likely, if successful, to add greatly to the fulness and precision of our knowledge of that region of Africa. Astronomical, geodetical, and topographical officers accompany the expedition, so that we may expect important scientific results. The terminus on the Niger will be either Bamakou or Dina, above Yanina and Segon.

COL. FLATTERS has returned from his explorations in the Touareg region.

THE Wellington correspondent of the *Colonies and India* states that the area of the Crown forest lands in New Zealand in 1879 was estimated at 10,158,870 acres, but it has been proved that some of the most valuable kinds of timber have been recklessly used, and it is said that at the present rate of consumption all the splendid *kauri* forests will be exhausted in twenty-one years, and that the value of the timber will be about 11,000,000*l*. He does not however appear to have taken into consideration the very serious effect which this wholesale destruction of forests will have upon the climate of New Zealand.

PROF. ASAPH HALL ON THE PROGRESS OF ASTRONOMY[1]

ASTRONOMY, in some of its forms, reaches back to the most distant historical epochs, and the changes that it has undergone during this long lapse of time give to this science a peculiar interest. In no other branch of human knowledge have we such a long and continuous history of the search after truth, of the painful struggle through which men have passed in freeing themselves from theories approved by the wise of their own times, and in overthrowing beliefs which had become incorporated into the life and culture of those times. Perhaps the grand array of the heavens, and the vast phenomena which they display, naturally led men to the invention of complicated theories; but these passed away at last before the test of observation and the criticism of sceptical men; and the Copernican theory of our solar system, Kepler's laws of elliptical motion, and the Newtonian law of gravitation, gave to astronomy a real scientific character.

The discovery of the laws that govern the motions of the heavenly bodies, and the construction of the theory of these motions, demanded from practical astronomy better observations and a more accurate determination of the orbits of the planets and the moon, or of the constants that enter into the problems of celestial mechanics; and this demand led to an improvement in the instruments, and in the art of observing. The astronomers and instrument-makers of England and France led the way in these improvements. The great national observatories of those countries were established, and in England, Flamsteed and Sharp, Bird and Bradley, were foremost in raising practical astronomy to the condition of satisfying the demands of theory. But theoretical astronomy was soon to receive a wonderful advancement. Perhaps no one contributed more powerfully to this progress than Lagrange. The writings of this man are models of simplicity and elegance; and yet so complete and general are his investigations, that they contain the fundamental theorems of celestial mechanics. By the invention and perfection of the method of the variation of the arbitrary constants of a problem, and by the establishment of the differential equations of a planetary orbit depending on the partial differential coefficients of a single function, Lagrange reduced the question of perturbations to its simplest form, and gave the means of deducing easily the most interesting conclusions on the past and future condition of our solar system. To supplement this great theorist there was needed another kind of genius. Combining the highest mathematical skill with unequalled sagacity and common sense in its application, Laplace gathered up and presented in a complete and practical form the whole theory of celestial mechanics. Besides his numerous and brilliant discoveries in theoretical astronomy, Laplace gave us some of the finest chapters ever written on the theory of attraction,[2] and a complete treatise on the calculus of probability.

[1] Address as Vice-President of Section A, at the Boston meeting of the American Association.
[2] "Ein schönes Document der feinsten analytischen Kunst."—GAUSS.

By such labours as these the questions of astronomy were brought into order and classified, and the attention of astronomers was directed better than ever before to the determination of the quantities which must be found from observation. Moreover, the refinement of analysis and the completion of theory brought out new and more delicate questions, not less interesting, and requiring more complete investigation and more powerful instruments. The careful examination and study of the instruments and methods of observation became necessary, as well as complete and rigorous methods of reduction; and finally there was needed a critical and satisfactory method for the discussion of observations. For these last improvements in astronomy we are indebted chiefly to the astronomers and mechanics of Germany.

Among those who contributed by means of their optical and mechanical skill to furnish astronomy with the instruments necessary for its further advancement, no one holds a more honourable place than Joseph Frauenhofer. This man began his scientific work at the age of twenty-two, and died at thirty-nine, and yet in those seventeen years he gave to astronomy great improvements in the manufacture of optical glass, driving clocks for equatorials, and telescopes and micrometers, that in the hand of Bessel and Struve gave to observations a degree of accuracy hardly thought of before. To such men as Frauenhofer and his co-workers, who have carried on and improved the construction of instruments of precision, practical astronomy owes much; and yet, after all, the principal thing in a science is the man himself. No matter how excellent the instruments may be, the question whether they shall be used for the advancement of the science, and shall contribute the full value of their peculiarities to help towards increasing the accuracy of astronomical determinations, depends wholly on the astronomer. Again, astronomy is now so completely a science, and all its operations are so closely connected with theory, that no one is fit to have charge of an extended series of astronomical observations who has not a fair amount of theoretical knowledge. Without such knowledge his labour is apt to be thrown away, and is never so effective.

As a good example of what the modern astronomer should aim to be, we may take Bessel. To this man we owe a large part of our best methods for the examination and determination of the errors of our instruments and the introduction of complete and rigorous methods for the reduction of observations. Bessel's reduction and discussion of Bradley's observations was a masterpiece of its kind, bringing out the value of Bradley's work, which had lain unnoticed for more than half a century, and forming a starting-point for sidereal astronomy. This work was continued and perfected in his tables for the reduction of astronomical observations, published twelve years afterwards, a work that has done more than anything else to introduce order and system into practical astronomy. In the discussion of instruments and the determination of their errors, Bessel's conception of an instrument was that of a geometrical figure, and the positions of the lines and divisions of this instrument were considered with corresponding rigour. Although devoted almost entirely to astronomy, yet Bessel was an able mathematician, and of this he has left abundant proof. It seems to be necessary that a man should die and be forgotten personally before his work can be fairly estimated; but time adjusts these matters at last, and I know of no astronomer whose work promises to endure the judgment of the future better than that of F. W. Bessel.

It has been said that for producing the most puzzling compound of metaphysics and mathematics something which has neither height nor depth, nor length nor breadth, and which no one can understand, the German mathematician is unequalled. And at the same time it must be said that, for clearness of conception and beauty and precision of expression, Germany has produced in Gauss a mathematician who is unsurpassed, and who is worthy a place by the side of Lagrange. Omitting all reference to the works of Gauss in theoretical astronomy and in geodesy, which are many and important, I refer here only to his method for the discussion of observations and of deducing the most probable values of our constants. Almost the entire work of astronomy is a vast system of numerical approximation, in which the first steps are obvious and easy, but where the theory soon becomes complicated and the labour enormous. Thus the calculation of the approximate orbit of a planet or of a comet is the work of only a few hours; but the computation of the perturbations and the correction of the elements from all the observations may be the work of months and years. It is therefore of the highest importance that we should have a method for the discussion of observations that will give us the best result, and which will introduce order and system into this department of astronomy. Such a method is that of least squares. For the complete theory of this method and for nearly all the arrangements and algorithms necessary for its practical application, we are indebted to Gauss. The invention and application of this method to the discussion of observations of all kinds seems to me one of the greatest improvements of modern times, and its proper use will lead to a steady progress in astronomy. We must remember, however, that this method does not undertake the improvement of the observations themselves, as some have seemed to think; but, when rightly used, it produces simply the best result we can hope for from a given series of observations. It does not therefore dispense with skill and judgment on the part of the astronomer, but one is tempted to say that, if he has not these prime qualities, then the next best thing for him to have is the method of least squares. The use of this method has become one of the chief characteristics of modern astronomy, and if we compare the results of its application with those of the older methods, we shall see its superiority. Thus, for example, no astronomer of to-day who is accustomed to the modern methods of discussion, would be satisfied with the manner in which Bouvard represents in his tables the observations of Jupiter and Saturn, but would suspect at once some error in his theory of the motions of these planets.

The present condition of astronomy is the result of the continued labours of our predecessors for many generations; and to this result the lapse of time itself has largely contributed. For the full development of the secular changes of our solar system, for an accurate knowledge of the proper motions of the stars of our sidereal universe, and of the great changes of light and heat that are going on among them, the astronomer must wait until future ages. It is his present duty to prepare for that future by making the observations and investigations of his own day in the best manner possible; and to do this needs a careful consideration of the present condition of the science. Although the objects for observation have become so numerous, and the range of investigation so wide, that there is room for the most varied talent and skill, yet there is danger that there may be a waste of labour either in duplicating work, or in doing it in an improper manner. Especially may this happen in observations of the principal planets of our system, and of the fixed stars. In the case of the planets the observations are abundant, and the orbits are already well determined, except that of Neptune, for which, on account of its slow motion, we must of necessity wait for time to develop its small peculiarities, if such there be. For all these planets the observations at one or two observatories are amply sufficient, and even then the observations ought to be confined to a short time near the opposition, or at quadrature, and so made that they may be easily combined into a single normal position, which will suffice for the theoretical astronomer. To scatter such observations over a period of several months is to throw away one's labour, and to leave to the computer the disagreeable duty of rejecting a part of the observations as useless. It seems to me, therefore, unwise for several observatories to continue heaping up observations of the four outer planets of our system, when ten observations a year of each planet will give all the data that are needed. Again, for all the principal planets, observation is now in advance of theory, except, perhaps, in the case of one or two of them. Thus, for Saturn, all the tables are decidedly in error, and, although an attempt has been made to accuse the observations of this planet, it is quite certain that the trouble lies in the theory; for in the case of Jupiter and Saturn we have the most complicated planetary theory of our system, and one that has not yet been completely developed. It seems to me, also, that observations of our moon might well be confined to one or two observatories. Here again observation is far in advance of theory, if indeed there be now in use anywhere a pure lunar theory. All the lunar ephemerides that we have are affected with empirical terms, and the lunar theory itself remains an unsolved mystery. In this case there is no attempt to impeach the observations. The trouble seems to be with the perturbations of long period, and this does not call for numerous observations during each lunation. By a proper consideration of these matters astronomers may, I think, save themselves much useless labour.

Observations of the fixed stars are of the utmost importance in astronomy, since the positions of the stars are of the fundamental points on which depends our knowledge of the motions of the planets, the moon, and of the stars themselves; and it is on account of this fact that Bessel's tables, published in 1830, were of such great service, since they introduced correct and elegant methods of reduction, and clearly defined all the constants and epochs. We now have the positions of several hundred stars so well known that they may be safely used in the reduction of observations; and for these accurate positions we are largely indebted to the astronomers of the Pulkowa Observatory, who have made such absolute determinations a special work. There is still an opportunity for the improvement of these positions, and every well-executed determination will be of value; but it is doubtful if crude and irregular observations can add anything to our knowledge of the positions of these stars. Neither can the routine, mechanical style of observing, that is apt to prevail in large observatories, be of much use here. It would be better in most cases for such observatories to assume the positions of the fundamental stars, and to leave the further improvement of their places to skilful astronomers who understand the theory of such work, and who carefully study and become masters of their instruments. In these refined observations the refraction of light by our atmosphere also plays an important part, and this question will need to be examined at every observatory that undertakes to do independent work. It is true that every new and good meridian instrument may, and perhaps ought, to contribute something towards removing constant errors, and giving us a more accurate knowledge of a star's position; but when this position is very well known, the only way for further improvement is through complete and careful observations, and their thorough reduction and discussion.

In the observations of double stars but little had been done before the present century, and the labours of W. Struve form the real starting-point in this branch of astronomy. These labours have been ably continued by his son, the present director of the Pulkowa Observatory, and the observations of these two astronomers, extending over a period of nearly sixty years, are of the greatest value for our knowledge of the motions of the double stars. This is a branch of the science into which irregular workers are apt to enter, and where some of them have done good service; but if any amateur astronomer will compare his own work with that of the Struves, and will study the methods followed by them in determining their personal and instrumental errors, and will emulate the steadiness with which they have followed out their purpose, he can do much to enhance the value of his labour. Here the observations are simple, and easily reduced, and the chief requisites are skill and patience on the part of the observer. He should not be discouraged because he obtains no immediate or great reward for his work, or public notice, or because some one who rants about the nebular hypothesis and kindred subjects of which he knows nothing is for a time the great astronomer of the day. The observer will learn finally that a good observation of the smallest double star, or of the faintest comet or asteroid, is worth more than all such vague talk. The observation has a positive value, however small, but the physical theories of the universe, of which modern popular science is so productive, are generally worse than useless.

The first step towards a rational and trustworthy knowledge of our sidereal universe must come from a determination of the distances of the stars. The solution of this problem was attempted soon after the Copernican theory of our solar system was established, when it was seen that we have a long base line for our measures, or the diameter of the earth's orbit, and it was supposed that the solution would be easy. These early trials were all failures, but they led to some very interesting and important discoveries, such as Bradley's discovery of the aberration of light; to the knowledge of the fact that the determination of the parallaxes, or the distances of the stars, although simple in theory, is practically a difficult question; and then to an improvement in the instrumental means of observation, to a careful study of the methods of observation and the instruments, and to a recognition of the necessity of a complete and rigorous reduction of the observations. An examination of these early attempts is an instructive study. It is only about forty years ago that the solution of this problem was at last attained, and then only by the application of the most powerful instruments, and the best observing skill. An interesting result of the determinations of stellar parallax is obtained at once in the check it puts on speculations concerning the structure of the sidereal universe. The first astronomers who considered the parallaxes of the stars very naturally assumed that the bright stars are nearer to us than the faint ones, and therefore they observed the bright stars for parallax. Now, while this assumption may be true as a general statement, the actual determinations of parallax show that some of the faint stars which are not visible to the naked eye are much nearer to us than the brightest stars of our northern sky. Again it was assumed that a large proper motion is a certain index of a star's nearness to us; but observation shows that this also may be an erroneous assumption. This is a problem whose solution is only just begun, but already we know enough of its difficulties to see that we need the most powerful micrometrical apparatus that can be brought into use. The invention of some micrometer that, while as accurate as the present filar micrometer, would give the observer a much greater range of observation, and enable him to select suitable stars of comparison, is something much to be desired. At present the heliometer seems to be the best instrument for observations of this kind. Formerly it was thought that photography would furnish a good method for such delicate determinations; but so far the photographic methods have not given the necessary degree of accuracy in the measurements, and the astronomical use of photography is confined mostly to descriptive astronomy, where, especially in solar eclipses, it has rendered excellent service. Closely connected with the parallaxes of the stars and their proper motions is the interesting question of determining their motions to or from our sun according to the theory of Doppler. Here likewise the numerical determinations are so discordant, that we cannot have much confidence in the results. In both these cases we need more powerful apparatus, and a complete and thorough investigation of the methods of observation. Perhaps some of the large instruments now constructing may be employed in these methods, and we may soon have better results.

A great advance has been made in cataloguing the fainter stars. This work was begun by the French astronomers nearly a century ago, and was continued by Bessel, Argelander, and others. An important step towards the completion of this work was taken by Argelander and his assistants in their great catalogue of the approximate positions of 324,198 stars, which was finished in 1861. This census of the stars will soon be extended, we hope, over the whole heavens; and it already forms the groundwork for the great zone observations of stars now going on in Europe and in this country, and which must be nearly finished. These observations will doubtless reveal many interesting cases of the proper motion of the stars, and will certainly form the basis for a knowledge of the motion of our solar system in space, and for sidereal astronomy generally, such as we have never had before. Our American observatories can render a good service by observing stars of southern declination, since our observatories are ten or twelve degrees farther south than those of Europe, and thus have an advantage of position which ought to be made use of; and which may serve to unite into a harmonious system the observations made in the northern and southern hemispheres. The work of mapping the very faint stars near the ecliptic has also been greatly extended, and it is to this extension that we owe the rapid increase in the number of the small planets between Mars and Jupiter. But besides aiding in the discovery of the asteroids, accurate charts of the small stars have a permanent value in giving us a knowledge of the heavens at their epoch, and also some idea of the distribution of the stars in space.

It is an interesting question whether, among the thousands of nebulæ that are scattered over the heavens, any of them show changes of form or of brightness. These objects seem to be at least as distant as the stars, and as they have sometimes an area of several degrees, they must be bodies of an enormous extent. That changes are going on in these bodies seems probable, but to be visible at such distances the changes must be very great. In this case there is need of much caution in the discussion of the drawings made at different epochs, and by different astronomers with telescopes of different power; since the nebulæ change their appearance with the telescope used, with different conditions of the air, and with a variation of their altitude above the horizon. Here the excellent photometers that have been recently invented, and which are being so well applied to the determination of the brightness of the stars, may give us assistance. Perhaps also new drawings of the nebulæ, and their criticism and discussion, and a full recognition of the difficulties of making such drawings, will soon lead to a decision of the question of their change of form. Since the study of the light of the stars with new and improved photometers has now become a specialty, we may look for more exact and continued observations of the variable stars. This is a matter of which we know but little, and it is one where a persevering observer may do good service. Although he may not find any immediate encouragement in the discovery of remarkable relations among these stars, or the probable cause of their variability, he will be collecting observations that must form the test of every theory. As examples of the result of intelligent and persevering observation, we have the case of the sun-spots, which led directly to the discovery of their period, and its singular variability; and that of the shooting stars, which has shown us a very curious relation between these meteors and the comets, and one which may open to us the most extensive views of the relations between our own solar system and other systems in space.

The present condition of astronomy, with its vast and rapidly increasing store of accurate observations, offers many interesting subjects to the theoretical astronomer. The observations of the stars are now so numerous, and have been so fully reduced and criticised, and the time during which the observations have been made is so extended, that we shall soon have excellent data for a new and very exact determination of the constant of precession. The orbits of the planets and the moon, and their masses, are now so well known, that little uncertainty can arise from this source; and by taking into the calculation a great number of stars in different parts of the heavens, we may be able to determine the motion of the solar system in space, as well as the constant of precession. The constant of aberration also needs a new determination, and since this constant is so closely connected with the theory of light and its velocity, and the methods of its determination are still under discussion, it would be well if several astronomers could determine this constant independently. The value we now use was found by W. Struve from prime-vertical observations, and is apparently very accurate; but no astronomical constant should depend on the work of a single astronomer with a single instrument, when it can be determined so easily and by other methods. The old method of finding the value of this constant from the eclipses of Jupiter's satellites may yet give us a trustworthy value. The value of the other constant necessary for the reduction of observations, that of nutation, must be nearly that found by Peters in his well-known investigation of this question. This value may be verified by a new series of observations of Polaris, or of the declinations of stars situated so that this constant has its full influence on the reductions.

There are many subjects in astronomy that need investigation, but in most cases the labour required is very great, and the completion of the work would occupy a long time. This follows of course from the fact that, with the refinement of observations and their exact reduction, many small terms must be considered which formerly could be neglected. The lunar theory has been a vexed question for the last two centuries, and may remain so for a long time to come. This will no doubt be the case until some able astronomer, with the will and perseverance of Delaunay, shall undertake its complete revision. This question should now be looked on as a purely scientific one, and its definite solution should be undertaken. The theory should not be patched up by guesswork to fit the observations, but should be carried out with the utmost rigour. This is a problem to which a young and able mathematician may well devote his life, and we must expect its solution from some such clear-headed devotee of science. Several of the planetary theories need a new investigation, and some of them are already in the hands of able astronomers. That of mercury is especially interesting in connection with the intra-Mercurial planets, and it is to be hoped that Leverrier's theory of this planet may soon have a careful revision.

Again, among the secondary systems, the satellites of Jupiter and Saturn offer many interesting questions to the astronomer. At present the satellites of Jupiter demand a more complete theory and new tables of their motions. Corrected elements of these satellites may be required for reducing observations of their eclipses, and for deriving a new value of the constant of aberration. These satellites form a peculiar and interesting system, and their theory is so complicated that the labour of correcting their elements and forming new tables would be great, but still within the power of a persevering astronomer. The recent discovery of the connection of comets with streams of meteors has given additional interest to cometary astronomy, and there is plenty of hard work to be done in reducing observations, in computing perturbations, and in deducing the best orbits of the comets. The periodical comets have another interest, since they may give us information concerning the matter filling space. It seems to be probable from different reasons, such as the consideration of the light of the stars, that there must be matter spread throughout the celestial spaces; but the only heavenly body that has directly given us information on this subject is Encke's comet, which has a period of three years and a third. For a long time the motion of this comet was very completely computed by Encke, whose calculations show very strong proof of a resisting medium. These calculations were continued by Von Asten, whose early death prevented him from finishing his work, and the theory of this comet is left in an unsatisfactory condition. It is very desirable that the motion of this comet should be completely investigated, and although the method of the special perturbations of the elements followed by Encke is probably the best that can be used, still in such a case it would be well to apply various methods. Here again, on account of the frequent returns of the comet, the labour of computation is very great, and probably would be enough fully to occupy the time of one astronomer. The interesting questions connected with the motion of this comet ought to induce some one to undertake this laborious work, and these questions are so important that two or three astronomers might well be employed on its theory.

The methods of astronomy have now become so well established that the future advancement of the science is assured, especially since long intervals of time give an increased value to observations. Yet we may hope for improvement in instruments, for the introduction of new methods of observing, for better trained and more efficient astronomers; and perhaps also the rapid advancement of the physical sciences may furnish us with new and more powerful methods of investigation. There is an intimate relation between the instrument-maker and the astronomer, and they should understand each other better than is generally the case. It may seem a small matter that the divisions of a circle, or of a scale, should not be too finely or too coarsely cut; that the reading scale should not be placed in an inconvenient position, and that the illumination of the instrument should be carefully studied, and brought under the control of the astronomer; but these are really essential points, and, if not rightly arranged, are certain to weary the observer and to impair the quality of his work. Such mistakes will not be remedied until the makers better understand the uses of an astronomical instrument, and have correct ideas of the end to be attained. Since our American opticians have placed themselves at the head of their craft, we may hope that our instrument-makers will do likewise, and that they will soon be able to furnish us with the best instruments of precision.

There is one point to which astronomers should give more attention, and from which we may reasonably hope that great advantages to astronomy may come; and that is to the selection of sites for new observatories. It is possible, perhaps probable, that our instruments may be greatly enlarged and improved, and that important discoveries and improvements in the manufacture of optical glass may be made; but it seems certain that we have within easy reach very decided advantages for astronomical work by the choice of better positions for our instruments. Very few American observatories have been established for the purpose of doing scientific work, or with much thought or care for their future condition; but generally they are built in connection with some college or academy, and are the product of local and temporary enthusiasm, which builds an observatory, equips it with instruments, and then leaves it helpless. The atmosphere that surrounds us, and its sudden changes of temperature, are the great obstacles to the good performance of a telescope; and the larger the instrument, and the higher the magnifying power, the more serious are these hindrances. Now, with our present means of travel, we can easily place our instruments at an altitude of eight or ten thousand feet, and above a large part of the atmosphere. In this way we may be able to do with small instruments what at common altitudes can be done only with large ones; and when possible it is always better to use small instruments, since they are more easily handled, and are relatively stronger and better than large ones. Uniformity of temperature may be secured by seeking locations in the tropical islands, or on coasts like that of California, where the ocean winds keep the temperature nearly uniform throughout the year. At great altitudes we may secure a clearness of vision that would be of the greatest value in the examination of faint objects, and by this means, and by persevering and continuous observation, interesting discoveries may be made. It is a matter of course that, except in the case of comets, the future discoveries in astronomy will belong to faint and delicate objects; but these are interesting, and should not be neglected. A uniform temperature, which secures good definition, and steady images of the stars, is necessary for accurate determinations of position, and for all measurements of precision. This condition is especially important in such work as that of stellar parallax, the determination of the constant of aberration, and wherever the yearly change of temperature may act injuriously. In the selection of better sites for observatories I think we have an easy means of advancing astronomy.

As this science grows and expands, it will become more and more necessary to study the economy of its work, in order that astronomers may bestow their labours in the most advantageous methods, and may rid themselves of all cumbersome and time-consuming processes. The manner of publishing observations has already been much abbreviated, and improved I think, by some of the European astronomers, and this change seems destined to become universal. As the positions of many objects are now well known, the need of printing all the details of the observation, such as the transits of the wires, the readings of the micrometers, &c., is very slight; and this printing may be safely abandoned. Even this change will lead to a great saving in the time and cost of printing. But this will necessitate a more complete discussion of the work, and a more careful examination of the instruments; things to be desired, since they tend to lift the observer out of his routine, and make him a master of his business. There are objections to this change, and some of them are real, such as the importance of publishing a complete record; but this is overestimated, I think, since the original records ought always to be referred to in case of doubt; and other objections are factitious, such as the need of publishing a large and showy book in order to impose on the public.

We may hope also for improvements in theoretical astronomy, and for the better training and preparation of students of this science. I know that it is sometimes said that theoretical astronomy is finished, and that nothing more can be done. Such assertions come from professors who are old and weary, or from those young men who tire out early in life; but they are wrong. The improvements that Hansen has made in the theory of perturbations, and Poinsot's study of the theory of rotation, show what careful investigation may do, and assure us of further progress. It must be confessed that some of the astronomical work done in our country bears evidence that the astronomers did not understand the correct methods of reduction, and much of it shows evidence of hasty and ill-considered plans. This is perhaps a natural condition for beginners, but we trust that it has been outgrown. An actual need for the astronomical students of our country is a good book on theoretical astronomy, similar to Pontécoulant's work, in which the whole subject shall be presented in a complete form, such as we find in the "Mécanique Céleste," together with an account of the improvements made by Gauss, Poisson, Hansen, and others. There is no American book of this kind, and the English works are too partial, designed apparently to fit the student for college examinations, and not to give him a complete knowledge of the science. Such a book has hardly been attempted in our language, unless that of Woodhouse may be an exception, and it may be a long time in coming, since it requires a man qualified to do the work, and will involve an expense of labour in the preparation and of cost in publishing such as few are willing to incur. In the mean time it is far better for the student to go directly to the writings of Lagrange and Laplace, of Gauss and Poisson and other masters, rather than to spend time in reading second-rate authors who endeavour to explain them. And generally this will be found the easier way also, since the student avoids the confused notions and symbols, and the grotesque expressions and egotism of small men, and is lifted into the region of ideas and invention.

In presenting his exposition of the nebular hypothesis, which has since become so celebrated, Laplace says: "I present this hypothesis with the distrust which everything ought to inspire that is not a result of observation or of calculation." It is a singular fact that, among all the writings on the nebular hypothesis, I have never seen a reference to this presentation of it by its most distinguished advocate; and yet this is the true spirit of scientific astronomy. Laplace did not wish to exempt his own theories from criticism, and neither should any one. In astronomy there is no final human authority, no synod or council, but simply an appeal to reason and observation. If a theory or a discovery be true, it will stand the test of observation and of calculation; if false, it must pass away to that Miltonian limbo where so many things have gone and are going. The question is sometimes asked, Of what use is astronomy? and the reply generally made is that it has conferred great benefits on navigation and on commerce, since it is by means of his astronomical knowledge that the sailor determines the position of his ship on the ocean. There is a truth in this reply, but it is only partial. The great value of astronomy is that it is really a science and that it has broken the path and led the way through which all branches of science must pass if they ever become scientific. It is the spirit of honest, unrelenting criticism, and of impartial examination, that finally eliminates error and awards to every one his just due, that makes astronomy honourable and attractive; and it is by cultivating this spirit that astronomy confers its chief benefit, for it is this that shall break in pieces and destroy all false assumptions in science and in philosophy.

SCIENCE IN NORWAY

We have received several publications from Norway of scientific interest.

Nyt Magazin for Naturvidenskaberne. B. 25, H. 4. (Kristiania, 1880.)

In this number of the Norwegian "New Magazine for Natural Sciences" Herr Hansen continues his description of the annelids yielded by the Norwegian North Sea Expedition of 1878, to which he appends drawings of his own of all the rarer forms.—Herr L. Schmelck gives the results of his analysis of sea-water obtained in the same voyage within and near the Polar circle. The water was taken at various depths, and was obtained from a stratum intermediate between the surface and the bottom by means of an apparatus devised for the purpose by Herr Tornöe.—Herr Brögger and Reusch's observations on the character and localities of Norwegian apatite, which originally appeared in 1875 in the *Zeitschrift d. deutschen geologischen Gesellschaft*, are here translated into Norwegian by the authors, who have made various additions to their paper, which is illustrated with numerous drawings.—In a paper on the Lepidoptera of Norway by W. M. Schöyen the author draws attention to the number of new forms added to this branch of the Norwegian fauna since the publication, in 1876, of Siebke's list of the insects of Norway. The number given at that time for the lepidoptera was 934; it is now raised to 1,019. The writer's own contribution to these is 38 hitherto undetected Norwegian species, the habitats and characters of which he describes.

Kort Fremstilling af de Norske Kursteders Udvikling, &c. Ved Axel Lund, M.D. (Kristiania, 1880.)—In this *brochure* we are reminded that Norway, from her geognostic character, is naturally deficient in thermic springs, and we are shown that till recently the *water-cure*—taking the words in a comprehensive sense to include the use of waters internally and externally—was unknown in the country. In fact even now the Norwegians stand exceptionally low in the scale of water-using nations, although an encouraging change in this respect has been manifested of late years by the establishment of mineral and sea-bathing places in various parts of the kingdom. These Dr. Lund describes at great length, giving the analysis of the waters yielded by the few springs that have been opened, and the amount of salt present in the sea-water at the various marine stations, with the medical reports of each and the mode of treatment adopted. In the last respect the only difference that we observe from the system generally followed at German baths is that at the sea-bathing establishment in the Sandefjord, a small species of Medusa is used to excite local irritation in cases of cerebro-spinal, rheumatic, or neuralgic affections, by passing the animals rapidly over the parts affected. To Dr. Thaulow, the founder of the baths at the Sandefjord and at Modum, the Norwegians owe a large debt of gratitude as the first of their countrymen who drew public attention to the paramount importance of baths as a hygienic agent. Sweden has long been in advance of Norway in its appreciative comprehension of the curative value of mineral and sea-waters; and from Dr. Lund the reader will learn all that there is to learn in regard to the water-establishments, springs, and baths of the sister kingdom, while he may also gather some information respecting similar institutions in the Danish dominions. In conclusion, we may observe that some of the newly-opened Norwegian water-cure establishments, as that of Modum, lying in the midst of pine-woods, and the sea-bathing places on the Sandefjord and Kristianiafjord, offer numerous attractions to foreigners in respect to salubrity of air, beauty of position, and moderate cost of living.

Knudshö, eller Fjeldfloraen. J. B. Barth. (Kristiania, 1880.)—Herr Barth, who is well known for his animated descriptions of the natural scenery of his country, and his lively narrative of the adventures of a sportsman in Norway, supplies us in the present sketch with a comprehensive *résumé* of the flora of the Fjelds. The spot he has chosen for his point of observation is the double-topped hill Knudshö, near Kongsvold, well known to the botanists of other countries, as well as to those of Norway, for its exceptionally rich and varied Alpine flora. Here may be gathered the rare *Artemisia norvegica;* the gentians, *nivalis* and *glacialis;* some of the less common saxifrages, numerous species of *Carex* and *Salix;* and some Alpine forms, as *Kobresia caricina*, *Chamærópes alpina*, which are not found elsewhere so low down; while here, too, the collector will find close at hand a number of sub-Alpine and south-Norwegian plants of rare occurrence in other parts of the country. Herr Barth, himself an enthusiastic botanist and a practised collector, never fails to give the local and ordinary Norwegian name of the plant he describes, in addition to its scientific designation; and thus supplies foreigners with very valuable and much-needed information, the want of which often proves to be a matter of great inconvenience in studying the flora of a foreign country.

Om Grantörken og Barkbillen. J. B. Barth.—In this little pamphlet the author, who is one of the first authorities in Norway on questions of forestry and arboriculture generally, explains his reasons for differing from the opinion commonly received, that the desiccation and ultimate death of the Norwegian spruce (*Abies excelsa*) are due to the attacks of *Tomicus typographus* (*Bostrychus typographus*), which is usually regarded as the most pernicious of all the insect-enemies of the Coniferæ. Herr Barth does not dispute the fact that this beetle is to be found often in large numbers on trees affected by abnormal drying up, whether still standing or cut down; but, in his opinion, although disease in the tree may be the cause, it is not the result of the presence of the Tomicus, which he believes to have absolutely no effect on the condition of the bark. According to this view the numerous agents employed in Germany and elsewhere to eradicate this beetle have no result but waste of labour and money; the only remedy against the drying up of the bark being a more scientific mode of clearing forests, in which the trees often perish either through overcrowding, or more frequently through reckless felling, by which cold blasts are allowed to fall directly on the interior. Herr Barth's views are in opposition to those of the majority of the working foresters of Germany and Scandinavia, but his extensive acquaintance with home and foreign forests, his great practical experience, and his reputation as a naturalist, entitle them to all possible respect, although it is not to be supposed that his plea for the innocuousness of the *Bostrychus typographus* will be admitted without much sifting of the evidence, seeing that this insect is generally believed by German foresters to have been the cause of the destruction of the forests of the Harz Mountains, when between 1780 and 1790 two million trees died of desiccation.

SPECTROSCOPIC INVESTIGATIONS[1]

As I have stated in my former communication,[2] all chemically related elements exhibit a homology of spectra, the various spectra of the elements of a group differing solely in the manner in which their groups of lines are shifted towards one end or other of the spectrum.

In a comparative investigation on the alkaline earths, I have arrived at conclusions which may explain these remarkable analogies in chemically-related elements.

I have now the honour of laying before the Academy a brief account of my investigations; on another occasion I shall report on this subject more fully.

If the spectra of the alkaline earths are produced by a jar-spark in a hydrogen-atmosphere,[3] spectra are obtained which show the homology of the spectral lines very beautifully. The spectrum of magnesium cannot be compared with the spectra obtained in this manner, because it does not contain the less refrangible lines. However, without the jar, or employing a smaller battery and a smaller induction-coil, it appears that in the spectra of calcium and strontium all lines in the red and yellow disappear, and the spectra which become visible are remarkably similar to those of magnesium.

[1] By G. Ciamician, in *Sitz. Ber. der k. Akad. der Wiss.*, Vienna, Vol. lxxiv. Heft i.

[2] "Ueber die Spectren der chemischen Elemente und ihrer Verbindungen" (vol. lxxvi. chapter ii., October, 1877). "Ueber den Einfluss der Dichte und der Temperatur auf die Spectren von Dämpfen und Gasen." (vol. lxxviii. chapter ii., October, 1878.)

[3] With four of six medium Bunsen's elements and a great Gaiffe's induction coil giving a spark of 10 centimetres.

Comparing the less refrangible part of the spectrum of the alkaline earths, which are only rendered visible by increased temperature with the less refrangible half of the entire oxygen spectrum, we find the remarkable fact that these two halves of the spectra show a decided resemblance or homology. From this we may conclude that *the spectrum of the groups of the earth-alkali metals is composed of the spectrum of magnesium and of that of the less refrangible parts of oxygen.*

In order to determine the real importance of these remarkable analogies—it being known that the atomic weights of baryum, strontium, and calcium are capable of being composed of the atomic weights of magnesium and oxygen[1]—I found it necessary to analyse the spectra of combinations, which are not saturated, but behave as compound radicals, and thus most resemble in chemical behaviour the simple radicals or elements.

Hitherto I have analysed cyanogen and carbonic oxide. The cyanogen spectrum comprises two portions, one of which is the homologue of the nitrogen spectrum, the other the homologue of the less refrangible part of the carbon spectrum.

Also in the carbonic oxide spectrum there are present the well-known groups of carbon appearing as bands, and displaced in the red field there are several lines homologous to those of oxygen. Therefore the same relation exists between the spectra of nitrogen and carbon, and between the spectra of cyanogen, carbon, and oxygen, and carbon oxide spectrum, which prevails between the spectra of magnesium and oxygen and the spectra of the earth-alkali metals.

One can go further and say that in general the homology of the spectral lines of chemically-related elements is in all probability based upon the circumstance that the elements of such natural groups conform to the laws of Mendelejeff on atom-weights, and consist of identical components.

[1] It is, namely, $24 + 16 = 40$ (calcium), $24 + 4 \times 16 = 88$ (strontium), and $24 + 17 \times 16 = 136$ (barium).

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Science Lectures at Cambridge this term include Prof. Liveing on the General Principles of Chemistry, and on Spectroscopic Analysis; and Prof. Dewar on Physical Chemistry. One of the demonstrators will give demonstrations in Volumetric Analysis; and Mr. Scott, assistant to Prof. Dewar, will give a course of demonstrations in Elementary Organic Chemistry. For permssion to carry out special investigations in the University laboratories application should be made to one of the Professors.

Lord Rayleigh will lecture on Galvanic Electricity and Electro-Magnetism in the Cavendish Laboratory; Dr. Schuster will lecture weekly on Radiation; Mr. Glazebrook will give an elementary course of demonstrations in Electricity and Magnetism; and Mr. Shaw will give demonstrations on the Principles of Measurement and the Physical Properties of Bodies. Courses of demonstrations are announced for the Lent Term on Heat and Advanced Electricity and Magnetism; and for the Easter Term on Light, Elasticity, and Sound.

Mr. W. J. Lewis will lecture on the Silicates, in the Mineralogical Lecture-room.

Mr. F. M. Balfour will give elementary and advanced courses on the Morphology of Invertebrata, with practical work. Prof. Humphry will lecture on the Osseous System; Prof. Hughes on the Principles of Geology, with Field Lectures; Prof. Latham on Therapeutics; Prof. Newton on Invertebrata; Prof. Stuart on Mechanism.

It is to be hoped that something may be done this term to relieve science students in the matter of Greek, and to encourage French and German studies, for want of which there is so much hindrance to science, as well as literature. The Sedgwick Geological Museum, with money accumulating, must still wait, we suppose. Will the Museum be ready for 1900?

Mr. Sedley Taylor will lecture on the Acoustics of Music in the Cavendish Laboratory.

SCIENTIFIC SERIALS

Bulletins de la Société d'Anthropologie de Paris, tome iii. fasc. 2 (1880).—M. Robin, Inspecteur primaire du Département de Loir-et-Cher, has laid before the Society his scheme for obtaining important anthropological measurements by the help of teachers of schools. The proposed questions, with a full description of the various appliances by which such measurements could be taken, have been submitted to the consideration of a special commission.—M. J. Parrot's paper on the development of the brain in infants, considers the subject chiefly in reference to the modifications of colour which the medullary substance undergoes. —The present number of these *Bulletins* gives M. P. Broca's remarks on his "goniomètre flexible," of the various parts of which drawings are appended.—M. Harmand makes the interesting communication that some Cambodian inscriptions, hitherto undeciphered, have been found by Prof. Kern, of Leyden, to be Sanskrit, written in Kawi and Kalinga characters.—M. Vinson suggested that fixed rules should be drawn up for the transcription of foreign words, and should form part of the official anthropological instructions provided for travellers and explorers in savage countries. His suggestion has been accepted. —In addition to the article already referred to on the flexible goniometer, these *Bulletins* contain several papers from the pen of the late M. Paul Broca, which will be read with the more interest as being among the last of his communications to the Society; these are his post-mortem reports of the appearances presented in the thorax of a young Zulu girl, with his remarks on a retrogressive anomaly in the aorta of this girl; a description of the appearances of the cranium of the assassin Prévost, more especially with reference to the assumed importance of the protuberance between the occipital and parietal, to which Gratiolet applies the term *calotte*, and which he regards as a simian character. M. Broca considered that in the interests of physical science it would be desirable that greater facilities should be afforded to scientific men for obtaining the heads of those who die in public prisons, asylums, &c. Finally we have the report of M. Broca's remarks on the case of an illiterate boy of eleven, possessed of extraordinary powers of calculation, and evincing surprising facility in extracting cube-roots. The consideration of this case gave additional interest to the discussion that had been raised at an earlier meeting, in regard to Galton's observations on the vision of serial numbers.—M. Moudière has drawn up a monograph on the women of Cochin-china, in which he has embodied the results of six years' laborious anthropological researches. The three races of Annamites, Cambodians, and Chinese, of which the Cochin-china population is composed, were severally studied.—M. Bertillon gives the results of his comparative analysis of the statistical tables of suicides for France and Sweden. The results show singular accord between the two countries, and the author considers himself justified in maintaining that they establish the two following laws:—1. That widowers commit suicide more frequently than married men. 2. That the existence and presence in the house of children diminishes the inclination to suicide both in men and women.—M. René de Semallé gives a comparative table of the mean length of the generations of mankind, based on the genealogy of the reigning and other princely families in Europe. From these it would seem that the period of thirty years, which in common parlance is accepted as that of a generation, very closely corresponds with the means obtained from these genealogical data.—M. Fourdrignier gives the result of his exploration of the double tumuli found at Thuizy, near Rheims, among a large number of other graves in which only one individual had been interred. Where these graves have escaped earlier spoliation, the human remains and the broken fragments of ornaments found in them would appear to show that the individuals buried together were of different sex. M. Fourdrignier has made an interesting discovery of the several parts of two conical casques. The fragments of these singular head-coverings were extracted from two of the double graves, and, according to their discoverer, they belong to a Gallic race of the pre-Roman period, and must in form have closely resembled the modern German "Pickelhaube."

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, October 4.—M. Wurtz in the chair. M. Perrier presented a *Compte rendu* of the determinations of longitudes, latitudes, and azimuths in Africa under his direction, at Géryville, Laghouat, Biskra, and Carthage in 1877 and 1878, with a description of instruments and methods. In the exchange of signals it was possible to calculate the mean retardation of transmission of a signal along an aërial conductor, from chronograph to chronograph, for distances comprised between 414 km. and 1,236 km. The mean velocity of propagation was found about 40,000 km. At this rate an electric signal would go round the earth in a second.—Military and geographical exploration of the region comprised between the Upper Senegal and the Niger, by M. Perrier. A Government expedition under Commandant Desbordes was to start on the 5th, Commandant Derrien having charge of the topographical department. They go to St. Louis, and make their way to Bafoulabé, at the confluence of the Bafing and the Bakhoy. Here they construct their first fort, and organise escorts and convoy, with a view to a general triangulation of the region between Bafoulabé on the Senegal, and Dina and Bamakou on the Niger. The railway contemplated would run from Medina, by Bafoulabé and Fangalla, to the Niger.—Order of appearance of the first vessels in the spike of *Lepturus subulatus*, by M. Trécul.—M. de Lesseps presented the "Bimensual bulletin of the Inter-oceanic Canal" for September.—On utilisation of the crystals of lead-chambers, by MM. Girard and Pabst. The crystals offer an abundant and economical source of nitrous acid, and the authors have been able to prepare on a large scale, the dinitric bodies, amidoazobenzol and nitroalizarine, by making the nitroso-sulphuric acid act on the corresponding amidised derivatives, or aniline and alizarine. But the crystals can only be employed in presence of a quantity of sulphuric or nitric acid (preferably the former) sufficient to prevent their decomposition by water.—Observations of Faye's comet made at the Observatory of Florence-Arcetri, by M. Tempel.—On some thermometric questions, by M. Crafts. It is very probable that the least change of volume of a thermometer is accompanied by a change of the coefficient of dilatation.—On the decomposition of salts by liquids, by M. Ditte. The laws of dissociation by heat which apply to decomposition of salts by pure water and by saline or acid solutions, apply also to decomposition by alcohols, and probably in general to decompositions of salts by the wet way, whatever the solvent.—On the physiological action of *Conium maculatum*, by M. Bochefontaine. Conine diminishes or abolishes the physiological properties of the nervous centres before acting like curare on the "nervo-muscular junctive substance" (Vulpian). In the dog and frog it at length abolishes the nervous excito-motricity if given in sufficient quantity, and it is fatal for batrachians as well as for mammalia. Hemlock then may act like curare, but it has additional physiological effects.—Floral dimorphism and staminal petalody observed in *Convolvulus arvensis*, L.; artificial production of this latter monstrosity, by M. Heckel. Petalody is the effect of direct fertilisation long continued. The autogamic process in plants as in animals (but in a longer period with the former) has the result of altering the organs of reproduction and leading to absolute infertility.

CONTENTS

PAGE
THE INDIAN FAMINE COMMISSION 553
GAMGEE'S "PHYSIOLOGICAL CHEMISTRY." By Dr. M. FOSTER, F.R.S. 555
PEAT-MOSSES 556
OUR BOOK SHELF:—
Lunn's "Vox Populi" 556
Angel's "Practical Plane Geometry and Projection for Science Classes, Schools, and Colleges" 557
Betti's "Teorica delle Forze Newtoniane e sui Applicazioni all' Elettrostattica e al Magnetismo" 557
Schubert's "Kalkül der Abzählenden Geometrie" 557
LETTERS TO THE EDITOR:—
The Spectrum of Hartwig's Comet.—W. H. M. CHRISTIE . . . 557
Wire Torsion.—Major J. HERSCHEL, F.R.S. 557
The Magnetic Storm.—G. M. WHIPPLE 558
Coral Reefs and Islands.—Prof. JOSEPH LECONTE 558
Geological Climates.—Dr. P. MARTIN DUNCAN, F.R.S. 559
The Yang-tse, the Yellow River, and the Pei-ho.—T. MELLARD READE 559
Miller's Elements of Chemistry—Part III. Organic Chemistry.—Prof. HENRY E. ARMSTRONG, F.R.S. 559
Swiss Châlets.—H. N. MOSELEY 559
Spectra of the Brocken at Home.—J. INNES ROGERS 559
Ice under Pressure.—C. A. M. 559
Mr. Haddon's Marine-Zoology Class.—ALFRED C. HADDON . . 560
Landslips.—THOS. WARD 560
LIQUEFACTION OF OZONE 560
THE UNIVERSITY OF NEW ZEALAND 560
DOCTORED WINES 561
MULTIPLE SPECTRA, III. By J. NORMAN LOCKYER, F.R.S. (*With Illustrations*) 562
WILLIAM LASSELL, LL.D., F.R.S. By Dr. WILLIAM HUGGINS, F.R.S. 565
NOTES 567
OUR ASTRONOMICAL COLUMN:—
Hartwig's Comet 569
GEOGRAPHICAL NOTES 569
PROF. ASAPH HALL ON THE PROGRESS OF ASTRONOMY 570
SCIENCE IN NORWAY 574
SPECTROSCOPIC INVESTIGATIONS. By G. CIAMICIAN 575
UNIVERSITY AND EDUCATIONAL INTELLIGENCE 575
SCIENTIFIC SERIALS 575
SOCIETIES AND ACADEMIES 576