THURSDAY, JULY 30, 1874

JOSEPH PRIESTLEY

DURING the present week the centenary of the birth of Modern Chemistry, as the discovery of oxygen on August 1, 1774, may justly be called, is being celebrated both in this country at Birmingham and Leeds, and in America at Northumberland, Pennsylvania; we have therefore thought it would be acceptable to our readers to be reminded of the principal events in the life of the author of this all-important discovery.

Joseph Priestley was born on March 13, 1733, at Fieldhead, near Leeds. At the age of six years he lost his mother, and his education was superintended by Mrs. Keighley, his aunt, a woman apparently of unusually wide sympathies. At an early age young Priestley distinguished himself at school by his great aptitude for learning languages; he was familiar with Chaldean, Syriac, and Arabic, and without the aid of a master acquired some knowledge of German, French, and Italian. A pupil of Maclaurin taught him mathematics. He took great interest in theological controversies, and his aunt's tastes provided him with many opportunities of gratifying his liking in this matter. Having studied for the Dissenting ministry, he was called to be minister of a small Unitarian congregation at Needham Market, in Suffolk, in 1755. Here he remained until 1758, when he went to occupy a similar post at Nantwich, in Cheshire. Here he opened a school, and by dint of rigid economy was able to buy some physical apparatus, with which he made, to his young pupils, a series of experiments that drew upon him the notice of the authorities of the Warrington Academy, so well known in connection with the name of Aikin. In 1761 he went to this Academy to take Dr. Aikin's place as teacher of languages and literature, and soon after married the daughter of a Welsh ironmaster. While at Warrington he published a number of works on various subjects, including the "Theory of Language and Universal Language" (1762-68), "Essay on a Course of Liberal Education for Civil and Active Life" (1765), "Chart of Biography" (1765), Chart of History" (1769), &c. A visit which he made to London during this period gave him the opportunity of forming a lasting friendship with Franklin and Price. He communicated to the former his intention of writing a history of discoveries in the department of electricity; and not only did he receive from Franklin a warm approval of the scheme, but also all the books and memoirs he required; and before the end of the year, by dint of persevering work, the first volume was published, under the title of "The History of Electricity" (London, 1764, 4to). Three editions of this were published by 1775; but it bears evident marks of having been written in haste.

Previous to the publication of this work, in 1766, Priestley was chosen a Fellow of the Royal Society, and about the same time the University of Edinburgh conferred upon him the honorary degree of LL.D. In the same year as the above-mentioned work was published, Priestley left Warrington and became pastor of Mill-hill Chapel, in Leeds. While here he was much occupied with theological controversies, but by no means neglected his scientific studies, as about 1768 his attention was drawn to chemistry, the result being that in 1772 he communicated to the Royal Society a paper entitled "Observations on different kinds of Air," for which the Copley Medal was awarded to him.

Meantime, Priestley had received an offer to accompany Capt. Cook on his second expedition to the South Seas; this he accepted gladly, but received an intimation that his nomination had not been confirmed by the Board of Longitude on account of his advanced theological opinions. In 1773, however, at the recommendation of his friend Price, he was appointed librarian to the Earl of Shelburne (afterwards Marquis of Lansdowne) at a comparatively liberal salary. In the following year, he accompanied this nobleman into France, Germany, and the Low Countries. At Paris his scientific reputation easily procured him the acquaintanceship of well-known men of science. Besides his salary, Lord Shelburne allowed him expenses for a laboratory, and it was on Aug. 1, 1774, that he made the discovery which marks so important an epoch in the progress of chemical science, and the centenary of which is being celebrated both in England and in America during the present week. The discovery was that of oxygen gas, which he announced in his "Experiments and Observations on Air," the first volume of which was published in 1774.

For some unexplained reason, Priestley and Lord Shelburne parted in 1780, the latter covenanting to allow the former till his death a pension of 150*l.* Priestley then settled in Birmingham, to which he was attracted, no doubt, by the prospect of meeting with men of kindred scientific tastes. Here he was chosen pastor of one of the principal Dissenting churches, his friends subscribing to defray the expenses of his scientific experiments and his theological controversies, for he was regarded as one of the greatest controversialists of his age. His opinions both on ecclesiastical and political topics were much ahead of his age; but this is not the place to enlarge on this aspect of the character of this remarkable man. We may only mention that he was brought forward as a candidate for the French National Convention, and was nominated a French citizen, a title of which he was very proud. For his unconcealed liberality and advanced opinions he was doomed, however, to suffer, as the populace of Birmingham, roused to a state of blind fury by the partisans of Government, rushed to Priestley's house, July 14, 1791, and set fire to it, reducing it and nearly all it contained to ashes. However, as the result of an examination, Priestley subsequently received an indemnity of 2,000*l.* for this mad act, this sum being considerably increased by the liberality of his private friends.

Although no word of complaint escaped Priestley concerning this misfortune, it no doubt influenced him to a considerable extent in deciding him to quit his native land for republican America. After spending three years in a college at Hackney, as Professor of Chemistry and minister, he embarked on April 7, 1794, and fixed his residence at Northumberland, in Pennsylvania. Even here it was some time before he was allowed to remain at peace, as a spiteful rumour had been circulated that he was a secret agent of the French Republic. Here he lost his wife and his youngest son, and here he himself died on Feb. 6, 1804.

Turning now from the external aspects of Priestley's life, let us consider the position he held as a philosopher and the influence that his discoveries had on the science of his time. The ever-memorable discovery of "dephlogisticated air" on Aug. 1, 1774, marks an epoch in the annals of chemistry with which the name of Dr. Joseph Priestley will be always associated. He obtained it by exposing a quantity of red precipitate of mercury to the action of the sun's rays concentrated upon it by a lens; the red precipitate was contained in a flask filled up with mercury and inserted in a basin containing the same metal. "I presently found," he says, "that by means of this lens air was expelled from it very readily. Having got several times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it; but what surprised me more than I can well express, was that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous air exposed to iron or lead of sulphur; but, as I got nothing like this remarkable appearance from any kind of air besides this particular modification of nitrous air, and I knew no nitrous acid was used in the preparation of *mercurius calcinatus*, I was utterly at a loss how to account for it." He then goes on to show that red lead and nitre also afford oxygen at a red heat, and calls it, consistently with the theory of combustion which was then prevalent, *dephlogisticated air*, regarding it as common air deprived of phlogiston, and consequently possessed of a powerful affinity for that imaginary principle.

This discovery, however, though unquestionably brilliant, must not be allowed to eclipse those other numerous and valuable contributions to science with which this indefatigable worker enriched the stores of natural knowledge during a period ranging from 1768 to 1800. There are indeed few branches of natural science which did not reap some benefit, direct or indirect, from the discoveries of the experimenter whose memory we now recall.

On the 17th of August, 1771, Priestley enclosed a sprig of mint in air in which a taper had been allowed to burn out, and he found on the 27th of the month that the same air then permitted the combustion of another taper with perfect facility. Thus was the secret of vegetable respiration first made known. In the discoverer's own words: "This restoration of air I found depended upon the vegetating state of the plant; for though I kept a great number of the fresh leaves of mint in a small quantity of air in which candles had burned out, and changed them frequently for a long space of time, I could perceive no melioration in the state of the air." In pneumatic chemistry (of which the germs had been originated by Black, Mayow, Hooke, and Hales), Priestley found a new engine of research, and in his hands this Οργανον yielded vast results. His productions in pure chemistry are too well known to be discussed fully here, even did space permit. In addition to oxygen he discovered nitrous oxide (1776), sulphurous anhydride (1774), ammonia gas (1774), carbonic oxide and hydrochloric acid gas (1772): he was also the first to investigate the properties of nitric oxide. We may point to nitrous oxide *en passant* as one of the many instances in which pure science has furnished a substance of practical utility to man: the discoverer of "dephlogisticated nitrous air" little dreamt that the lapse of a century would see this substance used as an anæsthetic for the purposes of dentistry. The pneumatic and mercurial troughs, now indispensable parts of our laboratory "plant," were also bequeathed to us by the philosopher of Fieldhead. Although chemistry received the greater part of Priestley's attention, other branches of science, as before stated, received the benefit of his thoughts. Thus we find a work by him bearing the date 1772, entitled "The History and Present State of Discoveries relating to Vision, Light, and Colours," and we have already referred to his "History of Electricity." From a catalogue of Priestley's works, printed at the end ot his "Experiments and Observations relating to various branches of Natural Philosophy," we find that this extraordinary man was the author of no less than thirty-six volumes on various subjects; among others, the theory and practice of perspective, charts of history and biography, rudiments of grammar, observations on education, a course of lectures on oratory and criticism, an essay on the first principles of government, and on the nature of political, civil, and religious liberty, together with large numbers of works on metaphysical subjects and on theology.

But it is with the *chemical* aspect of Priestley's life that we are more particularly concerned at present. The anniversary about to be celebrated is that of a purely chemical discovery, and one which to us appears doubly important, first, from the great flood of light which it shed on the processes of combustion and of respiration, both animal and vegetable, aërial and aquatic; and secondly, from the powerful illustration which it affords of the value of a new method in scientific investigation. The purely practical results which in after years flowed from the discovery of oxygen, such, for example, as the oxy-hydrogen blowpipe, which enables large quantities of platinum and of the most refractory metals to be smelted with ease, are at present of minor interest. Is it not this over-anxious regard for "practical results" that has led to the complaints, too frequently made, about the decline of chemical research in England? The spirit of the old investigators of the school of Priestley, Cavendish, and Black seems to be forsaking us, and, with certain exceptions, our most efficient workers are devoting their time and energies to effecting permutations and combinations among the elements—in seeing in how many ways certain atoms of carbon, hydrogen, and oxygen can be combined, or in locating atoms to certain imaginary positions in space. It must not be for a moment supposed that we advocate the entire cessation of this kind of work—it is useful in its way as supplying facts, but by itself it is not sufficient to lead us to hope for any great advancement in our knowledge of chemical laws. The greatest advancements in chemistry have been the results of the application of *physical* discoveries—witness the vapour-density control for the formulæ of compounds and the atomic weights of the elementary gases; or the determination of specific heat as a means of controlling the atomic weight; or turn again to that great engine of modern research, the spectroscope, which has enabled us to extend our list of known elements, and which reduces the chemistry of this globe and of suns infinitely remote to one common basis. So also is isomorphism an essentially physical phenomenon and one for the explanation of which we shall doubtless be hereafter indebted to physics. The Newton of chemistry may be looked for in the ranks of physicists. In the meantime let us only hope for "new methods" of research—let investigators seek for some method bearing the same relation to our chemistry that the "pneumatic chemistry" of Priestley did to that of his time.

ON TESTIMONIALISM

JUST now, there must be several scientific men asking themselves what can be the conceivable value of testimonials in determining the relative fitness of a number of candidates for any appointment of such importance as a Professorship of a most important branch of natural science in a great seat of learning.

It is not a point of any great difficulty to determine, to one's mental satisfaction, in what cases testimonials are of value—for they are sometimes most useful—and when they are worthless in comparison to other methods for testing the relative efficiency of different men.

Testimonials, or an examination, or the two combined, are no doubt necessary, when the post to be competed for is one, the qualities required for which are not capable of being exhibited to an electoral body by the competitors in any other way. For minor appointments, therefore, such as clerkships, smaller educational posts and the like, they are indispensable; as they are in cases where the intimacy of the relationship between the holder of the post and those he is placed above is close. But for appointments so honourable and responsible as the Professorship of Physiology in the University of Edinburgh, or that of Chemistry in the University of Glasgow, we cannot help thinking that testimonials are a farce. Candidates for such chairs are not youths; they must have had the opportunity of maturing their minds by careful training, during which time frequent opportunities must have occurred for them to take up some fresh branch of their subject and work it out independently, with some originality in the methods they employ. Their confidence in their methods and results ought to have been sufficient to make them publish them, and so expose them to the criticism of the scientific public, who do not generally take long to form a fairly correct estimate of the abilities of authors. If all candidates for important posts were compelled to rely for their election on their works alone as testimonials, we are sure that the electors would be less trammelled, and more in a position to make judicious selections.

By some it may be remarked that what is wanted in the cases above instanced is good teachers, and that if men with original power can be obtained at the same time, so much the better; this requirement makes the general ability of the professor a secondary consideration in comparison with his teaching powers. We are of opinion that this is a mistaken view of the subject. Very frequently the most talented followers of scientific inquiry are not such effective lecturers at first sight as their less-gifted colleagues; still, we never knew a case in which there was not a peculiar charm about the teaching of a master-mind that gives an impulse to study on the part of the student, producing in the long run more beneficial results than the routine discourses of a mere expositor of other people's work. Another thing is that the connection of great names with a seat of learning in itself gives a stimulus to younger workers, raising success in mental work to a position which it is not easy for it to attain, on account of the fact that its results have frequently no immediate practical bearing.

In one at least of the cases we are referring to it is unfortunate in some respects that the electors have no special interest in the science they have so great a power indirectly to advance. In consequence of this their knowledge of the respective merits of the candidates must be uncertain, and we do not think that it will be much increased by the showers of testimonials which it is evidently the intention of more than one of the candidates to submit. One candidate has sent broadcast a lithographed form, sometimes even to men his junior in position and age, courting testimonials. What possibly can be the value of the pound's weight of paper he will probably thus accumulate? He ought to remember that no number of shots from a smooth-bore gun will send a ball as far as a single one from an Armstrong, and on that principle reduce the number and endeavour to increase the quality of the testimonials he sends in; by which means he will save the adjectives as well as the temper of his acquaintances.

Another candidate sends us the printed list of his published works, and to that we see no particular objection. But appended to each is a selected series of reviews, from which all the unfavourable ones are carefully omitted. It is, no doubt, unpleasant to print adverse criticism, but how can the electors be expected to form a correct estimate of the value of the works reviewed, if those in their favour only are introduced? The reviews, as one-sided, had been much better omitted, or, if printed, had much better have been inserted without selection. It is this extreme mode of action thus adopted which has called our attention to the subject.

On the whole, we think that the electors for the Scotch Science Chairs have a by no means easy task before them, and we sincerely hope that in their selection they will lay stress on soundness of judgment and scientific thought rather than on quires of testimonials wrung out of acquaintances and friends, who would much rather have been otherwise employed than in putting pen to paper for the purpose.

Moreover, we are of opinion that not only should a man's researches be taken into account in making an appointment to any science chair, but also that no election should be made without taking the opinion of those competent to form an estimate of the value of these researches.

THE RAINFALL OF BARBADOS

Report upon the Rainfall of Barbados, and upon its influence on the Sugar Crops, 1847-1871. With two Supplements, 1873-74. By Governor Rawson, C.B.

THIS Report gives the result of observations made since 1847, at a large number of stations well distributed over the island. The total area of Barbados is 166 square miles; in 1847, only three stations had been established, in 1873 there were 178, so that at present there is more than one gauge to every square mile. By this system the conditions of local rainfall have been, as it were, put under the microscope; and the store of information obtained after the suggestive manner in which it has been analysed in this Report, will be not only valuable to the sugar-planter, but interesting to the meteorologist.

The north-east trade-wind prevails at Barbados during three-fourths of the year, and most of the rain comes from that quarter. Heavy showers come at certain seasons from south-west and north-west, but generally fail to reach the eastern districts. Indeed it very rarely happens that rain falls at the same time, or in equal proportions, over the whole island; it has, therefore, been divided into two main districts, the windward, and chiefly highland, and the leeward, or lowland section.

With regard to the yearly rainfall of the whole island from 1847 to 1871, it has been found, among other results (1), that the rainfall of fifteen years was above the mean, that of ten years below it; (2) that the deficiencies were generally greater than the excesses above the mean, that is, droughts, when they happen, are heavy; (3) that, taking the thirty years 1843-72, no succession of wet or dry years in cycles can be traced, but rather an alternation of wet and dry years. No more than two dry years have occurred together, but as many as three and four wet years.

With regard to the monthly rainfall: the mean of all the months is under 5 in.; March is driest, October wettest. In wet years May contributes most to the excess, March least. March is the only month of which the mean rainfall in dry years has exceeded the average. In dry years the deficiency is generally spread over the whole year; in wet years the excess is generally confined to the rainy season (autumn). On the other hand, taking the seven wettest and the seven driest years of the period, we observe that in the wet years two-thirds of the excess proceeds from heavy rains in the dry season, and that in the dry years more than two-thirds of the deficiency is caused by three out of the same four months, viz. June, July, and May, and by October and May.

A comparison between the three highest and the three lowest rainfalls belonging to each month during the whole period of twenty-five years shows a remarkable uniformity of the relation between the percentages of the extremes; thus the difference between each average of the lowest months and each average of the highest amounts in none of the twelve months to more than 90 (May) or less than 65 per cent. (August and September).

Of the two stations, Binfield and Halton, lying respectively at 1,065 and 280 ft. above the sea, the former received on an average nearly 11 in. more rain in the year, and showed a greater monthly variability. The influence of elevation is interesting. A table of rainfall in 1870 and 1871, at various heights from 100 ft. up to 1,000 ft., shows an increase at every step of 100 ft. but one, and the total increase at 1,000 ft. amounts to 20·73 in. on the mean of two years. Two exceptions to this regular increment in the means for 1871-73, in supplement No. 2, are ascribed to the lower stations catching the westerly rains, which do not penetrate far inland.

In March, one-half more rain fell at night than by day. From June to November, the days are slightly the wetter, from December to May, the nights.

One of the objects of this inquiry is to assist those who are interested in calculating the character of coming seasons. For such a purpose the annual averages of each month are taken, after eliminating the exceptional months of very great or of very slight rainfall. The original averages are not affected more than 6 per cent. by this removal. Having the ordinary limits of monthly rainfall tabulated, and observing the general appearance of the weather, every planter can form *some* conjecture whether the coming month will be wet or dry.

Appendix No. 36 shows the influence of each month according to the rainfall, upon the crop of the same year, and upon that of the following year. Thus a man might fairly bet 9 to 3 that a wet February will be followed by a bad crop, and 8 to 1, the highest odds of all, that a wet September will give a good crop next year.

A wet year is followed almost invariably by a good crop in the following year; and it is found that by multiplying the total rainfall of the preceding year by 800 and adding $7\frac{1}{2}$ per cent. if that year was a dry one, or subtracting $7\frac{1}{2}$ per cent. if that year was a wet one, the crop may be calculated in most instances within 3,000 hhds., the average yield of the island being 45,000 hhds. The good chance of predicting so nearly the total exports of Barbados for the coming year cannot fail to be of value, and further experience will no doubt reduce the probability of error. Let us hope that other States may be led to undertakings of the same kind by this example.

F. A. R. RUSSELL

OUR BOOK SHELF

Manual of British Botany, containing the Flowering Plants and Ferns arranged according to Natural Orders. By C. C. Babington, M.A., F.R.S., F.L.S. Seventh Edition; corrected throughout. (London: J. Van Voorst, 1874).

WE cordially welcome this new edition of a "Manual of British Botany" which continues to hold its ground against all its competitors. We do not propose to discuss the rival merits of Hooker's, Bentham's, and Babington's hand-books; each has specialities in which the others are wanting; and each will, no doubt, long have its advocates and admirers. A special claim to popularity as a *field-book* is advanced by the present work on the ground of its portability; and a great advantage is alleged by those who use it to be presented by the practice of placing in italics a few words in the description of each species referring to the character by which it is more readily distinguished from its nearest allies. Comparing the work with the most recent of the earlier editions which we have at hand—the 4th, published in 1856—we find that it extends only to twenty-six pages more, notwithstanding the numerous additions made since that time to British botany, of which ample account has been taken in the present edition. The only alteration made in the primary classification (comparing these two editions) is the separation of Cannabinaceæ from Urticaceæ. The number of natural orders is six more than in Hooker's "Student's Flora," notwithstanding the union of Salicaceæ, Myricaceæ, Betulaceæ, and Cupuliferæ into the somewhat artificial group of Amentiferæ. The location of individual genera has also been in some cases revised, as the removal of *Narthecium* from Juncaceæ (Babington, 4th ed.) or Liliaceæ (Hooker) to Melanthaceæ. Referring to some of the more difficult genera, in which Prof. Babington is an acknowledged authority, we find the number of species of *Rubus* increased from 41 to 45, while that of *Rosa* is reduced from 19 to 11, and of *Salix* from 32 to 29. We have never been able to understand on what principle Characeæ find a place in a work devoted to "Flowering Plants and Ferns," by the latter term being apparently meant Vascular Cryptogams. Prefixed to the work is a useful Glossary not found in the earlier editions; but the author has wisely refrained from acceding "to the wishes of some young botanists by prefixing a short Introduction to Botany." With the numerous admirable works now at their disposal, students ought to have no difficulty in making themselves acquainted with the Flora of the British Islands. A. W. B.

Eclipses, Past and Future, with General Hints for observing the Heavens. By the Rev. S. J. Johnson, M.A., F.R.A.S., Rector of Upton-Helions, Devon. (Parker, 1874).

THIS little book is a combination of two distinct treatises; one a description of past and future eclipses; the other, a catalogue of celestial objects falling within the range of such small telescopes as amateurs frequently possess. Each of these, it seems, was originally of greater bulk, and intended for separate publication, but they have now been condensed into a single small volume. This has the merit, not very common in these days, of being more than a mere compilation; the ancient eclipses, including those in the "Saxon Chronicle" (of which the author tells us no description has hitherto been published), having been approximately computed for the purpose from the tables in the "Encyclopædia Britannica;" and the notices of the planets, double stars, &c., being derived from actual observation. The book is pleasantly written, and without professing to go deeply into the subject, may well find readers among those who feel a general interest in astronomy, but have no intention of making it matter of serious or accurate study, or of going much beyond the limits of a 2½ in. telescope. It would have been improved (without departing from its sketchy character) by a little more fulness and explicitness of treatment in some places; for instance, in the description of the belts and satellites of Jupiter, and where the abbreviated symbols of the Palermo Catalogue are left unexplained. Some misprints, too, have escaped in the revision. The following extract may interest our readers:—"For those who have very large telescopes, and who are not disposed to take them to oriental climates, it would be useful to have records of the number of clear nights in different parts of the kingdom. By clear nights, let us understand nights cloudless, or nearly so, till 11 P.M., or else clear for a full hour or two. Formerly my observations were taken in South Lancashire, but since the early part of 1870 in Devonshire. In 1859, the number of nights clear, partly or throughout, was 60; in 1860, 43; in 1861 and 1862, 46 each; in 1863, 47; in 1864, 83; in 1865, 82; in 1866, 77; in 1867, 55; in 1868, 62; in 1869, 58; in 1870, 112; in 1871, 98; in 1872, 90; in 1873, 82."

The Human Eye. By W. Whalley. (London: J. & A. Churchill.)

IN this small work the author tells us that he has incorporated the substance of a lecture on the subject, together with additions in various directions. He discusses, in a popular manner, the eye in man, and adds many facts with regard to its structure in other animals. His remarks are mostly anatomical, and we are disappointed to see so little notice of many physiological phenomena connected with the power of sight, which bring out the beauty of the organ of vision in a way which can be understood by the most amateur of readers. There is a want of consecutiveness in many of the paragraphs and chapters, though as a whole the book is a very readable one. Many of the instances given are wanting in grasp; for instance it is remarked that "In some of the ichneumons or 'Pharaoh's rats,' as the Egyptians call them, in the coatimundi, which somewhat resembles the racoon, and in the mangre, the osseous orbital ring is incomplete, and in a group of minor quadrupeds, entitled the Hyracidæ, the malar, or cheek bone, constitutes a perfect orbital ring." It is well known that the orbital ring is complete in all the Quadrumana and many Ungulata, and that it is absent in most other mammals; why then take the particular examples, which are not particularly good ones, and lay special stress on them. The deductions drawn are of a strongly teleological nature, and we cannot do better than recommend the author's reperusal of his work for the refutation of one of his concluding remarks, namely, that "In reviewing this very imperfect and disconnected sketch of the structure of the eyes of the different classes of animals, we cannot fail to recognise the fact that the human eye far transcends, both in mechanism and power, that of every other animal." We however deduce that the condor can see further, that many animals have an extra eyelid, and some bigger eyes than man himself, showing that his is inferior instead of superior in many respects.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Early Contributions to Spectrum Photography and Photo-chemistry

1. MY first attempt at photographing the fixed lines of the spectrum was made in 1834. It was on paper covered with silver bromide. As mentioned in the *Philosophical Magazine*, May 1843, it proved to be a failure. In the summer of 1842, simultaneously with M. Becquerel, by using daguerreotype plates, I succeeded, and in the following March sent a drawing of the photograph to the *Philosophical Magazine*, which was published in May. At that time I did not know that M. Becquerel was experimenting in the same direction.

The great lines α, β, γ, less refrangible than the rest, and which M. Lamansky has recently re-detected by the aid of the thermo-multiplier, are given in that drawing. These in the diffraction spectrum must be bands of very considerable width.

2. Sometimes a person is deprived of fair credit for his labour by what may be termed public perversity. I experienced this in the case of the chlorine and hydrogen photometer. The principle of this instrument is, that chlorine and hydrogen obtained in equal volumes by the electrolytic decomposition of hydrochloric acid, are made to reunite by exposure to light. I described a simple instrument of the kind in the *Philosophical Magazine* for December 1843. It still remains the most sensitive f all photo-o meters. Twelve years subsequently, Professors Bunsen and Roscoe modified it, and used it in their photo-chemical researches. In their memoirs, published in the Transactions of the Royal Society, they give full credit for the invention to me, and remark that by its use I had "succeeded in establishing experimentally some of the most important relations of the chemical action of light." They did justice in the matter, but not so the public. The instrument currently passes as their invention, not mine.

While speaking of photometers there is another to which I may allude. It depends on the principle that a solution of ferric oxalate is decomposed with evolution of carbonic acid on exposure to light. The carbonic acid may be measured or weighed by any of the ordinary methods. I described such an instrument in the *Philosophical Magazine*, Sept. 1857. Quite recently M. Marchand has published in his *Annales de Chemie* several experiments by its use, evidently unaware that it had been employed by me many years ago.

3. In 1843 I made photographs of the diffraction spectrum formed by a grating both by reflection and transmission, and published drawings of them. An account of this may be seen in the *Philosophical Magazine* June 1845 and March 1857. These were the first diffraction photographs ever made. They therefore preceded those of M. Mascart by many years. Of course they were not at all comparable with the very fine ones obtained recently by my son, Dr. Henry Draper.

4. In my memoir "On the production of light by heat" (Phil. Mag., May 1847), I established experimentally the following facts:—

(1) All solid substances and probably liquids become incandescent at the same temperature.

(2) The thermometric point at which some substances become red hot is about 977 Fahrenheit degrees.

(3) The spectrum of an incandescent solid is continuous; it contains neither bright nor dark fixed lines.

(4) From common temperatures nearly up to 977 F., the rays emitted by a solid are invisible. At that temperature they are red, and the heat of the incandescing body being made continuously to increase, other rays are added, increasing in refrangibility as the temperature rises.

(5) Whilst the addition of rays so much the more refrangible as the temperature is higher is taking place, there is an augmentation in the intensity of those already existing.

This memoir was published in both American and European journals. An analysis of it was read in Italian before the Royal Academy of Sciences at Naples, July 1847, by M. Melloni, which was also translated into French and English.

Thirteen years subsequently M. Kirchhoff published his celebrated memoir "On the relations between the coefficients of emission and absorption of bodies for light and heat." A translation of this memoir may be found in the *Philosophical Magazine*, July 1860.

In this memoir, under the guise of mathematical deductions, M. Kirchhoff, taking as his starting-point the condition discovered by Angström in 1854, respecting the relations between the emitting and absorbing powers of different bodies for light and heat, among other things deduces the following facts. I give them as they are succinctly stated by M. Jamin in his "Cours de Physique de l'école Polytechnique" (1869).

(1) All bodies begin to be red-hot at the same moment in the same space, and become white-hot at the same time.

(2) Black bodies begin to emit red rays near 525 C. (977 F.)

(3) The spectrum of solids and liquids is devoid of fixed lines.

(4) The rays first emitted by black bodies are red; to these are added successively and continually other rays, increasing in refrangibility as the temperature rises.

In his celebrated memoir, and in subsequent publications on the history of spectrum analysis, M. Kirchhoff abstains from drawing attention to the coincidences I am here pointing out, except that in a foot-note to his memoir he makes in a single word allusion to mine. But from this no one would infer what were really the facts of the case, and accordingly in the bibliographical lists subsequently published, in works on spectrum analysis, such as those of Prof. Roscoe and Dr. Schellen, my memoir is not noticed.

I earnestly solicit those who take an interest in the history of spectrum analysis to compare my memoir in the *Philosophical Magazine*, May 1847, with those published by M. Kirchhoff thirteen years subsequently, on the radiating and absorbing powers of bodies (Phil. Mag., July 1860), and on the history of spectrum analysis (Phil. Mag., April 1863).

JOHN WILLIAM DRAPER

University, New York, July 8

Sounding and Sensitive Flames

In NATURE, vol. x. p. 223, Prof. Herschel describes some experiments recently made at the Newcastle College of Science, whereby sonorous vibrations are produced in tubes by means of heated wire-gauze instead of the ordinary gas flame. Interesting as are these experiments, they are, however, by no means new. The influence of heated wire-gauze in giving rise to vibrations of air within tubes was, I believe, first published by Prof. Rijke of Leyden. In Kœnig's catalogue for 1865, Rijke's tube is advertised (No. 27) and the method of experiment described. The readiest way of making the experiment is to cut a piece of the ordinary fine iron-gauze to the size of a sixpence or shilling, and press it some three inches up a glass tube of corresponding bore. Almost any length of tube over one foot may be employed, so that notes of varying pitch can be obtained. The gauze is easily heated by a little alcohol flame at the end of a bit of quill tubing. Employing platinum-gauze heated by an electric current, or a gas flame resting above the gauze, the sounds can be rendered permanent. By one or other of these methods no doubt many of your readers have, like myself, often repeated this experiment during the last six or seven years.

I notice also that Prof. Herschel has kindly attributed to me a modification of the ordinary sensitive flame; the credit of this belongs to Mr. P. Barry, of Cork. This arrangement simply consists of a sensitive flame burning on wire gauze, instead o directly from the gas jet. It was described in NATURE, vol. v. p. 30, and some further experiments on this kind of flame are to be found in the journal of the Franklin Institute for April 1872.

Perhaps it is not out of place to add here that when a sensitive flame under the influence of sound is viewed in a moving mirror, the state of its vibration, thus seen, reveals some interesting facts. Under such circumstances, the flame is capable of showing the nature of the different vowel sounds, and further, by the broken appearance of the flame one is able to detect sonorous vibrations too faint to be heard and too feeble otherwise to affect the flame. I have given a representation of the flame seen in a moving mirror on the plate appended to an article in the *Popular Science Review* for April 1867. The flame that is most suited for the vowel experiments happens to be the parent of the family of sensitive flames, and is described in a little paper of mine in the *Philosophical Magazine* for March 1867.

W. F. BARRETT

Science Schools, South Kensington, July 27

Aid to Private Research—Circulation of Scientific Memoirs

THERE are many scientific students scattered through the country, as science-masters in schools, and in other capacities, who are willing and competent to undertake original researches in their special branches of science. The great obstacle to their attempting it is, in most cases, the cost of the necessary instruments. It is of course impossible to expect such apparatus as is required for original work to be supplied from the science funds of a school, these being properly applied to provide only what is requisite for teaching the pupils; so that if an investigation is to be attempted, the whole cost falls upon one who is probably just beginning life, and is quite unable to afford it. The work is therefore postponed for a considerable period, and perhaps is given up altogether. Now the Department of Science and Art grants aid in fitting up the schools which are under its control. If the Department would give similar aid towards purchasing expensive apparatus for research, or would allow competent workers to hire such instruments for the period they require them, much of the difficulty to which I have alluded would be removed. Many, I am sure, would be glad to avail themselves of the opportunity, and would willingly fulfil the conditions necessary to ensure the safety and proper use of the apparatus. I may remark that by this means it would probably be easy to organise to a certain extent the investigations to be carried on, and thus render the results far more valuable than they would be if isolated. Looking to the national importance and the unremunerative character of this kind of work, few will think that this appeal is exorbitant.

I wish to allude to another point, to which attention has already been drawn in your correspondence columns (NATURE, vol. viii. pp. 506, 550). A scientific man, unless he is fortunate enough to be within easy distance of a large scientific library, is practically debarred from reading even the most valuable memoirs that are published. Abstracts, indeed, he may see; but these only serve to remind him that if he would get the original memoir for himself, he must purchase with it matter which is useless to him, but perhaps of the highest value to a worker in another branch. If these memoirs could be purchased in a separate form—or even if collections of papers bearing upon closely related subjects could be obtained—another cause of the costliness of science would be removed.

It has occurred to me that something ought to be done amongst ourselves to remedy our position as regards the transactions of the learned societies and the scientific periodicals. Could not a book-club be instituted, the members of which, upon paying a small annual subscription, should receive in turn the chief scientific periodicals? Or would it be more easy for a number of us who happen to take in different journals, to exchange them? If any of your readers should be inclined to co-operate with me in initiating either of these schemes, or to furnish any suggestions on the subject, I should be glad if he would communicate with me.

Sherborne, Dorset, July 11 H. W. LLOYD TANNER

Photographic Irradiation

As the question of whether irradiation is due to the imperfection of the instruments, or to an action taking place within the thickness of the collodion film, is a matter of considerable importance in all cases in which photography is made use of for the purposes of accurate measurement, I have repeated and somewhat varied the experiments which have lately been described in NATURE, vol. x. pp. 205, 223, by Mr. Ranyard. I therefore laid on a uranium dry plate a piece of platinum foil, and with full aperture of lens took, with an exposure of twenty-five minutes, a photograph of a piece of cardboard, in which were four parallel slits, hung against a background of bright sky. In spite of the long exposure, the images of the slits are sharply cut off at the place occupied by the edge of the platinum foil, though at the same time there are very marked traces of the outer hazy irradiation arising from reflection from the back of the plate. I then took with the same exposure, and under what seemed to be similar conditions of illumination, a photograph of the same cardboard sheet, on an extra-sensitive Liverpool plate, and again found that the images of the slits were sharply cut off. This seems to me to decisively show that the irradiation cannot be due to a spreading within the film, caused by the light dispersed from the highly illuminated particles in the collodion, as suggested by Mr. Aitken; and I feel inclined to agree with Lord Lindsay and Mr. Ranyard that it must be due to some cause that has its seat of action in front of the collodion film.

Bedford W. C. CROFTS

Feathering in Flint Weapons

IT is now some years since I first noticed the fact that in a number of flint weapon heads in my possession a distinct spiral could be traced in the form, this being evidently due in part to the direction of the line of fracture in the flint, but also in part to an exaggeration of this by the hand of the workman. In the last number of the *Scientific American* is depicted an arrow-head with the edges very distinctly feathered, so that if the weapon with which it was armed was propelled with any great rapidity, its revolution would be a matter of necessity and would result in a greater steadiness in its line of trajectory.

After having ascertained that my own weapons were all twisted, I examined a number of others with the view of ascertaining if the same spiral existed in them, and in all I found that there was something like it, and the more finish they presented the more twisted they were.

A very simple method enabled me to show the twist well. I pressed a flint between two pieces of greased pipeclay, then removed it carefully and filled its place with liquid plaster of Paris. Cross-sections of this cast in various directions showed the twist to perfection, and I found that the two wings of the flint were twisted in opposite directions though identical in relation to the axis of rotation), and that the curvatures were identical with those seen in the iron arrow-heads provided with wings which are used in many savage countries to this day, and were till lately, if indeed they are not still, made in large quantities in Birmingham. The most perfectly twisted stone arrow-head which I have yet seen is one made of quartz, where the line of fracture could not help the manufacturer in the least, and where it must have been the result of deliberate workmanship. It was an American weapon. The line of fracture of flint always gives a more or less pronounced spiral, and this may be one of the many reasons for its having been almost universally selected as the material for arrow-heads when it could be got. In fact, it is a difficult thing to find a flint flake of any size which has not a very evident spiral form, and I have a photograph in my possession of two weapons which I have examined and which are almost identical, one found without its shaft near Bridlington, in Yorkshire, and one with its shaft found in the hands of a native of New Zealand; and it would be impossible to tell, from the style of manufacture, which weapon belonged to which country. It is impossible to regard this as mere coincidence, but we must look on it, in each case, as an independent discovery of the principle of the rotation of the rifled projectile. LAWSON TAIT

LOCALISATION OF FUNCTIONS IN THE BRAIN

AT one of the last meetings of the Royal Society, Dr. Burdon-Sanderson related the results of experiments he had recently made with a view to the further investigation of the important discovery of Hitzig and Fritsch, that there are certain spots on the surface of the cerebral hemispheres by the excitation of which the muscles of the opposite side of the body can be thrown into combined action.

It is well known that Dr. Ferrier, of King's College, who has studied the topographical distribution and limitation of these active spots or areas with great minuteness on a considerable variety of animals, has founded upon his experiments a theory that these spots correspond to organs situated at or near the surface of the hemispheres, and that it is the function of these organs to originate combined voluntary movements. Dr. Ferrier has accordingly proposed to call them "motor centres."

As, however, the facts appeared to Dr. Sanderson to be quite as consistent with the view previously entertained by physiologists that the function of co-ordinating voluntary movements is localised lower down in the cerebrospinal centres, he thought it necessary to ascertain, with reference to some of the most characteristic combined movements produced by stimulation of the surface of the brain, by the interrupted voltaic current (Hitzig and Fritsch), or by induced currents (Ferrier), whether the very same combinations of movements could not be produced after ablation of the grey substance in which the "centres" for their production were supposed to be contained. If it could be shown that after complete removal of the "centres," the effects to the production of which they were supposed to be essential could still be observed, this would go far to prove that the facts had been misinterpreted; and if it could be further shown, not only that the phenomena might present themselves in animals deprived of the centres from which they were supposed to originate, but that they could be produced in such animals by the same methods and under the same circumstances as in normal animals, this would go far to negative the existence of any organs at the surface of the brain to which the term "motor centre" could with any propriety or accuracy be applied.

In accordance with these considerations, Dr. Sanderson planned experiments, in some of which the superficial convolutions containing "centres" were removed, while in others the whole of the anterior part of the left hemisphere as far down as the outer portion of the *corpus striatum* was taken away with the aid of a sharpened spoon. In each case it was found (1) that when after the removal of the cortical grey substance, the cut surface of white substance is excited by induced currents, movements of the opposite side of the body are produced, which are of the same character as those which result from excitation of the natural surface; (2) that the excitability is limited to certain spots, which can be as sharply defined as those demonstrable on the natural surface; and (3) that the relative positions of the active spots on the cut and natural surfaces respectively correspond closely with each other.

Simultaneously with the publication of Dr. Sanderson's communication, a paper appeared in Eckhardt's *Beiträge*, in which an account was given of very similar experiments, of which the results, though incomplete, corresponded, so far as theory went, with those above related. We learn also that Prof. Hermann of Zürich has also made experiments which have led him to reject in the most unequivocal manner the conclusions of Hitzig and Fritsch.

THE FORM OF COMETS*

II.

LET us see what ideas, what explanations have been suggested by the aspect of these monstrous phenomena, so evidently subject to the influence of the sun.

On examining comets, the first idea which is pre-

* Continued from p. 229.

sented to the mind is that the head of a comet is the seat of an emission of matter which takes place in a direction opposite to the sun; it seems as if the comet fused at one end, and that the matter thus thrown off is arranged into an immense plume, exactly like the smoke which escapes from the chimney of a steamer at full speed. Let us examine this analogy more closely, and suppose, first, the boat to be motionless, with the smoke ascending vertically in a perfectly calm atmosphere. Each puff of this smoke is sent into the air with a certain speed, and the successive sections of the vertical plume thus formed will represent the positions which these puffs will have reached at the same instant. The puffs first emitted will be the highest; the latest ones will be lowest; if then we knew the law of the ascending movement of any puff, we should thus be able to assign the instant at which each section of the vertical plume was shot forth. Meantime, should we set the steamer moving in the motionless air, the place at which each section is emitted will gradually advance; each of these will ascend almost vertically over its place, for the speed of the horizontal movement which the boat communicates to it will be very rapidly exhausted in resisting the motionless air, and at the end of a certain time these puffs will be found dispersed in an inclined plume, presenting a curvature more or less marked. At first, this curvature will assume a vertical direction, *i.e.* the direction of emission.

On the other hand, the successive puffs, in ascending, tend to spread out; the earliest and highest must then become rarefied and disappear from sight. The tail,—no, I should say the plume of smoke thus formed, must become less and less dense, at the same time becoming less and less distinct and gradually getting obliterated.

Does it not seem as if here we had put our finger upon a complete analogy? The comet proceeds on its way like a steamer; it describes round the sun an orbit elongated like the path of a bomb; heated more and more by the solar rays, its matter is expanded and escapes into space, like that of a rocket. Is it not natural that it should send off a plume analogous to that which escapes from the funnel of a machine in motion? If we knew the rate of emission of each puff of cometary vapour, would we not be able to calculate the place which it must occupy in the tail, and even the form of the tail itself? Reciprocally, after having carefully determined the figure of this tail, would we not be able to form some estimate of the rate of the nucleal emission of the comet? Such, very nearly, was Newton's point of view in studying these magnificent phenomena. The comet of 1680, which appeared in the time of Newton, had a tail of 25,000,000 leagues in length; it forcibly impressed this great geometer, and originated in his mind views similar to the analogy which we have just indicated.

But analogy is not always a perfectly trustworthy guide. Here the differences preponderate considerably over the likenesses. We have certainly in the heavens a heated body which in its progress emits vapours like a gigantic steamer; but where is the funnel, where is the atmosphere? And, remember, the atmosphere here plays an important part, for it is its presence which determines the ascent of the puffs of smoke. If these ascend, it is from the same cause as balloons, because they are lighter than air. Take away the air, instead of mounting they will fall. Well, in the sky there is no air; space is void of matter forming a continuous and ponderous medium, layer on layer, until the surface of the sun is reached. Moreover, Laplace has shown that the power of the sun in attracting a ponderable fluid will not extend beyond a very narrow limit. As to the ether of the physicist, it need not engage our attention for an instant, since, by definition, this hypothetical ether is imponderable. We shall not be much astonished that the genius of Newton should have been content with a similar analogy, if we only reflect on all the difficulties which the doctrine of attraction raised in the minds of the eighteenth century, and on the Cartesian prejudices which greeted its first appearance on the Continent. What would have happened if, at the first, the too absolute terms of this doctrine had seemed to be contradicted by the phenomena of the figure of comets? It was then necessary, at any cost, after having incontestably connected the movement of these bodies with the new doctrine, to let it also be seen, even though it was by an analogy somewhat forced, that their figure could be explained in the same manner.

Now that the doctrine of attraction is established on an immovable foundation, our mind is able to detach itself from the purely metaphysical part of the original affirmations, which presented it to us as the single force to which all celestial phenomena ought to be subordinated. But before invoking another force, it is necessary at the very outset to draw from attraction all the consequences applicable to comets; and we shall do so by showing that the force, which seems constantly to tend to unite, to agglomerate scattered material, is, in reality, also quite capable of producing in certain cases the opposite effect, viz., of undoing existing agglomerations.

To proceed in order, let us ask, first, why comets have tails while planets have not. Is it because comets approach closer to the sun and are thus subjected to a very powerful heat? Certainly not; for the planets Venus and Mercury, especially, are constantly closer to the sun than most of the comets at their perihelion, and yet neither Venus nor Mercury has the faintest trace of a tail. Must we attribute the figure of comets to the parabolic nature of their orbits, in virtue of which their distance from the sun varies enormously, while the planets remain always very nearly at the same distance from the centre of our solar system? An illustrious poet, Lamartine, wishing to depict a creator of the earth, indifferent to his creature, has beautifully said—

> Et d'un pied dédaigneux la lançant dans l'espace,
> Rentra dans son repos.

If the kick had been stronger, the earth would have been sent to describe a cometary orbit round the sun, *i.e.* an elongated ellipse or a parabola, instead of the circle which it now describes; but, for all that, it would not have become a comet, it would have had no tail. Do you know what shape would be the result on this supposition? The imperceptible solar tides of the ocean would be gradually restrained in proportion as the earth increased its distance from the sun, and soon would disappear altogether; our atmosphere would be more and more condensed into layers always spherical and concentric with the earth; our planet would be lost in the depths of infinite space without any other change than a more marked contraction due to the predominating cold of space.

Are comets, then, formed of matter different from that of the planets? No; such an idea cannot be accepted now that spectrum analysis has told us of the existence of sodium, magnesium, and calcium in the sun, hydrogen in the stars, and our ordinary gases even in the most distant nebulæ. Above all, we find the same elements subject to the same mechanical, physical, and chemical laws.

The truth is more simple. If our planets have no tails, it is because they have an enormous mass; if comets have tails, it is because their mass is extremely small, and because the attraction which this mass exercises upon their materials is not sufficient to hold them back and to overcome the external forces which tend to decompose them.

Now have we hit upon a notion which I must dwell upon all the more that it has not hitherto been sufficiently popularised. You have heard of a general law in the world of organised and living beings, called "the struggle for life," the fight or effort which it is necessary to make in order to live, *i.e.* to resist the external forces which tend to death. Those that have in themselves a sufficient resisting force are developed and found persistent races; the feeble succumb and disappear. The same law reigns in the heavens. A body would subsist eternally by virtue of its internal forces if it were alone; but every neighbouring body becomes for it a dissolving cause by virtue of the attraction which the former exercises on the latter. The strong resist; they are the planets: the weak yield and end by succumbing; they are the comets.

Mechanics will convince us of this. Let us take a comet far away from the sun, leaving out of consideration at first the very weak attraction to which the former is subject; we can do this, for it is then sensibly the same for all its parts. Its solid, liquid, or gaseous materials are under the influence of their mutual attractions and of the feeble heat which they receive from without, freely disposed in regular layers, superposed so as to form a globe spherical like the earth, a globe whose centre will be occupied by the most compact parts and whose surface will be formed of the lightest parts. Whether this globe be at rest or in motion, if things remain thus, the comet will subsist; you will see its bright nucleus surrounded by a less luminous but quite sunny nebulosity, and this same form will indicate to you a body in which the forces which act on all its parts are directed towards the centre. Such is the first form in which we have represented Donati's comet (Fig. 3).

But if the comet comes nearer to the sun, the solar attraction will rapidly modify this state of things. The parts nearest to the sun will be attracted more strongly than the centre, and will have a tendency to separate from it; the difference of the solar attraction on the various parts of the comet will have the effect of elongating that body somewhat in the direction of the radius vector; this is a phenomenon quite like that of the tides. The second sketch (Fig. 4) of the comet of 1858 offers an example of this; but already the eccentricity of the nucleus ought to put us on our guard against any incompleteness in our present reasoning, founded upon the sole consideration of attraction. Nevertheless, you see, the body remains entire; the solar action being very feeble, at that great distance, the attraction of the comet on its exterior strata still preponderates, and the resultant of these various forces at each point is still turned towards the interior; the layers which compose it are everywhere convex externally, and do not show any symptoms of dissolution.

But bring the comet still nearer to the sun; the attraction of that body will no longer be limited to the production of an elongation; you will see the external layers become still more deformed and finally open out so as to let matter escape.

There exists, for every body placed within the sphere of action of our sun, a surface limit beyond which its matter may not pass, under pain of escaping to that body and falling within the domain of the solar action. This surface limit depends on two things—the mass of the body and its distance from the sun. For a planet like the earth, whose mass is so considerable, this surface limit is very distant, and yet, within the still terrestrial region of its satellite, the moon, a child could lift, without much difficulty, a body which would weigh for us 36,000 kilogrammes, so feeble does the attraction of our globe become at that distance of 60 terrestrial radii. A little beyond the lunar orbit, a body would cease to belong to the earth, and would enter the exclusive domain of the sun. But for a comet, this surface limit is much nearer the nucleus, and, moreover, it draws nearer and nearer, in proportion as the comet approaches the sun. One of the most eminent professors of the high education, M. E. Roche, of Montpellier, has submitted this question to analysis, leaving aside accessory circumstances such as the rotatory movement of the body under consideration and the curvature of its trajectory; he has thus been enabled to discover that the surface which so limits a body in the vicinity of the sun presents two singular points in the direction of the radius vector, setting out from which this surface is widened out into conical network, in such a manner that the dissolution of a body the matter of which reaches or passes beyond these boundaries, is effected principally in the vicinity of the points referred to, flying, so to speak, into two pieces, thus obeying at once the attraction of the comet and especially the thenceforth preponderating attraction of the sun.

And it ought not to be objected to this that there is no reason why the matter of a body should tend thus to be separated from its centre and to fill a volume greater and greater, so as to reach or surpass the fatal limit. This tendency exists; it proceeds from the increasing heat which a body that approaches nearer and nearer to the sun experiences, and from the progressive expansion which thence follows in the matter. Certainly if the earth were drawn nearer to the sun, the dilatation of its solid nucleus would be a small matter, but thenceforth the seas would be reduced to vapour and would pass wholly into the atmosphere. In the case of comets, in which the matter presents a much less marked degree of aggregation —doubtless because its original heat, due to the union of the particles which compose it, was not sufficient to bring about all the chemical reactions—the solar heat produces an expansion comparable to that of gases. According to my calculations, this expansion dilates the radius of the concentric zones which we can distinguish so well in the head of Donati's comet, at the rate of 19 metres per second. So long as these zones remain in the interior of the surface-limit, they are not dissolved; but if they should happen to go beyond it, their materials go off at the bidding of the sun's attraction.

Thus all the conditions of instability are found united in comets. Their mass is extremely small, and, consequently, the surface limit is very near the centre of gravity. Their distance from the sun diminishes rapidly in the descending branch of their trajectory; consequently this surface limit becomes more and more contracted. Finally, their enormous volume tends unceasingly to dilate, because of the increasing heat of the sun, and to cause the cometary matter to shoot out beyond this surface limit.

What becomes of this matter after it is set free by the action of the sun? Having escaped from that of the comet, it will none the less preserve the original speed, *i.e.* the speed which the comet itself had at the moment of separation; this speed will scarcely be altered by the feeble attraction of the cometary nucleus, or by the internal movements of which I have spoken, since these are measured by a few metres per second, while the general motion round the sun takes place at the rate of 10, 15, 20 leagues and more per second. The molecules, separated and thenceforward independent, then describe isolated orbits around the sun, differing very little from that of the comet. Those which are found in advance go a little faster and take the lead; those which are behind remain a little in the rear; so that the abandoned materials are divided along the trajectory of the comet in front and in rear of the nucleus. In time these materials are separated considerably from the body from which they emanate, and are more and more disseminated; but considered at the moment of emission, they will form two visible appendages, two sorts of tails opposed and stratified on the orbit of the comet.

We touch here on the decisive point of our research. To take the final step it will be sufficient for us to consider the two figures 6 and 7. The first represents the successive shapes C, C′, C″, which a comet must take, according to the preceding theory, if there were no other force in play than that of attraction. Fig. 7 represents the actual fact, *i.e.* the forms which a comet assumes in reality according to its progress in its orbit round the sun, S. Evidently there is no resemblance between these two series of figures. Then the preceding theory fails in some point, and as the error will not have been in the part attributed to attraction, it must be found in the assumption that this is the only force. In other words, it is sufficient to compare the effects of attraction with the real facts, to be convinced that there must be another force at work in the cometary phenomena. And as the former would be capable only of disseminating the matter along the orbit, the new force must be capable of driving this same matter in the direction of the radius vector; it must then be opposed to attraction; it must repel and not draw. What may this force be? Ought it not to make itself felt elsewhere than in the gigantic tails of comets? How can the same body, the sun, at once attract and repel matters of the same origin? And how does it come to pass that since it acts so powerfully on the matter of these bodies, this repelling force of the sun does not change the movement of their nuclei which appear to follow so faithfully the laws of solar attraction? This last question will put us on the right track.

And first, do comets follow rigorously, like planets, the laws of attraction? That the law has been firmly established in the case of the planets, I cannot doubt, for we have for these bodies a historic series of observations going back to the Chaldeans and including thousands of revolutions of each of them. If there had been the least disagreement between the phenomena and the law to which they are assigned, the disagreement, no matter how small, must at length have become sensible, after accumulating during so lengthened a period. But comets, in general, appear only once; we only see them and can only observe them in a very restricted part of their orbit; so that should a very slight influence alter their movements, its effect would be confounded with the inevitable errors of observation, and astronomers would not be able to distinguish it. There are, no doubt, some periodic comets, such as those of Halley, Biéla, Encke, &c., but the first has a period of seventy-five years, so that in going back to its earlier appearances, we very soon reach the time when comets belonged to the domain of astrology. That of Biéla has a period of $6\frac{3}{4}$ years, but its first certain appearance dates only from the end of last century, and in the course of that time a singular accident has happened to it: it has been divided in two. There remains Encke's comet, the only one which can be subjected to the verification of which we have spoken, on account of the numerous revolutions which it has accomplished since its discovery in 1786. Well, it is found that this comet, the only one which can be tested in the way we speak of, does not follow exactly the laws of gravitation. According to these laws, when we have taken account of the perturbations caused by the neighbouring planets, the time of revolution ought to be constant, while, in fact, it diminishes regularly during each revolution; the effect established in this instance is of considerable magnitude, about half a day.

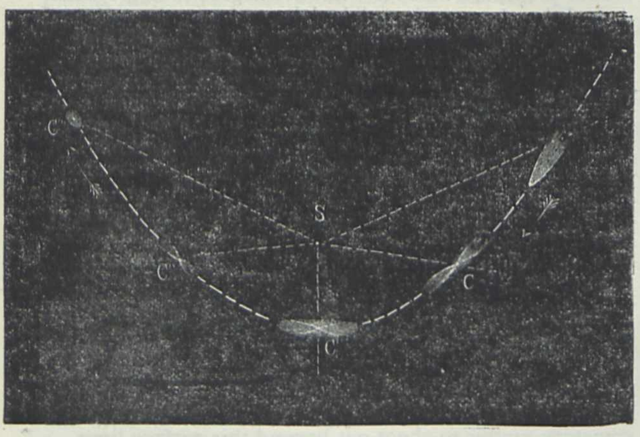

Fig. 9.

In face of such a fact there is room for the question under consideration, viz., Is attraction the only force which governs the universe? But how can we formulate such a doubt, when the carefully-studied movements of the planets may be perfectly accounted for, for thousands of years past, exclusively by the theory of attraction? We can escape the difficulty by an artifice identical with that which enabled Newton to account for the tails of comets by attraction alone: I refer to that vast and rare atmosphere which Newton placed in space around the sun, and in the midst of which the cometary materials are elevated, according to him, exactly as the smoke of our chimneys in our terrestrial atmosphere. Geometers, then, introduced the resistance which this general medium ought to oppose to the progress of a comet on account of its small density, while the same medium would oppose only an insensible resistance to the planets on account of their relatively small volume and their enormous density. It is a remarkable fact that the analysis founded on this impossible hypothesis perfectly accounts for the anomaly proved to exist in the orbit of Encke's comet, viz., its progressive acceleration. I feel bound to question this analysis, and to show (1) that its primary basis is radically false, since it leads to the admission that a material and ponderable medium may remain immovable around the sun; (2) that the conclusion of this analysis, so far as it is valid and conformable to observation, simply proves that there must exist an action opposed to the movement of the comet and directed along the tangent to its orbit. Various causes, moreover, may lead to the same conclusion, and differ only, as to other effects, in quantities difficult to appreciate. But we learned above, from the phenomena of the tails, that there also exists an action in the direction of the radius vector. The resisting medium of Encke, or the immense solar atmosphere of Newton, being physically impossible, I have been led, by two different ways, to a new force which would satisfy these data by producing the two actions or components above mentioned: that which expels the cometary molecules in the direction of the radius vector, and that which acts upon the comet in the inverse ratio of its tangential velocity.

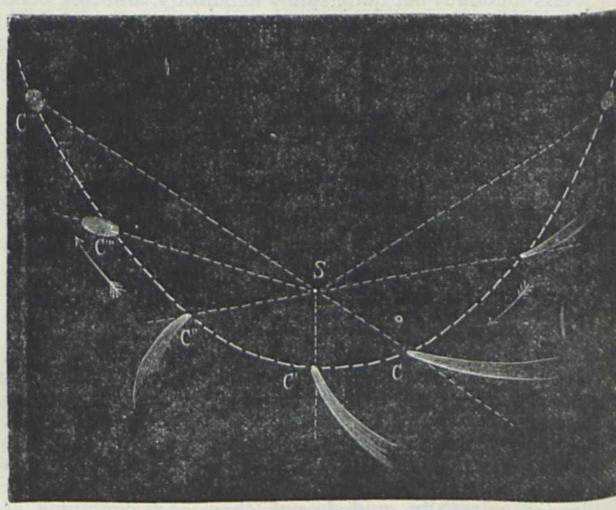

Fig. 7.

(*To be continued.*)

REPORT OF PROF. PARKER'S HUNTERIAN LECTURES "ON THE STRUCTURE AND DEVELOPMENT OF THE VERTEBRATE SKULL" *

VI.

IN no animal has the study of cranial development yielded richer results than in the frog. In tadpoles, from the time of hatching onwards, such points as the true nature of the trabeculæ, and their distinctness from the investing mass, the fact that the stapes is a segmented portion of the ear-capsule, and not the apex of the hyoid arch, and the relations of the pterygo-palatine arcade have been demonstrated with certainty. Most instructive, also, is the way in which the various arches become segmented, altered in shape, direction, and relative size, and made to subserve the most various functions.

Fig. 17.

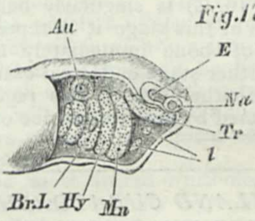

FIG. 17.—Head of Tadpole 2–3 lines long, with the facial arches exposed by removal of the skin from the left side (× 6½).

Besides the adult, nine stages of the frog's skull were described.

1. (Fig. 17).—In tadpoles at about the time of hatching the whole organism is in a very rudimentary condition. The mouth and the gill-slits are closed, the dehiscence of the tissue between the facial arches by which they are formed not having yet taken place. On the first and second branchial arches small papillæ, the rudiments of the external gills, have made their appearance (see Fig. 17). The little creature, now about a quarter of an inch or less in length, is usually found attached to water weeds by the horseshoe-shaped sucker beneath its throat, which, though serving the same purpose, must on no

Fig 18.

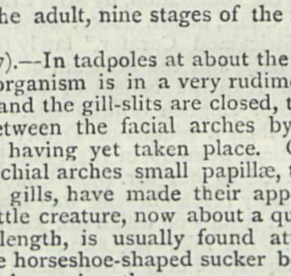

FIG. 18.—Head of Tadpole, 5 lines long (× 6½). Or.P. Orbitar process.

account be confounded with the suctorial mouth of the lamprey. The facial arches are in a perfectly simple and undivided condition, all those behind the mouth are curved slightly backwards in the lower half, while the trabeculæ incline forwards and are thus made to diverge considerably from the mandibulars, although in their upper portion they have almost exactly the same inclination as their successors in the series. The investing mass consists of two small patches of nascent cartilage, one on each side of the notochord. The auditory are the only sense-capsules which have undergone chondrification, and in them the process is quite incomplete, a large membranous space being still left uncovered by cartilage. Two pairs of labial cartilages (l) are formed, and probably answer in a general way to the first and fifth of the series described in the shark (see Fig. 2, l^1, l^5).

* Continued from p. 168.

2. Tadpole about ⅓ in. long). The external gills have now (four or five days after hatching) become plumose and the mouth and branchial clefts open freely into the pharyngeal cavity. The most important advance is in the commencing separation of a small segment (hypo-mandibular) from the second arch, which in the next stage has become Meckel's cartilage. The hyoid has also begun to diverge from its predecessor in its lower part, and a fourth branchial arch has appeared in addition to the three observable in the first stage.

3. (Tadpole about ½ in. in length, Fig. 18.) The trabeculæ have now united with the investing mass and with each other before and behind the pituitary body, and have become almost horizontal; they likewise begin to foreshadow some of the changes which afterwards take place in them, becoming slender anteriorly, to form the cornua trabeculæ (H.Tr), and just behind the olfactory sac

Fig. 19.

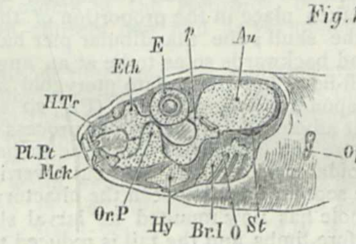

FIG. 19.—Head of Tadpole, 1 in. long (× 4½). Op. Opercular aperture.

being thickened slightly in the future ethmoidal region. Meckel's cartilage now forms a true articulation with the fixed or suspensorial portion of the arch to which it belongs; slightly above the articulation two processes are sent out from the suspensorium; the outer (Or.P) is the *orbitar process*, while the inner (Pl.Pt), uniting with the trabecula, forms a commissural band of cartilage, the rudiment of the pterygo-palatine arcade: between these two processes, the second and third divisions of the trigeminal nerve run. The hyoid arch has assumed a wonderfully shark-like character (see Fig. 2), having divided into an upper and a lower segment, the former of which (hyo-mandibular, H.M) has come into close relation

Fig. 20.

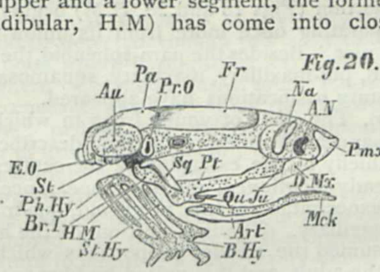

FIG. 20.—Skull of Young Frog, with tail just absorbed (× 5).

with the preceding arch, while the latter hangs free, forming an open angle with the mandible, and unites with its fellow of the opposite side by means of a median basihyal. The investing mass and ear-capsules are now completely cartilaginous.

4. (Tadpoles 1 in. long, Fig. 19.) At this stage the hind limbs have made their appearance, and the opercular fold has completely grown over the gill arches on the right side, a small slit (Op) still remaining on the left. The cranial elements have now assumed somewhat the appearance of a skull, which however differs most markedly from that of the adult frog. The trabeculæ, by complete union in their hinder two-thirds with each other and with the investing mass, have formed a solid *basis cranii;* they have also sent up a low wall on either side of the brain, thus tending to inclose it, and just in front of their union with the pterygo-palatine have developed a prominent transverse ridge (Fig. 19, Eth), the rudiment of

the ethmoid. The suspensorium is still greatly inclined forwards, so that the quadrate lies immediately under the ethmoidal region, and, consequently, the palato-pterygoid and Meckel's cartilage, though lengthening, are still extremely short. The hyo-mandibular has completely coalesced with the suspensorium, which is now therefore a compound structure, and presents above two of the three processes mentioned in the axolotl, namely the pedicle (p) and the otic process (o), the latter at this period belonging equally to both arches, the pedicle to the mandibular only. The branchial arches have united with one another above and below to form a perfect branchial basket. The stapes (St) is now completely cut out of the wall of the ear sac, and the first ossification has made its appearance on the base of the skull, in the position of the para-sphenoid.

5. In tadpoles in which the legs have increased greatly in size and the tail has begun to shrink, a marked advance has taken place in the proportion of the jaws to the rest of the skull; the mandibular pier has moved downwards and backwards so as to lie at an angle of 45° with the skull-flow, and the palato-pterygoid and lower jaw are correspondingly lengthened. (Fig. 20 shows the process further advanced.) The orbitar process is greatly decreased in size and lies higher up on the suspensorium, and the ethmoidal cartilage has sent out a vertical keel-like plate (the septum nasi) between the olfactory sacs.

6. The tadpole has now moulted its larval skin, so as to expose the fore-limbs, and the tail is reduced to half its original size. The walls of the brain-case, commenced in the fourth stage, are now complete, and by their union above have formed a roof, interrupted only by their membranous fontanelles, which are persistent in the adult, one in the frontal, and a symmetrical pair of smaller ones in the parietal region. The septum nasi is complete, and two wing-like processes growing from it have inclosed the nasal capsules by uniting with the floor formed by the greatly expanded hypo-trabeculars. The hyoidean portion of the otic process (Fig. 19, o) has now freed itself from its connections, and appears as a triangular nodule of cartilage, the pharyngo-hyal (Fig. 20, Ph.Hy), or detached apex of the arch; at the same time the remainder of the coalesced portion (Figs 18 and 20, H.M) begins to show signs of separating once more from its union with the mandibular pier. Besides the para-sphenoid, the parietal, frontal, nasal, pre-maxillary, maxillary, squamosal, articular, and dentary ossifications have appeared.

7 (Fig. 20). The skull of young frogs in which the tail has just disappeared differs from that described in the last stage, chiefly by the extension of the centres of ossification already mentioned, and the appearance in addition of the exoccipital, prootic, pterygoid, quadrato-jugal, and septo-maxillary. The free portion of the hyoid (St. Hy) has assumed the slender proportions which characterise it in the adult, and it is united by fibre to the upper part of the arch (H.M), which, although still fused with the suspensorium, is marked off from the latter by a distinct depression, and shows unequivocal signs of commencing separation.

8. A most important metamorphosis has taken place in this stage, which includes young frogs just commencing their first summer. The pharyngo-hyal or nodule of cartilage separated from its arch in the sixth stage (see Fig. 20, Ph.Hy) has now come into close contact with the stapes, although it does not actually articulate with it until the succeeding stage; this freed apex of the hyoid arch thus becomes the inter-stapedial piece (Fig. 16, p. 168, i.st) of the ossicula auditus, the representation of the *os orbiculare* of mammals. At the same time the next segment of the same arch (Fig. 20, H.M) has become completely separated from its connection with the suspensorium, and has taken on the form of the other three elements of the chain of ear-bones, the medio-, supra-, and extra-stapedials (Fig. 16, m.st, s.st, e.st), which together are the homologue of the mammalian *incus*. The malleus, although having its functional analogue in the extra-stapedial (the end of the chain fitting into the drum-membrane) is represented morphologically by the frog's suspensorial cartilage, being, as will be shown in a future paper, the proximal end of the mandibular arch.

9. The embryonic characters are now (first autumn) fast disappearing. The suspensorium is at right angles with the long axis of the skull, or almost exact half-way between the positions it occupies in the seventh stage (Fig. 20), and in the adult (Fig. 14, p. 168). The ossicula auditus have come into union with the stapes, and the stylo-hyal instead of being attached (as in Fig. 20) to the suspensorium, has grown backwards to its adult position, where, however, it is united only by fibrous tissue. The parietals and frontals are still separate, and the maxilla has not extended backwards to the quadrato-jugal, although the fibrous space between them is now quite small. The girdle-bone (Fig. 19, G) is singularly behindhand in its ossification; even at this stage it is represented only by a slender plate of bone immediately anterior to the frontals. At a further stage endosteal ossification sets up in the cartilage on either side of this region, so that the girdle-bone is formed by the coalescence of three separate centres.*

THE STRICKLAND CURATORSHIP IN THE UNIVERSITY OF CAMBRIDGE

THE Vice-Chancellor of the University of Cambridge has approved the nomination, by Miss Frances Strickland, of Apperley Court, of Mr. Osbert Salvin, F.R.S., to the office of "Strickland Curator," lately founded and endowed by that lady, and the Museum of that University will therefore reap the benefit of having attached to it one of the best English ornithologists of the day. Mr. Salvin, being then a scholar of Trinity Hall, graduated in mathematical honours in 1857, and immediately afterwards proceeded to join Mr. (now Canon) Tristram in the natural history researches he was making in Algeria, the important results of which are known to many of our readers. In the following autumn he sailed for Central America, and there began that series of scientific observations which has made him the chief authority on the zoology of that part of the world. How many times he has since visited it we cannot say, but he only returned from his last expedition some two months ago, and he has besides been all the while well occupied. In addition to the many papers he has published, mostly on the birds of the Neotropical Region, he has, in conjunction with Mr. Sclater, brought out an illustrated "Exotic Ornithology," intended as a sequel to the celebrated works of Daubenton and Temminck, and in 1870 was chosen editor of the *Ibis*, the leading ornithological periodical of the world.

But our object here is not to sound the praises of Mr. Salvin, who, it will be seen from what we have said, does not requre them, but to point out the advantages that would accrue to science if posts for the study and promotion of its various other branches, similar to the recent foundation, were established in our Universities. We are greatly mistaken if the "Strickland Curatorship" is not the very first step that has been made towards a fulfilment of that idea of the endowment of research which has been often urged in these columns, and was especially recommended in the late Report of the Royal Commissioners on Scientific Instruction and Aid to Science. Admitting that the intention of Miss Strickland was mainly to secure the proper keeping of her late brother's ornithological collection, which was some years ago given by his widow to the University, what will be the effect of the foundation? The merely mechanical part of the curator's

* It should have been stated in the last paper that Fig. 13 is taken from a drawing kindly furnished by Prof. Huxley.

duties is slight. A collection once put in order is easily so retained. Even the cataloguing of it is a task that may not be expected to occupy an ornithologist of Mr. Salvin's ability, knowledge, and experience, a very long time—though catalogues in these days, to be worth anything, are more serious affairs than most people would fancy. The regulations of the office prescribe that its incumbent should then turn his attention to the other ornithological collections possessed by the University; and, even if the rest be trifling, the Swainson Collection may be expected to form a formidable undertaking—to say nothing of others that may be acquired from time to time. We take it for granted that the University will not allow such catalogues to remain in manuscript, but will print and publish them as they are completed. If so, it will be promoting the advancement of science in this particular direction in the most efficacious mode possible, and yet, be it remembered, not in a way that by any means can be termed "educational." The compilation of these catalogues will be purely a matter of research, and the amount of aid they will furnish to scientific ornithologists cannot be calculated. There can be little doubt that to the centre in which such good work is being done, many other collections will gravitate, and thus Cambridge will be for many years to come a recipient and disseminating focus of Ornithology.

Now, even the most ardent ornithologist will hardly maintain that his favourite study is the most important in the wide round of the sciences, or even of those which have to do with biology. The moral of the "Strickland Curatorship" is, that similar appointments ought to be established to do for other sciences what that will do for Ornithology. And even now we have to mention a curious fact which should be an encouragement for future founders or foundresses to cast their bread upon the waters: two other benefits to this branch of science have unexpectedly been the result of Miss Strickland's endowment. The naturalist first selected by her for the new appointment was the learned Dr. Finsch, who, until the last few months, had been pursuing his unwearied labours on a scanty and uncertain pittance at Bremen. When the good people of that city learned that they were likely to lose his services, they bethought them that it was expedient to retain him, and to do this they resolved upon raising his stipend and making his office in their museum permanent. In like manner it happened that Miss Strickland's next selection, a young naturalist of great promise, was induced to stay at Berlin by the creation of a post in the museum there specially for him. Thus the benefactress of Cambridge has the satisfaction of knowing that her bounty has been the means of providing for two meritorious men, besides accomplishing the object she had directly in view. Will no one come forward to further the good work she has so well begun? Now that there is a rumour that one of our greatest living naturalists is likely to be tempted by a glittering bait to the other side of the Atlantic, it is in the power of many a one to preserve the glory of his services to England by founding a Professorship of Biological Research in the University of John Ray and Charles Darwin!

A NEW ORDER OF HYDROZOA

ON the southern shores of France, at a slight depth below the surface of the sea, there may be found attached to stones small patches of one of the horny sponges which will probably arrest the attention of the zoologist by what will appear to him as an unusually obvious and well-defined condition of their efferent orifices or oscula.

If one of these patches be transferred to a phial of sea-water, the observer will soon be astonished by seeing that from every one of the apparent oscula a beautiful plume of hydroid tentacles will have become developed, and he will naturally believe that the form has at last been found which will remove all doubt as to the zoological position of the sponges, and decide in favour of the hydroid affinities recently assigned to them.*

A more careful examination, however, will show that the orifices on the surface have been incorrectly regarded as oscula, and that the tentacles form no part of the sponge, but proceed from an entirely different organism which is imbedded in its substance.

It will be further seen that the organism with which the sponge is thus associated is contained in a congeries of chitinous tubes which permeate the sponge-tissue, and open on its surface in the manner of genuine oscula, and it will be still further apparent that this organism, while undoubtedly a hydrozoon, and even presenting quite the aspect of a hydroid trophosome, is no hydroid at all, and cannot indeed be referred to any of the hitherto recognised orders of the Hydrozoa, but must take its place in an entirely new and as yet undefined order of this class.

The chitinous tubes and their contents are united by a common tubular plexus which lies towards the base of the sponge, and they thus constitute a composite colony of zooids. The tubes, towards their free extremities, where they open on the surface of the sponge, become much increased in width, and here their contents become developed into a very remarkable body, which has the power of extending itself beyond the orifice of the tube, and of again withdrawing itself far into the interior exactly like the hydranth or polypite of a campanularian hydroid in its hydrotheca. When extended, it displays from around the margin of a wide terminal orifice its beautiful crown of tentacles; but when withdrawn into the interior of the cup-like receptacle, the tentacles are greatly contracted and thrown back into the cavity of its body. Its general appearance, indeed, is very like that of a campanularian hydranth, and a careful examination is needed in order to show that it possesses all the essential characters, not of a hydranth, but of a medusa. It has a circular canal surrounding the terminal orifice and supporting the tentacular crown, and it has four symmetrically-disposed longitudinal canals extending from the circular canal backwards in the walls of the body. No manubrium could be detected, though this was carefully sought for at the point where it might be expected to be found, namely, where the medusiform zooid passes into the common cœnosarc which occupies the narrower portion of the tube; neither was there any appearance of a velum, nor of lithocysts or ocelli; but these are comparatively unessential modifications.

The reproductive system is probably developed in the walls of the longitudinal canals, but in none of the specimens examined was this part of the organisation sufficiently mature to admit of a satisfactory demonstration.

For the little animal thus constructed I propose the name of *Stephanoscyphus mirabilis*. Whether it is to be regarded as parasitically connected with the sponge, or whether the two are only accidentally associated, it is at present impossible to say. At all events, in no instance did I find the Stephanoscyphus unaccompanied by the sponge.

Stephanoscyphus may then be regarded as a compound hydrozoon, whose zooids are included in cup-like receptacles resembling the hydrothecæ of the calyptoblastic hydroids; but these zooids, instead of being constructed like the hydranths of a hydroid, are formed on the plan of a medusa. It has plainly very decided affinities with the Hydroida, but is nevertheless removed from these by a distance at least as great as that which separates from them the Siphonophora. It thus becomes the type of a new hydrozoal order, for which I propose the name of THECOMEDUSÆ.

GEO. J. ALLMAN

* See Haeckel's "Kalkschwämme."

ANOTHER NEW COMET.

THE following letter from Mr. J. R. Hind, dated Mr. Bishop's Observatory, Twickenham, July 27, appeared in Tuesday's *Times*:—

"M. Stephan, Director of the Observatory at Marseilles, notified to us by telegram yesterday the discovery of a comet on the previous night by M. Borrelly, a colleague of M. Coggia (to whom is due the first detection of the bright comet which we have just lost), at that Observatory. The position at 2 A.M. on the 26th inst. appears to have been close to the star Theta, in the constellation Draco, in right ascension 238 deg. 4 min., and polar distance 30 deg. 28 min. The comet is pretty bright, and its motion towards the west. Clouds have prevented any observation at Twickenham during the past night.

"A communication from Berlin this morning mentions —contrary to what I should yet have expected from my own calculations relating to the orbit—that Dr. Tietjen, of the Imperial Observatory, has found indications of a sensible deviation from parabolic motion in the recent bright comet between April 19 and July 14. The curve is elliptical, but the inferred period of revolution is of such length as to be open at present to uncertainty, which can only be removed by observations in the other hemisphere. The semi-axis major is found to be rather more than 430 times the earth's mean distance from the sun, and the corresponding length of revolution is nearly 9,000 years.

"The tail of the late comet increased very quickly and considerably in length, as frequently happens soon after perihelion passage. Assuming it to have proceeded from the nucleus very nearly in the direction opposite to that of the sun, its actual length had increased from 4,000,000 miles on July 3 to 16,000,000 on the 13th, and on the 19th, the last night it was visible in this hemisphere, to something over 25,000,000 miles. The increase of apparent length in this interval was from 4 deg. to 43½ deg."

NOTES

THE Priestley Centenary is to be celebrated, not only at Birmingham, as we have before announced, but at Leeds, by two meetings, to be held in the hall of the Philosophical Society. The chair will be occupied at the two meetings by Dr. Clifford Allbutt and Mr. Sykes Ward, F.C.S., and addresses are to be given by the Rev. J. C. Odgers, who is to read a paper On the personal history of Priestley; Mr. T. Fairley, F.C.S., On the phlogiston theory; and Mr. S. Jefferson, F.C.S., On the discovery of oxygen.

DR. ACLAND, Regius Professor of Medicine in the University of Oxford, has been appointed president of the Medical Council, in succession to Dr. Paget, of Cambridge. We believe the appointment is a five-yearly one.

AT a general meeting of the Council of the Yorkshire College of Science, held last Friday, Dr. T. E. Thorpe was elected Professor of Chemistry. Dr. Thorpe has for the last four years had the direction of large classes in theoretical and practical chemistry at the Andersonian University, Glasgow. He is the author of "A Manual of Inorganic Chemistry" and "A Text Book of Quantitative Chemical Analysis," and has made many original contributions to chemical literature.

THE death is announced of Father Paul Rosa, the colleague of Father Secchi at the Roman Observatory.

THE Select Committee of the Legislative Assembly of New South Wales, which was appointed to inquire into the management of the Sydney Museum, has furnished its report, in which the appointment of a Curator, with complete charge of the property of the Museum, subject to the Minister of Public Instruction, is proposed; at the same time an extension of the building at present holding the collection is suggested.

MR. C. A. BOWDLER'S apparatus for steering balloons was tested on Saturday last at Woolwich, in the presence of several officers of the scientific branches of the army. The balloon to which the apparatus was attached was the new large one, 80 ft. high, belonging to Mr. Coxwell, which was considered by Mr. Bowdler too large for the size of his machine. His apparatus is very simple, consisting of fans like the screw propeller of a ship, 3 ft. in diameter, and making 12 or 14 revolutions per second, worked by hand. When the balloon was exactly balanced the vertical fan caused it to rise and fall, but the horizontal fan was found to have no effect whatever in guiding the direction.

THE French National Assembly has adopted the proposal to award to M. Pasteur a pension of 12,000 francs, one half of which reverts to his wife should she survive him.

WE view with great pleasure the advance of the Birkbeck Institution within the last few years in its scientific department. Quite recently a scientific society has been established in connection therewith, the object of which is to inculcate and develop a taste for scientific pursuits among its members, by the reading of original papers upon scientific subjects and by debates, and particularly for the encouragement of the application of scientific principles to the arts and manufactures. In immediate connection with this society we find a Naturalists' Field Club, the aim of which is to organise excursions to the various districts possessing scientific interest, for the purpose of studying practically and under the direction of practical men, those sciences, such as geology, mineralogy, botany, &c., a real and sound knowledge of which can only be obtained by the actual study of Nature. We wish this new undertaking all possible success. As a proof of the high character of the work performed by this institution and the excellence of the instruction provided, we need only call attention to the fact that this year its students have been awarded one half of the total number of prizes offered for public competition by the Society of Arts at its annual examinations.

THE Royal Academy of Belgium proposes the following subjects for prizes to be awarded in 1875:—1. To examine and discuss, on the basis of new experiments, the perturbing causes which influence the determination of the electro-motive force and the interior resistance of an element of the electric pile; to find out the number of these two quantities for some of the principal piles. 2. To give an exposition of the knowledge attained on the relations of heat to the development of phanerogamous plants, particularly in reference to the periodic phenomena of vegetation; and, in this connection, to discuss the value of the dynamical influence of solar heat on the evolution of plants. 3. To make new researches on the embryonic development of the *Tunicata*. 4. To make new researches to establish the compositon and mutual relations of albumenoid substances. 5. To describe the coal-system of the basin of Liége. A gold medal of the value of 1,000 francs is the prize in subjects 4 and 5: one of 600 francs for subjects 1, 2, and 3. The memoirs ought to reach the Secretary of the Academy before August 1, 1875. They must be written either in Latin, French, or Flemish.

THE destruction of vineyards by *Phylloxera*, which has lately so much engaged the attention of entomologists and botanists, was recently the subject of a bill in the French Assembly. Many prefects, on the plea of public welfare, have issued orders for the uprooting and burning of diseased plants, and opposing the introduction of foreign stocks; but to make this desperate course effectual, a special law putting the *Phylloxera* on a level with the rinderpest is necessary. M. Destreaux has submitted a bill to make this possible, and the Academy of Sciences gives it its support. Notwithstanding the investigations that have made, no reliable specific against *Phylloxera* seems to have been yet discovered. The bill before the Assembly is received as "urgent."

MR. NEWBIGGING, in his "Handbook for Gas Engineers and Managers," London, 1870, p. 159, gives a "Chronology of Gas Lighting." By this author's statement the first public exhibition of gas in London was in 1807, by Mr. Winsor, who lighted Pall Mall at that time. But Prof. B. Silliman, writing to the *American Gas Light Journal*, gives an extract from the elder Prof. Silliman's "Journal of Travels in England, Holland, and Scotland," containing a description of a public exhibition of illuminating gas from coal in July 1805, by "an ingenious apothecary" in Piccadilly, near Albany House. "The inflammable gas," the journal states, "is extricated simply by heating common fossil coal in a furnace, with a proper apparatus to prevent the escape of the gas, and to conduct it into a large vessel of water, which condenses the bituminous matter resembling tar, and several other products of the distillation that are foreign to the principal object. The gas, being thus washed and purified, is allowed to ascend through a main tube, and is then distributed by means of other tubes concealed in the structure of the room, and branching off in every desired direction, till, at last, they communicate with sconces along the walls, and with chandeliers depending from the roof, in such a manner that the gas issues in streams from orifices situated where the candles are commonly placed. Then it is set on fire, and forms very beautiful jets of flame, of great brilliancy; and from their being numerous, long, and pointed, and waving with every breath of air, they have an effect almost magical, and seem as if endowed with a kind of animation. The gas is sometimes made to escape in revolving jets, when it forms circles of flame—and, in short, there is no end to the variety of forms which ingenuity and fancy may give to this brilliant invention. The expense of the apparatus, and its liability to accidents, forms an obstacle of magnitude, and, on the whole, it is probable it will not be generally adopted." This is curious reading in 1874! Mr. Murdock had employed gas illumination in 1792, and gas was used in Paris in 1802. But London was in the dark until 1805.

DR. MELLICHAMP, of Bluffton, South Carolina, has been prosecuting researches on the pitchers of *Sarracenia variolaris* and the way in which insects are caught in them. The species abounds in this district, and even early in May many pitchers were developed. He has confirmed the presence of the sugary secretion within the rim. He finds that it bedews the throat all the way round the rim, and extends downwards from ¼ in. to ¾ in. Dr. Mellichamp also finds—and this is his most curious discovery—that this sweet secretion is continued externally in a line along the edge of the wing of the pitchers down to the petiole or to the ground, forming a honeyed trail or pathway up which some insects, and especially ants, travel to the more copious feeding-ground above, whence they are precipitated into the well beneath. Ants are largely accumulated in these pitchers. As to the supposed intoxicating qualities of this secretion, Dr. Mellichamp was unable to find any evidence of it. On cutting off the summit of the pitchers and exposing them freely to flies in his house, he found that the insects which came to them, and fed upon the sweet matter with avidity, flew away after sipping their fill, to all appearance unharmed. On the other hand, he thinks that the watery liquid in which the insects are drowned and macerated possesses anæsthetic properties; that house-flies, after brief immersion in it, and when permitted to walk about in a thin layer of it, "were invariably killed—as at first supposed—or at any rate stupefied or paralysed in from half a minute to three or five minutes," but most of them would revive very gradually in the course of an hour or so.

IT is probable that a scheme for the establishment of another Medical School at Dacca, on the same footing as those of Calcutta and Patna, will shortly be sanctioned by Government.

THE success which has attended the ostrich-breeding farms in South Africa has induced some French gentlemen to endeavour to imitate the system in Algeria, and African birds have also been sent to La Plata and other countries in South America, where it is hoped they may take the place of the native birds, which are inferior in quality to the African ostrich. Generally speaking, the system on which ostrich farms are conducted is as follows. The birds kept for breeding purposes, about three years old, are placed in separate paddocks, in pairs, and their eggs are either hatched in the natural way or placed in incubators prepared for the purpose. By this means a larger proportion of eggs is hatched. The young birds are fed on grass, lucerne, and other vegetable matters, and are sheltered at night. Each pair of birds will produce about twenty chickens, which may be plucked when they are about eighteen months old, before which time the feathers are not of much value. The price of good ostrich feathers, wholesale, is about 40*l*. per pound weight. If the birds are well kept, and have plenty of exercise and food, their feathers are of good quality; but the plumage of wild birds is considered superior to that of inferior tame ones. The value of each year's plucking from the young birds is about 7*l*., and of the birds themselves at six months old is 30*l*. to 35*l*. The breeding birds are worth 125*l*. per pair.

THE new screw steamship *Durham* sailed from Plymouth on Sunday, bound for Melbourne, having on board several members of the Imperial German Astronomical Expedition. They carry with them a large number of instruments with which to observe from Port Ross, one of the Auckland Islands, the coming transit of Venus.

WE have received, reprinted from the excellent Indian ornithological journal *Stray Feathers*, a copy of a lengthy paper by Mr. V. Ball, on the Avifauna of the Chota Nagpur division of Bengal, which, besides giving an account of the birds found in the district, contains an instructive description of its geology, flora, and mammalian fauna; the author laying great stress, as is but too seldom done, on the interdependence between these mutually related phenomena.

THE tenacity of life of popular errors is well exhibited in the following extract from the *Californian Horticulturist*:—"The influence of forests in drawing moisture from the heavens may be seen from the experience of San Diego, California. Previous to 1863 there was yearly a rainy season, which made the soil nourishing and productive. In 1863 a destructive fire swept over the greater part of the country, destroying the forest and blackening the hills. Since then there has been no rainy season at San Diego." When will public writers learn that forests influence the climate by drawing water, not from the air, but from the soil?

AN addition is in preparation to the Colonial Floras published under the authority of our Colonial Government, in the form of a "Flora of Mauritius." It will be edited by Mr. J. G. Baker, assistant-curator to the Kew Herbarium.

PROF. SCHIMPER, of Strassburg, in a paper read before the Botanical Congress at Florence, claims to have discovered a fossil plant in "protogine," a rock hitherto considered of igneous origin, which occurs in the form of erratic blocks on the sides of Mont Blanc and in the plains of Piedmont. The specimen, which was collected by M. Sismonda, and is preserved in the Museum of the Turin University, has been identified by Prof. Schimper as *Annularia sphenophylloides*, a plant, perhaps aquatic, widely distributed in the coal-strata of Mont Blanc.

A DRAWING-ROOM meeting in aid of the Palestine Exploration Fund was held on the 24th inst. at the residence of the Duke of Westminster, Grosvenor House. Capt. Warren, before giving an account of his experiences, made an appeal to the meeting for increased support to carry on the work of exploration, which was at present flagging for want of funds. He urged the subscribers to the Fund to complete the work of surveying the country as soon as possible, as the land, being so fertile, was constantly being taken by the Greeks and other foreign cultivators of the soil for farming purposes. As a consequence, the old names of the towns and villages were fast disappearing, and the whole country was assuming a different aspect. This meeting was the first of a series that is to be held, information as to which can be obtained of the secretary at the office of the Fund, 9, Pall Mall East.

ACCORDING to the State geologist of Minnesota, the cretaceous lignite beds of Minnesota Valley are likely to afford valuable coal mines.

IN the report to the Admiralty of Capt. G. S. Nares, of H.M.S. *Challenger*, dated Melbourne, March 25, 1874, Capt. Nares, speaking of the temperature of the ocean, especially near the pack edge of the ice, says:—"At a short distance from the pack, the surface water rose to 32°, but at a depth of 40 fathoms we always found the temperature to be 29°; this continued to 300 fathoms, the depth in which most of the icebergs float, after which there is a stratum of slightly warmer water of 33° or 34°. As the thermometers had to pass through these two belts of water before reaching the bottom, the indices registered those temperatures, and it was impossible to obtain the exact temperature of the bottom whilst near the ice, but the observations made in lower latitudes show that it is about 31°. More exact results could not have been obtained even had Mr. Siemens' apparatus been on board." It seems to us that the difficulty mentioned is one which would certainly have been surmounted by Messrs. Negretti and Zambra's new recording thermometers, a description of which appeared in NATURE, vol. ix. p. 387; this being exactly one of the cases to which this instrument is peculiarly adapted. We believe the inventors and makers have greatly improved their thermometer since our description appeared, and no doubt means will be taken by the Admiralty to transmit one to the *Challenger*.

MR. PILLISCHER, optician and scientific instrument maker, of New Bond Street, W., has been decorated by the Emperor of Austria with the golden Cross and Crown of Merit, as are cognition of his Majesty's approval of the superior quality and precision of his scientific instruments shown at the late Vienna Exhibition.

THE following is a list of candidates successful in the competition for the Whitworth Scholarships (Science and Art Department), 1874:—William Martin, metal turner, Wolverton; Robert A. Sloan, engineer's apprentice, Birkenhead; William Sisson, engineer, Gateshead; Frederick Stubbs, engineer's apprentice, Derby; Thomas L. Daltry, draughtsman's apprentice, Newcastle-on-Tyne; Frederick H. Livens, engineer's apprentice, Gainsborough.

THE additions to the Zoological Society's Gardens during the past week include two Tigers (*Felis tigris*) from Calcutta; two Yellow-billed Sheathbills (*Chionis alba*) from the Southern Ocean, presented by Mr. H. Roberts; a Wanderoo Monkey (*Macacus silenus*) from the Malabar Coast, presented by Lieut. Vipan; a Rose-crested Cockatoo (*Cacatua moluccensis*) from the Moluccas, presented by Mr. John Elms; three Grey-breasted Parakeets (*Bolborhyncus monachus*) from Monte Video, presented by Mr. C. Purnchard; a King Vulture (*Gyparchus papa*) from Tropical America; a Red-backed Buzzard (*Buteo erythronotus*) from South America, purchased; a Philantomba Antelope (*Cephalophus maxwellii*) born in the Gardens.

ON SPECTRUM PHOTOGRAPHY *

II.

I NEXT come to a very beautiful reflex action of spectroscopy on photography; and now I must take you back to America. I am nearly certain that everyone in this room is perfectly familiar with the name of Rutherfurd in connection with celestial photography: if you will allow me I will point my reference to him by throwing on the screen one of his magnificent photographs of the moon, which he was good enough to give me some little time ago. Unfortunately, I am not able to throw on the screen a photograph of the solar spectrum which we owe to him, the most magnificent photograph—and I say it with the intensest envy—which I think it is possible to obtain. However, I have a copy of it on the wall, and it is well worth inspection. Rutherfurd, whose name is associated with that of Delarue in the matter of celestial photography, was not content with reflectors. He lives in the centre of New York, and I suppose New York is as bad as London for tarnishing everything that the smoke and atmosphere can get at; and he came to the conclusion that he must abstain from celestial photography altogether, or else make a lens—and a lens with Mr. Rutherfurd means something over 12 in. diameter—which should give him as perfect an image in New York with 15 in. of glass, as a perfect reflector of 15 in. aperture would give him as far away from a city as you please. Mr. Rutherfurd, who never minces matters, knowing that it was absolutely impossible to get such a lens as this from an optician, who of course neglects almost entirely the violet rays—the very rays which he wanted—in constructing an ordinary telescope, determined to make such an one himself. He thought about the matter, and he came to the conclusion that in any attempt to correct a lens of this magnitude for the chemical rays, the use of the spectroscope would be invaluable. He therefore had a large spectroscope made, in order to make a large telescope, and then we have just as distinct an improvement upon the instruments which we owe to the skill of those who first adopted the suggestion of Sir John Herschel and brought together the chemical and the visual rays, as the improvement we owe to Herschel was upon the instruments which dealt simply with the visible rays. Mr. Rutherfurd simply discards the visual rays, and brings together the chemical rays; the result of his work being a telescope through which it is impossible to see anything, but through which the minutest star, down I believe to the tenth magnitude, can be photographed with the most perfect sharpness. This is the instrument of the future, so far as stellar astronomy is concerned. Having thus achieved what he wished in the construction of this instrument, and having the spectroscope, Mr. Rutherfurd commenced a research, which, I am sorry to say, he has never published, for it would be of the greatest value to any photographer or any astronomer amongst us, upon various kinds of collodion and upon the best arrangement of lenses for photographing the spectrum. Mr. Rutherfurd found that some collodions which he got were so local in their action as to be almost useless for that reason, and that others were so general in their action that they were also almost useless for the exactly opposite reason. I will now throw on the screen the line G and the lines in the green, or rather the lines approaching to the green near F; with ordinary collodions, such as one generally gets, that is to say, collodions not absolutely good, but free from both the extremes referred to by Mr. Rutherfurd, we want something like five seconds for the part near the line G. Well, when you go a little way along the spectrum in the less refrangible direction, you have to put minutes for seconds—in other words, the exposure has to be sixty times as long. I have another photograph of the spectrum, which will show you the part of the spectrum less refrangible than the line F to which I have referred. This photograph which you see on the screen required an exposure of very nearly half an hour.

Those of you who are most familiar with the solar spectrum will recognise the extreme importance of Mr. Rutherfurd's contribution to photographic spectroscopy, when I tell you that his photograph of the solar spectrum is quite as admirable and excellent as is the photograph of the moon which I have just shown you on the screen. During the last year this question of the solar spectrum has again been considerably advanced by photography in America. Mr. Rutherfurd's photographs, admirable although they are, are refraction photographs, that is to say prisms were

* Continued from p. 112.

used, and, more than this, prisms of glass. You will, therefore, quite understand that the photograph extends only a very little distance beyond the lines H. But America was not satisfied with this, and in the person of Dr. Draper, the son of the Professor Draper whose name is so honourably associated with the commencement of work done in photography thirty years ago, has just now photographed the solar spectrum far beyond H. A copy of his photograph is on the wall, but unfortunately I have not a copy which I can throw on the screen.

I have already referred to the extreme importance of photography in astronomy, and the point that I wish to urge to-night, after what I have stated regarding all the work which has been done up to the present time, is this—That what photography has been in the past to astronomy—what it will be in the future no one can say—such can photography, and such must photography, be to chemists and physicists. Of course, in the way of photographic application, it is scarcely fair to say that a daily photographic record of the prominences around the sun is a question either of physics or of chemistry. But still the method which enables us, or which, I hope, will enable us shortly, to obtain a daily photograph of every prominence which bursts out—although absolutely invisible to our eyes—on the sun, is a method which depends on physical laws, and has nothing to do with astronomy in the ordinary sense. If you will allow me, I will show you now on the screen a photograph of a drawing which was made by an eminent Italian observer in India during the last eclipse. It is a drawing made by Prof. Respighi, of the sun's corona, as seen by the spectroscope; and I hope in the next eclipse we shall not any longer have merely drawings to refer to, but that we shall have a photograph which can be brought here, and which will let us know exactly how the matter stood. You see there on the screen three rings—a red ring, a green ring, and a blue ring. They are red, green, and blue, because the element in that part of the sun's atmosphere—hydrogen—gives us lines in the red, green, and blue; and they are rings because the hydrogen atmosphere extends in the most admirably regular way all round the sun. In fact, we may say that, in observations of this kind, we use the corona instead of the slit, and if that is good for the corona it is perfectly obvious to you it is good for the chromosphere—for the brighter regions lying closer to the sun than the corona does—as we know that it gives a line of intense blue, exactly where photography, as it is generally carried on, has its strongest *point d'appui* in the spectrum; and it is quite clear to you that we ought to be able to get a photograph of this every day, just as easily as we saw it in India during the eclipse.

We will next consider the application of photography, no longer to the mere solar spectrum, but to the physics of the sun. What is the solar spectrum? It is the continuous spectrum of the sun, minus certain portions where the light of the continuous spectrum has been absorbed. What have been the absorbers? The gases and vapours, generally speaking, in an excessively limited zone of the sun's atmosphere, lying close to the bright sun we see; close, I say, to the photosphere. This zone is called the reversing layer. Then if the solar spectrum is the result of the absorption of this reversing layer, what will happen to the solar spectrum if the constitution of the layer changes? Obviously a change in the solar spectrum. Now, recent researches carried on by means of photography show us that if you take any particular vapour in the reversing layer, which you may call A, for instance, and then assume that the quantity of A in the layer is reduced, the absorption of that particular vapour will be reduced; what then will be the result on the photograph of the solar spectrum? Some of the lines will disappear. Suppose that this particular vapour which we call A, instead of being assumed to decrease in quantity, increases in quantity, what will happen to the solar spectrum? The same researches have told us that as its quantity increases its absorption will increase, and that its increased absorption will be indicated by an increase in the number and in the breadth of the lines absorbed. What, then, will happen to the solar spectrum if any change of this kind is going on? The photograph of a solar spectrum taken, say, to-day, may be different from the photograph of the same part of the spectrum taken at some distant period. What is the distant period we do not yet know—whether three months, six months, six years, or eleven years; but, at all events, there is reason to think already that if we had a series of photographs of the solar spectrum, taken year by year, we should see great changes in the spectrum. Allow me to show you a photograph of a very limited portion of the solar question, and I will prove my case; and let me tell you I could not prove my case if photography had not been called in, because if the existence of any particular metal, or of the increase of any particular metal, depends on such a small matter as one line among 10,000, what will happen if a man neglects to observe this change? People will say, "Oh! in a research of that kind it is altogether excusable if he has made a mistake." But if you have a series of phenomena recorded by means of a camera on "a retina which never forgets," as Mr. Delarue has beautifully put it, and if you compare those pictures day by day, and year by year, the thing is put beyond all question when you get one line disappearing, or another line appearing.

Now we have before us a part of the solar spectrum near the line H, and I wish to call your particular attention to one line. We have admirable drawings of the solar spectrum taken about the year 1860. If the draughtsman was recording by means of his eye the lines in the spectrum, he would not be very likely to overlook a line darker than some he inserts, but he might easily overlook finer lines. Now, it is a fact that in the most careful map that we have—a map drawn with a most wonderful honesty and splendid skill—a line is absent in the region indicated, which line is now darker than some that were then drawn, and that line indicates the presence of an additional element in the sun—strontium. I do not make this assertion thinking that subsequent facts will show the drawing to be wrong, but because I see reason to believe that what we know already of the sun teaches us that it is one of the most likely things in the world that strontium was not present in such great quantity in the reversing layer when the drawing was made; but, however that may be, I think you will see how important it is that this photograph, which I have just thrown on the screen, should be compared with photographs made five, ten, fifteen, a hundred, or two hundred, or as many years as you like ahead, and it is in this possible continuity of observation of the solar spectrum, carried on for centuries, that I do think we have in photography not only a tremendous ally of the spectroscope, but a part of the spectroscope itself. Spectroscopy, I think, has already arrived at such a point, at all events in connection with the heavenly bodies, that it is almost useless unless the record is a photographic one. I am glad to say that only to-day I have had a letter from Dr. Draper, who tells me he has at last succeeded in getting an admirable photograph of the spectrum of a star. Now that is of the very highest importance, because the sun is nothing but a star, and the stars are nothing but distant suns; and as long as we merely investigate the sun, however diligently or admirably we do it, and neglect all the others, it is as if a man who might have the whole realm of literature to work at should confine himself to one book, and that book probably not a good representative of the literature of the country he was examining into.

So much for the application of photography to what may be called the celestial side of spectroscopy; but let me tell you that this, so far as spectroscopy is concerned, does not exist. To the spectroscope all nature is one, and it is absolutely impossible to make a single observation, either on a sun, or a star, or a comet, without bringing chemical and physical considerations into play; and it will be a regrettable circumstance if chemists employ the spectroscope in terrestrial chemistry—they have not done much in that way yet—without taking the sun and all the various stars of heaven into counsel, because the spectroscope is absolutely regardless of space, and shows us that the elements which are most familiar to us here, or at all events a good many of them, are present in the most distant stars, and the spectroscope shows us those elements existing under conditions which are absolutely impossible here.

There is another point, too—spectroscopy is, above all things, molecular. We are dealing with the ultimate atoms, or molecules, or whatever you like to call them, when, by means of the spark, we drive a substance into vapour. And if chemists, for instance, will simply ask themselves which substances have their lines reversed in the solar spectrum, I think, before they have thought that problem out—that very simple problem, as it seems—there will be such a flood of light thrown upon terrestrial chemistry, that the only wonder will be that it has not been seen before, years and years ago. These, you will say, are theoretical applications. It is perfectly true; and there are a great many other theoretical applications that it would be my duty, as it would be my pleasure, to bring before you, if time permitted. But that is not all. I have to refer to the application of the spectroscope in what are considered by some people more practical directions. The more you deal with the most abstruse considerations of Science, the more likely you are to get practical applications out of them.

You have already seen how exceedingly important it was to use a slit instead of a round hole in these experiments. It was the verdict of Wollaston, and it was the verdict of Becquerel and Draper, as I have shown you to-night with regard to photography. You have also seen that we can use the circular corona as a slit equally well.

Now if we take a long slit and divide it into as many portions as we choose, we see at once the improvement that we introduce into spectroscopic photography. All we have to do is to divide that slit into portions, as it were, by letting a window run down the slit, and when the window has arrived at the second part of the slit, let in light from a new source. This principle has been carried out practically in the following manner:—A rectangular brass plate 71 mm. long, and 35 mm. broad, slides in grooves in front of the slit of the spectroscope, and a window 4 mm. high, cut out of this plate, leaves a portion of the slit of this length exposed. A small pin presses firmly against the face of one of the sliding plates, and a row of small shallow holes or notches is drilled in the plate so as to intercept it in its upward or downward movement at those points where the pin falls into a notch. The distance between the notches is precisely the same as the height of the opening cut in the sliding plate, so that the movement of this plate from one notch to another corresponds to a distance equal to the height of the exposed part of the slit, and the spectra compared are confronted, so to speak, absolutely; the upper edge of one spectrum abuts against the lower edge of the other, and the coincidence, or want of coincidence, between lines in the two spectra can thus be determined with the greatest precision. The spectroscope employed contains three prisms of 45° and one of 60°; its observing telescope is replaced by a camera with a 3-in. lens by Dallmeyer of about 23 in. focal length for the use of which I am indebted to Lord Lindsay. With this arrangement—the spectrum being received upon a sensitised ¼ plate—the portion between the wave-lengths, 3,900 and 4,500, can be obtained at once in good focus. A ray of sunlight, reflected from a heliostat mirror so as to fall upon the slit-plate, is brought to a focus by means of a double convex lens just between the carbon poles of an electric lamp, while a second convex lens placed between the lamp and the collimator tube, serves to cast an image of the sun or of the electric arc upon the slit-plate. Supposing, now, we wished to compare the iron spectrum with that of the sun: the sun's image in sharp focus on the slit-plate is first allowed to imprint its spectrum on the prepared plate. The ray of sunlight is then cut off, the sliding plate moved up or down till the pin catches in the next notch, and the image of the arc, passing between an upper pole of carbon and a lower pole consisting of a carbon crucible containing a fragment of iron, is allowed to fall on the portion of the slit thus exposed.

Let me show you some photographs illustrating this description. Here is a single photographic plate on which the new method has enabled us to register no less than four different spectra; those of you who are familiar with photographic processes will immediately see how it is that the number is not forty instead of four. Having a slit of a certain length, if I open all the length of that slit at once I should get a spectrum the breadth of which would depend upon the length of the slit; but if I commence operations by allowing the light first to come through one small portion of the slit, then we shall get the light from the particular metal which I employ in the electric arc falling on one part of the plate, and registering itself on the photographic plate. Then, if I close up that part of the slit, and open another one, I shall be able, through that newly opened part of the slit, all the rest being closed, to photograph on the plate the spectrum of another substance, say iron. Then, having used up that part of the plate, I can close that portion of the slit, I can bring my window lower down, and there we have the spectrum of cobalt. The window has been brought farther down, and there we have the spectrum of nickel, so that we have, as the work of some eight or nine minutes at the outside, a photograph—not a perfect one in this case, but this was the first one taken on this method—which will register with the most absolute and complete accuracy and certainty not less than 1,000 lines. Now a careful student of those lines, working as hard as he can, thinks himself very fortunate if he can lay down ten an hour. Therefore, as ten an hour are to 1,000 in seven minutes, so is the eye to photography in these matters.

I have a photograph of a somewhat similar nature, which I am anxious to place before you. We have here an absolute comparison rendered possible, by means of photography, between the lines of the spectrum of iron and the lines of the spectrum of the sun. You see that in the solar spectrum corresponding with the lines of the iron. And, more than this, you see, I hope, all of you, that these lines of iron are of different lengths. The reason of that is that I have been careful to photograph on the plate the lines due to the various strata of iron vapour, from the rarest vapour, which is obtained at the outside of the electric arc, to the densest, which occupies the centre of the core, and you will see the most beautiful gradation as we pass from the outside part of the spectrum to the inside. This inside part represents the complete spectrum of the core, and the outside the incomplete and almost mono-chromatic spectrum of the vapour which surrounds the denser core in the middle of the spark; thus we have practically reduced the spectrum of iron to one line, instead of 460. That is the first photograph of the kind which has been taken; I say that, not because I am proud of it, but because you all know how enormously photographic processes are likely to be developed the moment, not one individual, but a great many, try their hands upon them, so that an enormous improvement upon what you now see may be anticipated. Not only have we developed, in the application of photography to spectroscopy, a valuable ally to Science, as we have in the application of photography to astronomy—and you know what that has done, and what it is going to do—but we have, I believe, what we may almost call a new chemistry, some day to be revealed to us by means of photographic records of the behaviour of molecules. Recollect that the difference between the iron spectrum of one line and the iron spectrum of between 400 and 500 lines is simply due to the difference in the arrangements of the molecules or atoms of iron in the centre of the electric arc and its exterior. There is one question which all lovers of the spectroscope may ask of photographers, and that is this, why should we any longer be confined, in registering spectra, to the more refrangible end of the spectrum, when one of the very first spectra of the sun that was ever taken was a complete photograph of the spectrum, including not only the blue, the green, and the yellow, but the red, and the extreme red? I think that if photographers will study the action of light on molecules, and read that extraordinary paper of Becquerel's, and will give those who are familiar with the spectroscope, and those who are anxious to promote the progress of spectroscopic research, a means of extending photographic registration, not only into the green part of the spectrum, which they do already with difficulty, but to the extreme red, then the use of the eye will almost entirely be abolished in these inquiries. And although no one has a higher estimate than myself of the extreme importance of the eye, I think that the more it is replaced by permanent natural records in these inquiries, the better it will be for the progress of Science.

J. NORMAN LOCKYER

SCIENTIFIC SERIALS

THE current number of the *Quarterly Journal of Microscopic Science* contains several papers of interest. Dr. Michael Foster commences with an article On the term Endothelium, in which he proves that the word is etymologically pure nonsense, Ruysch's word epithelium signifying that it covers papillæ. His endothelium must be understood to mean *that it is inside a papilla.* It is also valueless for other reasons: for if it is defined as that epithelium developed from the germinal mesoblast, the epithelium of the Wolffian ducts, of the ureters, and of Muller's ducts would have to be included. Therefore the term is insignificant and must be abolished. *Monoderic* and *polyderic* are proposed as terms to indicate that the cells form one or several layers.—The second part of Prof. Haeckel's interesting Gastræa theory follows, in English. In it the systematic and the phylogenetic signification of the Gastræa theory and the ontogenetic succession of the system of organs are discussed, as well as the bearing of the whole on the theory of types. The author is so prolific in his introduction of new words, the definitions of many of which are to be found in other publications, that a Haeckel Glossary in the next number of the *Journal* would not be out of place, to assist readers in the full appreciation of that illustrious biologist's very suggestive theory.—Mr. J. W. Groves explains his method of arranging and cataloguing microscopic specimens.—A paper follows by Mr. E. C. Baber On picro-carminate of ammonia as a microscopic staining fluid, in which he explains M. Ranvier's method. The great advantage of this reagent is shown to consist in its staining tissues in a series of colours varying from red to yellow; it also acts rapidly and can be kept in a dry form.—The Rev. E. O'Meara describes a collection of Diatomaceæ from Spitzbergen, including many species not enumerated by Cleve's Diatoms of the Arctic Sea.—Mr. Buck describes and figures a new Polyzoon belonging to the family *Halcyoneleæ*, named by him *Clavopora hystricis*, from a single specimen obtained in the expedition of the *Porcupine* in the Mediterranean.—An article from the *Indian Medical Gazette*, On the etiology of Madura foot, is discussed, the vegetable origin of that disease being severely handled. A note by the Rev. M. J. Berkeley is appended, strongly supporting Dr. V. Carter's original observations.—Dr. W. G. Farlow, of Harvard University, writes On a sexual growth from the prothallus of *Pteris cretica*, in which he shows that whilst in some of the prothalli archegorina and antheridia were developed, others gave rise to young fern-plantlets by a direct budding of the cells, without any sexual intervention. The paper is illustrated with two plates.—Mr. E. R. Lankester has two papers, one on *Torquatella typica*, a new type of Infusoria, allied to the Ciliata, from Naples; peculiar in not possessing cilia, not even round the oral region and capitular prominence, but in their place a bell-like prolongation of the body-wall like a ring of united cilia. The second paper is on the heart of *Appendicularia furcata*, in which that organ is shown to consist of two nucleated cells connected by fourteen or so slender vibratile fibrillæ, whose mutual connection by a membrane is uncertain. This organ is thus nothing more than a "most vigorous churn, beating and stirring up the fluid in the great perivisceral hæmolymph space without propelling it in any particular direction." The paper ends with some suggestive remarks on the reduction of the structure of organs in diminutive elaborate types generally.

Justus Liebig's Annalen der Chemie und Pharmacie.—Band 172, Heft 2.—The following papers are published in this part:—On the salts of parabanic acid, by N. Menshutkin. The formula of the ammonium salt is $C_3H_2N_2O_3.NH_3$; by the action of water on the salt the ammonium salt of oxaluric acid is produced, and by the action of heat alone oxaluramide. The potassium and sodium salts have likewise been examined and two silver salts obtained, of which the formulæ are $C_3HAgN_2O_3.H_2O$ and $C_3Ag_2N_2O_3.H_2O$.—The same author contributes a paper entitled, "Notice on potassium oxalurate and the determination of the alkaline metals in the salts of the acids belonging to the uric acid group."—On the oxidation products of colophony and oil of turpentine, by Dr. Josef Schreder. By digesting colophony with dilute sulphuric acid, isophthalic acid ($C_8H_6O_4$) and trimellithic acid ($C_9H_6O_6$) are obtained. Turpentine oil oxidised by dilute nitric acid, gives terebinic and terephthalic acids.—On the conversion of cinchonidine into an oxy-base, by Dr. J. Skalivert. Cinchonidine is mixed with carbon disulphide and bromine dropped into the mixture. A brominated compound of the formula $C_{20}H_{22}Br_2N_2O$ is thus obtained, which on treatment with potassium hydrate yields the new oxy-base $C_{20}H_{24}N_2O_3$. Analyses of the sulphate and of the double Pt salt are given.—On ferrous anhydrosulphate, by T. Bolas, already noticed in the Journal of the Chemical Society.—The following are communications from the Tübingen laboratory:—(1) On the cyan- and carboxyl derivatives of diphenyl, by Oscar Doebner.—(2) On normal phenyl propyl alcohol and allylbenzene, by Leopold Rügheimer.—(3) Researches on the synthesis of allylbenzene, by Rudolf Fittig.—(4) Researches on the constitution of piperine and its decomposition products piperic acid and piperidine, by R. Fittig and W. H. Mielck. This is the fourth notice on the subject, and the authors now touch upon the constitution of piperic acid. By the action of bromine a tetra-brominated acid $C_{12}H_{10}Br_4O_4$ is obtained which by the action of sodium carbonate is converted into the dibrominated compound $C_{12}H_8Br_2O_4$. This last substance boiled with soda solution and precipitated by an acid yields a monobrominated body of the formula $C_{12}H_8BrO_5$. The authors next proceed to the consideration of a new acid which they have obtained by acting upon monobrompiperonal with bases, and then decomposing the salt produced by means of hydrochloric acid. The new acid has the formula $C_{12}H_{10}Br_4O_5$, and must be regarded as the substitution product of the acid $C_{12}H_{14}O_5$. By the action of soda on the sodium salt of the new acid an intermediate compound having the formula $C_{12}H_8Br_2O_5$ has been produced. Bromine dropped into a solution of hydropiperic acid in carbon disulphide, gives rise to the formation of the compound $C_{12}H_{12}Br_2O_4$. In the concluding section the decomposition of hydropiperic acid by means of fused potassium hydrate is treated of. The chief product of the reaction is protocatechuic acid, $C_7H_6O_4$, H_2O. The authors finally assign the constitutional formula

$$C_6H_3\left\{\begin{array}{l}\left.\begin{array}{l}O\\O\end{array}\right\rangle CH_2\\ CH = CH - CH = CH - CO - OH\end{array}\right.$$

to piperic acid.—The part concludes with papers by Peter Greiss On the sulphurisation of sulphurobenzoic acid or dicarboxylsulphocarbanilide, and by M. G. Gustavson On tetra-iodide of carbon. This substance has been obtained by the action of aluminic iodide on CCl_4 according to the equation—

$$3CCl_4 + 2Al_2I_6 = 2Al_2Cl_6 + 3CI_4.$$

The substances are made to react in carbon disulphide solutions.

Gazzetta Chimica Italiana, Fascicolo V., 1874. This part contains the following papers:—On the extraction of sulphur, by F. Sestini.—On the action of sulphur on earthy carbonates, particularly on neutral calcium carbonate, with regard to geology and agriculture, by Prof. Egidio Pollacci. This paper was communicated in April to the Reale Istituto Lombardo di Scienze e Lettere. The author's principal object is to prove that a mixture of sulphur and calcium carbonate acted upon by water with free access of air gives rise to the formation of calcium sulphate. The chemical changes are thus expressed—

$$2S + 3O_2 + 2H_2O = 2H_2SO_4$$
$$2H_2SO_4 + 2CaCO_3 = 2CaSO_4 + 2CO_2 + 2H_2O.$$

The author is of opinion that the oxidation of the sulphur is effected *directly* by atmospheric oxygen in presence of $CaCO_3$ and water.—The concluding paper is entitled Chemical Research on Turkey Red, by Prof. Abelardo Romegialli. The remainder of the part is devoted to abstracts from foreign periodicals.

Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie, July 1.—This number contains an article by Dr. J. Hann On the diminution of atmospheric vapour with increase of elevation. Experiment and mathematical theory both deny the existence of an independent vapour atmosphere, which according to Dalton's law would decrease much less rapidly with elevation than atmospheric vapour really does. Hence Mr. Strachey (Proceedings of the Royal Society, March 1861) would not deduct the vapour pressure from the height of the barometer to obtain the pressure of dry air. From a table showing the actual decrease of vapour tension with increase of height, observed in various ascents of mountains and in balloons, is derived a formula to express this decrease. Thus where p and p_0 stand for the pressures at the height h and at the surface of the earth, h being measured in units of 1,000 English feet,

$$(1)\ p = p_0\,(1 - 0{\cdot}075\,h + 0{\cdot}00146h^2)$$

and where e is the bases of natural logarithms, and h in units of 1,000 metres,

$$(2)\ p = p_0\,10^{-\frac{h}{6517}} = p_0 e^{-\frac{h}{2830}}$$

If atmospheric vapour obeyed the law of Dalton, its weight over any place would be four and a half times greater than the real weight. Dr. Hann calculates the weight of vapour at 1,962 metres to be only half, at 6,500 metres one-tenth, of the weight at the surface of the earth. With respect to this rapid decrease of moisture, Strachey remarked that mountain chains, even of moderate altitude, must have great influence upon the hygrometric state of the atmosphere. The above formula can only be used safely for calculating the *mean* pressure of vapour at a given height. It may be useful for barometric measurements of altitudes, since it frequently happens that the vapour pressure of only one of two stations, of which the difference in height is required, is known. Observed values, up to 1,884 metres, have been actually found to agree well with those calculated by the formula. This formula may be only another expression for the opinion of Strachey, that the mean degree of saturation at different heights remains nearly uniform, and therefore the vapour tension depends merely on decrease of temperature. But the calculation of the mean vapour pressure of one level from that of another level with so great accuracy appears not to have been hitherto accomplished.

Annali di Chimica applicata alla Medicina, t. lviii., No. 5.—In dietetics there is a paper by Dr. F. Turbacco On cheese and its alimentary use.—Beaumertz furnishes a contribution on farinaceous substances as food for children.—In pathology there is a paper by Dr. L. Ledeganck (translated from *La Presse Médicale Belge*) On the action of parasitic organisms in the production of necrosis.—In therapeutics we have the following papers:—On the anæsthesia produced in man by the injection of chloral into the veins, by Oré.—Under the heading "Varieties" there is a paper by Prof. Fausto Sestini On the chemical composition of mulberry leaves; one On a new method of extracting logwood for vines and inks, from the agricultural chemical laboratory of Bologna, directed by Prof. A. Casali and Francesco Marconi; and, finally, a contribution by Melsens On the use of solutions of sulphurous acid, of neutral acid and sulphites, and of hyposulphites.

SOCIETIES AND ACADEMIES

London

Royal Society, June 11.—Researches in Spectrum Analysis in connection with the spectrum of the sun.—No. IV., by J. Norman Lockyer, F.R.S.

Maps of the spectra of calcium, barium, and strontium have been constructed from photographs taken by the method described in a former communication (the third of this series). The maps comprise the portion of the spectrum extending from wave-length 3900 to wave-length 4509, and are laid before the Society as a specimen of the results obtainable by the photographic method, in the hope of securing the co-operation of other observers. The method of mapping is described in detail, and tables of wave-lengths accompany the maps. The wave-lengths assigned to the new lines must be considered only as approximations to the truth. Many of the coincidences between lines in distinct spectra recorded by former observers have been shown by the photographic method to be caused by the presence of one substance as an impurity in the other; but a certain number of coincidences still remain undetermined. The question of the reversal of the new lines in the solar spectrum is reserved till better photographs can be obtained.

Royal Horticultural Society, July 1.—Scientific Committee. Dr. Hooker, P.R.S., in the chair.—Dr. Gilbert described the results of some investigations made by Mr. Lawes and himself on the conditions of the development of fairy rings. The mycelium of the fungus which produced the rings accumulated nitrogen in the superficial layers of the soil with the result of stimulating the growth of the grass and giving it the dark green colour which is characteristic of vegetation richly supplied with nitrogenous nutriment. When this luxuriant growth was grazed off, the soil was left relatively poor in nitrogen, and it was accordingly found that the superficial soil inside the rings was poorer in nitrogen than that outside it.—Dr. Hooker stated that seeds of the Kerguelen's Island cabbage (*Pringlea antiscorbutica*) sent to Edinburgh in a sealed bottle had germinated, while those sent to Kew in boxes had altogether failed. The following communication from Mr. Darwin was read:—"The leaves of *Pinguicula vulgaris* possess a power of digesting animal matter similar to that shown by the sundews (*Drosera*). Albumen, fibrin, meat or cartilage induce a secretion from the glands of the upper surface of the leaf, and their secretion becomes feebly acid (but not so much so as that of *Drosera*). Their secretion is reabsorbed, and causes an aggregation of the protoplasm in the cells of the glands, such as had been observed in other similar cases. Before excitement the glands were seen to be filled with a homogeneous pale greenish fluid; after the aggregation of the protoplasm it can be seen to move. When a row of insects or of cabbage seeds are placed near the margin of a leaf (or when a single insect is placed at one point), the whole margin (or one point) becomes curled considerably over in two or three hours; the apex of the leaf will not turn over towards the base. Small fragments of glass also cause a similar movement, but to a much less degree. The inflexed margin pours forth a secretion which envelops the flies or seeds, but pieces of glass cause no, or hardly any, increase of secretion. But here comes a puzzle: if the flies or fly be removed, the margin of the leaf turns back in less than twenty-four hours; but it does so also when a row of flies and cabbage seeds are left adhering; so that the use or meaning of the inflexion is at present quite a puzzle."—Mr. W. G. Smith showed engraved wood blocks of lignum vitæ, which he found more brittle than box.

Vienna

Imperial Academy of Sciences, Feb. 26.—Dr. Urba communicated a paper on some rocks of South Greenland, collected by Prof. Laube, from the second German Polar Expedition.—M. Pelz presented a memoir on determination of the axes of conical surfaces of the second order.—Dr. Adolph Meyer gave an account of new and little-known birds of New Guinea.—Dr. Exner read a paper on the employment of the ice-calorimeter for determining the intensity of the solar radiation; describing an apparatus by which the intensity may be measured directly in calorics, without (as in the Pouillet pyrheliometer) a change of temperature in the instrument, rendering correction necessary.—Dr. Brauer communicated a note on the development and mode of life of *Lepidurus productus* Bosc.—MM. Schulhof and Holetschek communicated the elements and ephemerides of a comet discovered on Feb. 20 by Prof. Winnecke at Strassburg.

Paris

Academy of Sciences, July 20.—M. Bertrand in the chair.—The following papers were read:—Note on the action of two current elements, by M. Bertrand. The assertion that two elements of the same direction attract one another is shown to be inexact, even for parallel elements, and does not agree even with Ampère's law. The author has solved the following problem:—A current element being given, to find in a point of space M the direction that must be assigned to another element, that their mutual action may be attractive, repulsive, or *nil*.—Extract from the Report of the Commission of the Agricultural Society of Chalon-sur-Saône, in the department of Saône-et-Loire, on Phylloxera, by M. Boully.—Reply to a criticism by M. Garrigou, contained in a recent note entitled "Carboniferous Limestone of the Pyrenees; Marbles of Saint-Béat and of Mont," by M. A. Leymerie.—On the efficacy of the method of submersion as a means of improving the vine in the Crimea: extract from a letter from M. Boutin to M. Dumas.—Employment of the *résidues d'enfer* of the oil-mills against *Phylloxera*, by M. Rousseau.—Third note on the electric conductivity of ligneous bodies, by M. Th. du Moncel.—On the stratification of the electric light, by M. Neyreneuf.—On the passivity of iron, by M. A. Renard. The author described several experiments illustrative of methods by which iron can be made passive in ordinary nitric acid.—Action of chloroform on sodic acetatic ether, by MM. A. Oppenheim and S. Pfaff. The product of the reaction was saponified by soda and then acidulated with HCl. A new acid of the formula $C_9H_8O_5$ is thus obtained. This acid is dibasic and belongs to the aromatic series, the authors considering it an isomer of uvitic acid, the substituted groups occupying the positions 1 : 2 : 4.—On the isomeric compounds C_2H_4IBr, by M. C. Friedel. The author has repeated the experiments recently published on this subject by M. Lagermarck, and concludes therefrom that no third isomer of this formula exists.—On a development of heat produced by the contact of sodium sulphate with water at temperatures when the known hydrates of sodium sulphate cannot exist, and when the saturated solution of the salt deposits it only in the anhydrous state, by M. de Coppet.—Ethers of normal propylglycol, by M. E. Reboul.—Experiments on the generation of proto-organisms in media protected from aërial germs, by M. Onimus.—Indifference in the direction of the adventitious roots of a cactus, by M. D. Clos.—Observation of a bolide on the evening of July 18, at Versailles, by M. Martin de Brettes.—On the composition of potassium permanganate, by M. E. J. Maumené. The author concludes that the formula of the salt is Mn_2O_7KO.—New method of determining metals or oxides, by the same author.

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