THURSDAY, JUNE 1, 1871

SCIENCE LECTURES FOR THE PEOPLE

IT is the great weakness of Science in this country that its professors are rather a mass of incoherent units than an organised body eager to influence others and themselves enjoying the privileges of such influence.

Each one is apt to work too much by himself, and while he often exhibits the most rare skill in discovering truth, he too frequently leaves to others less able than he the task of bringing his labours before the world at large.

Now, while the man of science complains with much justice that his pursuits have not been recognised by the rulers of our country, he ought not to forget that it is likewise his duty to help others, in doing which he will help himself. Whatever be the faults of our rulers, they are eminently sensitive to public opinion; men of science, therefore, have only to prove to the people that they are a useful class in order to have their services recognised. It is really absurd to suppose that one of the most intelligent and useful bodies of men in this country could not obtain their just demands if they set themselves earnestly and unitedly to the task. They have hitherto tried to prove to our rulers that the promotion of science will benefit the country, but have met with only indifferent success: let them supplement their endeavours by convincing our rulers that to promote it will be for their own benefit, and they are sure to succeed. Success, in fine, will not be attained by a policy of isolation, but by leavening the whole mass of the community with the love of science, and when this is done science will rise to its just place in the councils of the nation.

Many of its chiefs have now begun to perceive this, and we are glad to record the success of one of the best organised attempts that have hitherto been made to extend the knowledge and love of science among the working classes. The Science Lectures for the People, lately delivered in Manchester, have been a very great success, whether we regard the numbers who attended them, or the standing of the lecturers, many of whom came from a considerable distance in order to give their information to the people of Manchester. In the cheap and simple form in which these lectures are now published they constitute an eminently readable and instructive book, suitable for all classes. The titles of the lectures are as follows: (1) Coral and Coral Reefs, by Prof. Huxley; (2 and 3) Spectrum Analysis, by Prof. Roscoe and Dr. Huggins; (4) Coal, by Mr. Dawkins; (5) Charles Dickens, by Prof. Ward; (6) The Natural History of Paving Stones, by Prof. Williamson; (7) Temperature and Life of the Deep Sea, by Dr. Carpenter; (8) Formation of Coal Strata, by Mr. Green; (9) The Sun, by Mr. Lockyer. We are much indebted to Dr. Roscoe for arranging this admirable series of lectures, and also to Mr. T. J. P. Jodrell, who has generously defrayed the heavy expenses connected with their publication. Surely, too, the men of Manchester owe a debt of gratitude to Dr. Roscoe and his friends for this intellectual feast, the elements of which are at once so excellent and so varied. It would be presumptuous in any one man to criticise such lectures, but let it be said no more that the chiefs of science are either unable or unwilling to explain to others the discoveries which they themselves have made. They are at last emerging from their seclusion, and recognise their functions as teachers of truth. "A people," says Dr. Roscoe, "whose masses are without knowledge and without tastes for higher things than the mere struggle for existence can come to no good." These are truthful and noble words, and point to what ought to be the future action of men of science. Their author, we learn, is constantly asked about science lectures, and he thinks that if there were the means of sending lecturers to various localities they might be of the greatest value. But to do this a common action is necessary; for it is surely too much to expect that each large town should independently obtain such lecturers, and publish such a volume as that now under review. Indeed, the question is a more important one than at first sight appears; for a national society, formed with the view of diffusing scientific information among the populace of large towns, would be the beginning of a powerful union capable of forcing the claims of science before the Government of the country. Most of the leaders of science are disposed to admit that such a union is desirable, but many of them object to the formation of a new body. For, curiously enough, in matters of administration we are all of us evolutionists, and dislike very much the appearance of a new organisation that has not been developed by insensible degrees from some previous organisation of a humble character and living under other conditions.

Now, such a nucleus exists at Manchester; and as the necessity for an extended union of scientific men is strongly felt, might it not be desirable to extend the Manchester organisation into one for supplying the scientific wants of the whole community?

We make this suggestion with the view of eliciting the general opinion of the scientific public. This is a transitional age, and the social elements around us appear to be ripe for such a transformation.

CROOKES'S CHEMICAL ANALYSIS

Select Methods in Chemical Analysis (Chiefly Inorganic). By William Crookes, F.R.S. Illustrated by twenty-two woodcuts. Pp. 468. (London: Longmans and Co.)

THE title of this book fails to convey any adequate idea of its true province. It is not a mere text-book of quantitative analysis after the manner of Fresenius; nor is it, as one might be inclined to suppose, a collection simply of analytical examples designed to illustrate to students the more important determinative methods, as in the well-known and deservedly appreciated "Handbuch" of Wöhler. It aims rather at being a laboratory *Vade-mecum*—a sort of "Chemists' Constant Companion"—designed alike for the teacher and the taught. It presents in a remarkably clear and well-arranged manner a number of thoroughly reliable methods of analysis—some original, others modifications of older and well-known methods—of which the greater portion have been rigidly tested by the author in his own laboratory. Every working chemist must have repeatedly felt the need of a book such as this, and in its publication Mr. Crookes has rendered an important service to the analytical chemistry of the day. The author's connection with the *Chemical News* has doubtless afforded him great and peculiar advantages in the compilation of the materials for his work; indeed, we notice that not a few of the most valuable processes he describes have already appeared in that journal.

Not the least admirable feature in the book (and herein it differs from the ordinary run of quantitative manuals) is the prominence given to methods for detecting and estimating the so-called "rare" metals. Thus we have methods given for the extraction and quantitative separation of lithium, cæsium, and rubidium; cerium, lanthanum, and didymium; glucinum and yttrium, &c. Bunsen's method of analysing platinum ores is also fully described. The discoverer of thallium may justly say that if investigators were more in the habit of looking for the "rare" elements, they would doubtless turn up unexpectedly in many minerals.

But for fear that some "practical man" has already made up his mind about the character of this book, we hasten to say that by far the larger portion of it is devoted to the analytical processes connected with the more important metals and their ores. The sections on iron and copper are particularly complete, all the newest and best methods being minutely described. We may instance Matthiessen's process for the preparation of pure iron, the Mansfield method of copper assay, and Meunier's methods for the immediate analysis of meteoric iron. Under the article "Soda-ash," attention is very properly directed to the absurdity practised in the alkali trade of at one time using the old atomic weight (24) of sodium, and at another the real number in stating the value of soda-compounds, the manufacturer invoicing the strength of his ash in accordance with the basis of calculation which he knows will be employed in reporting on its quality. This custom in many cases amounts to a positive abuse, and is a constant source of vexatious complaint between buyer and seller. The author quotes an instance in which soda-ash of identical quality has been known to be invoiced, part to one customer as containing 48 per cent., part to another as 49 per cent., and part to a third as 50 per cent.; the actual percentage being 48½; the separate consignments being reported also of these different strengths by the analysts in the different towns to which the goods were sent. Inasmuch as soda-ash is usually valued at so much per cent., this amounts to a fraud on the purchaser. Surely the Alkali Manufacturers' Association would consult their real interest by putting an end to such a petty sham as this. But after all this is only on a par with the iniquitous system of "high" and "low" analyses, which is a scandal to the chemistry of the day. *Apropos* of this we would add that those interested in the agitation which has arisen in certain chemical quarters respecting the proper methods for estimating superphosphates and phosphoric acid generally, will find ample details in this work of the reliable processes which have hitherto been devised to that end. Under the head of carbon the method of assaying animal charcoal is described, and this includes an account of Schiebler's calcimeter and the mode of using it; the account of Heinrich's inquiry into the methods for the proximate analysis of coal, which is here introduced, together with the blowpipe assay, constitute one of the most complete things in the book.

In the midst of so much that is excellent it may seem invidious to seek for real or supposed omissions, but we venture to think that a few addenda in a future edition would add to the value of the book. For example, Bunsen's method of preparing pure platinum tetrachloride from scrap platinum, and his simple and expeditious method of recovering this metal from the residues in the process, might be an advantageous addition to the condensed description by Messrs. Teschemacher and Denham Smith of the ordinary method of estimating potash. Without doubt not a few of the errors to which a potash determination made by this method is liable are due to the use of impure platinum. A chapter devoted to the description of useful or improved forms of apparatus, such as the new filter-pump, would also form a valuable addition. Still we must not forget to add that Dr. Carmichael's neat and ingenious method of analysis receives its due share of attention. Nor do we see the reasonableness of making the selection of methods strictly inorganic; for surely the modifications in the process of ultimate analysis introduced by Mr. Warren cannot constitute the sum of the improvements in organic analysis, proximate and ultimate, which have come under the author's notice.

But we have said enough to indicate the character and scope of this work, and imperfect as our sketch is, it will at least serve to show that the book admits of very general application. It will doubtless attain to the popularity which it merits, and the chemical community will thank the author for the worthy contribution he has rendered to its literature.

T. E. THORPE

LETTERS TO THE EDITOR

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

The Sun

IN NATURE for May 18, I find a review by Prof. Newcomb of my book on the Sun, and I beg leave to give some explanations respecting it. It is a pity that Mr. Newcomb has received my book only so lately (about one year after the printing was finished). On this account the criticism which he makes respecting spectrum analysis and the defects of the work on that head is entirely admissible; but this is no fault of the author. Since the book was written sixteen months have elapsed, and during this time great progress has been made in this branch of science, and nobody knows it better than myself, since I find so many things to add to that chapter.

But the criticism seems not so just when he reproaches the work with appearing to undertake to spare the reader the trouble of having recourse to elementary books. At the time of writing, neither of the valuable books which we have now by Roscoe and Schellen were published or had reached me, and the common treatises are very scanty in this respect, so that it was indispensable then, and perhaps even now, to indicate to the general reader the principles of the new science.

In the rapid progress of discovery, a book becomes old very soon, and a reviewer must not forget that printing requires time, and that this work appeared at the very moment of the breaking out of the war, so that it was shut up in Paris for almost another six months.

But a more serious criticism is that which refers to the temperature of the sun. As this is a repetition of M. Zöllner's critique, I beg to give some explanation. It is true that I have assumed that radiation is proportional to temperature without regard to the law of Dulong and Petit, and to the condition of the surface of the body. I did not, however, ignore that law, and I have taken it into account when speaking of Prof. Kirchhoff's discoveries. But here it was quite out of place to refer to it. Indeed, the consequence of this law is that a body may have a very high temperature, and yet radiate but very little; but the law does not state that a body may radiate more than its own temperature allows. When we judge, therefore, of the temperature from the radiation, we certainly commit an error; but so we always judge the temperature from one part of the effect that it is capable of producing; and taking into account the law of Dulong, we should find even a higher temperature in reality in the radiant body, as is the case with the gases.

The conclusion, therefore, to which I have arrived, after Mr. Waterston, is, I think, by no means excessive, but if there is an objection possible to be made, it is exactly in the direction opposite to that of my reviewer. Certainly this conclusion is at variance with that of M. Zollner, but it agrees with the results of other observers. This high temperature besides is really a virtual temperature, as it is the amount of radiation received from all the transparent strata of the solar envelope, and this body at the outer shell must certainly be at a lower temperature.

But this does not prove the incorrectness of my proposition that a thermometer dipped inside the solar envelope in contact with the photosphere, would indicate the enormous temperature that Mr. Waterston has found for the first time.

P. A. SECCHI,
Director of the R. College Observatory

WILL you permit me to make a few comments on Prof. Newcomb's review of my treatise upon the Sun.

Soon after the work had appeared, I was informed that the account I had given of Mr. Stone's treatment of the transit observations in 1769 was not such as Prof. Newcomb would admit to be just. Knowing how much attention Prof. Newcomb has given to this subject, and his great skill as a mathematician, I was prepared to learn that I had misapprehended some points of the discussion between himself and Mr. Stone. I do not even now know to what specific statements of mine he objects; but he may rest assured that my sole object has been, and is, to give a just account of the matter. My account is in agreement with that given by Sir John Herschel, and by Admiral Manners when the Gold Medal of the Royal Astronomical Society was presented to Mr. Stone. I had also inferred from the nature of the discussions between Mr. Stone and M. Faye, and between Mr. Stone and Prof. Newcomb, that the truth lay much as in my narrative. At any rate, those who were present at the meetings of the Royal Astronomical Society in 1869 and 1870 will scarcely think that I have been led by any personal prejudices to advocate Mr. Stone's cause with undue favour, for Mr. Stone's strictures on some of my papers, and especially on papers relating to the subject of the transits of Venus, were severe even to bitterness.

I believe that Prof. Newcomb's mastery of this special subject is calculated to prevent him from rightly judging my treatment of it. He sees it from too near a stand-point, and therefore unduly enlarged. I am sure that on a careful reconsideration of the matter he will feel that I could not have given a fuller account of it than I have, without spoiling the symmetry of my book. Already a seventh part of the letter-press and more than a third of the appendix (besides three plates and twenty-four diagrams) have been given to the subject of the sun's distance. I do not think that more space could very well have been spared. It remains yet to be proved that a single statement in these pages is inaccurate. I deny confidently that the distortion of the limbs of Venus and Mercury in transit has been *proved* to be the product of insufficient optical power. Irradiation must produce such effects to a greater or less extent; and I renew "gravely" my proposal to measure the effect, whatever its cause or causes, during the next transit. I would remind Prof. Newcomb that every observer at Greenwich noticed the effect (more or less) during the transit of Mercury in 1868. Now the Greenwich instruments are not commonly supposed to be utterly imperfect, nor the Greenwich observers wholly unskilled. Even if we admitted both these points, I should still adhere to my proposal. For I have shown in the Monthly Notices of the Royal Astronomical Society for 1870, that those two observations which differed most widely are brought into perfect agreement when the relative breadth of the black ligament observed in each case is taken into account.

Prof. Newcomb has misunderstood my remarks about the D line in the spectrum. I have never concluded, and certainly I have nowhere stated, that "the light of the sodium lines proper is reduced." What I have pointed out is that where those lines fall on the spectrum of the electric light, and where, therefore, we should expect an increase of light, there seems to result darkness. In p. 118 I am careful to use in one place the word *seem*, in another the word *appear*.

I must remind Prof. Newcomb that three countrymen of his own, Professors Harkness, Curtis, and (quite recently) Young, have supposed, with me, that the theory *has* been maintained, that the light of the corona is due to sunlight directly illuminating our atmosphere, and that they and Mr. Baxendell have opposed that theory as pointedly as I have.

I have, however, to admit that some passages "indicating personal feeling" would have been better—much better—omitted. I should have remembered that the explanation of such personal feeling would be unknown to most of my readers. Those who know that because I advocated opinions respecting the corona, which are now all but universally admitted to be just, I was spoken of as "simply making myself ridiculous," will at least acquit me of responding as rudely as I had been attacked. But the generality of my readers had heard nothing of this and other assaults upon me.

I take this opportunity of noting that Dr. Armstrong, of the London Institution, has shown me that in my account of the researches of Mr. Lockyer and Dr. Frankland I have not done the former justice. Some alterations must be made also in my narrative of the work of Dr. De la Rue and P. Secchi in Spain in 1860; much more of the credit of the results then obtained being due to Dr. De la Rue than I had judged from the narrative in P. Secchi's "Le Soleil." Also the enunciation of the aurora theory of the corona must be assigned to Prof. Norton of America.

RICHARD A. PROCTOR

[With respect to the penultimate paragraph of the above letter, we need only refer to our own comments on two previous letters from Mr. Proctor, under date July 7 and August 4, 1870, which we now reprint. ED.:—"For an accurate though incomplete statement of Dr. Frankland's and Mr. Lockyer's theory of the Corona, we refer our readers to the first number of NATURE. Many of them will not be surprised to find that it is *not* what Mr. Proctor states it to be. Dr. Frankland and Mr. Lockyer, from their laboratory experiments, have shown that the pressure at the base of the chromosphere is small, and they have therefore stated that it is scarcely possible that a very extensive atmosphere lies outside the chromosphere. Mr. Lockyer has shown, moreover, that the height of the chromosphere as seen by the new method probably falls far short of its real height as seen during an eclipse as it was seen by Dr. Gould. A reference to the same number of this journal will also show that Mr. Proctor has misrepresented Dr. Gould's statements, which endorse the idea put forward by Dr. Frankland and Mr. Lockyer. Dr. Gould has expressly stated 'that there were many phenomena which would almost lead to the belief that it was an atmospheric rather than a cosmical phenomenon.' This is an opinion held by Faye and other distinguished astronomers, and Mr. Lockyer has simply shown that should this turn out to be the case, the continuous spectrum observed may be explained. Astronomers did not require Mr. Proctor to tell them what he has recently been enforcing; but, more modest than he, they have been waiting for facts, and Mr. Proctor surely is old enough to see that by attempting to evolve the secrets of the universe, about which the workers speak doubtfully, out of the depths of his moral consciousness, he simply makes himself ridiculous, and spoils much of the good work he is doing in popularising the science."—"Still holding to our comments, we gladly state that they were not written in the spirit in which Mr. Proctor has read them. He is known to all as an astronomical worker, and our objection to his mathematical result was that it was based upon data among which the principal point at issue was accepted as proved."]

Rain after Fire

IN Paris, on Wednesday the 24th inst., after describing the terrible conflagrations, one of the correspondents writes thus:—

"A more lovely day it would be impossible to imagine, a sky of unusual brightness, blue as the clearest ever seen, a sun of surpassing brilliancy even for Paris, scarcely a breath of wind to ruffle the Seine. Such of the great buildings as the spreading conflagration has not reached stand in the clearest relief as they are seen for probably the last time; but in a dozen spots, at both sides of the bridges, sheets of flame and awful volumes of smoke rise to the sky and positively obscure the light of the sun. I am making these notes on the Trocadéro. Close and immediately opposite to me is the Invalides, with its gilded dome shining brightly as ever."

Another as follows:—"As I drive along the green margin of the placid Seine to St. Denis, the spectacle which the capital presents is one never to be forgotten. On its white houses the sun still smiles; he will not refuse his beams spite of the deeds which they illumine. But up through the sunbeams struggle and surge ghastly swart waves and folds and pillars of dense smoke; not one or two, but I reckon them on my fingers till I lose the count."

Twenty-four hours later, the change has come. "The rain is now falling heavily, has been falling heavily all day, and may do something for burning Paris. The sound of artillery has died away;" and from another writer:—"A heavy smoke hangs over Paris and rain is constantly falling."

I believe it has often been remarked that rain generally follows a heavy cannonading, but in this case there is an almost unexampled artillery fire and tremendous conflagration at the same time, accompanied by a sudden and violent change in the atmospheric conditions. From where I am writing we noticed a remarkable change on Thursday morning, and about 2 P.M., after intense closeness and oppression, a rain of a tropical character set in for twelve hours or more. On many occasions in Queensland, I noticed that in seasons of drought, after extensive grass fires, causing intense heat, heavy thunderstorms generally followed.

GEORGE PEARCE SEROCOLD

Rodborough Lodge, Stroud, May 27

Alleged Daylight Auroras

SEVERAL letters having appeared in recent numbers of NATURE, giving what the writers consider to have been undoubted instances of aurora visible in the daytime, you will, I hope, allow me to state the reasons why I still adhere to the views expressed in my former communication on this subject.*

And, first of all, I must beg your correspondent Mr. Jeremiah not to think me uncourteous if I dismiss at once, as unworthy of serious criticism, the cases which he has dug out of monkish chronicles. It is likely enough that some of these old records may be imaginative descriptions of nocturnal auroras, and as such they are not without interest, but I cannot admit them as competent witnesses on a point of nicety.

A more modern instance adduced by the same correspondent will be found at p. 7 of NATURE for May 4, under the title "Aurora Borealis, seen in the daytime at Canonmills." In this case it is difficult to know what relation is intended between the title and the account which follows. The account describes the clearing off of the clouds in a mass from the north-west, with the production of an "azure arch," the centre of which "reached an elevation of 20°." If I reply to this that the clearing off of clouds is not an aurora, even though they clear off in a compact body from the north-west, leaving an "azure arch," I may be met by the rejoinder that nobody said it was; and yet I strongly suspect that the writer had some confused idea that he was describing an auroral arch, and I am certain that nine out of ten readers, misled by the heading, would take the same view. Stripped of the cloud-phenomena, all that remains of the Canonmills aurora is the appearance of some "very faint perpendicular streaks of a sort of milky light," which could be traced across the segment of blue sky, but were "extremely slight and evanescent." Considering the probability that the observer regarded the cloud-arch as auroral, which it certainly was not, and considering how his judgment would be likely to be biassed by that idea in the interpretation of "extremely slight and evanescent" appearances, I think we may fairly regard this testimony as particularly weak.

In NATURE for Dec. 8, 1870, Mr. Cubitt describes and figures a double auroral arc which he saw in broad daylight on the 25th October. It was "some 25° above the horizon, and almost due east." In my first letter I expressed a doubt of the correctness of this observation on the ground that auroral arcs are not seen in the east. My criticism has since been challenged on two distinct issues. Mr. Jeremiah insists that an auroral arc may *extend* towards the east, and that what Mr. Cubitt saw may have been the eastern extremity of a northern arc. A reference to Mr. Cubitt's letter and illustration will show at once that if what he saw was any part of an arc, it was the apex and not an extremity. But another correspondent, Mr. Reeks, in NATURE of Dec. 29, 1870, in criticising my remark, makes a statement which is more difficult to answer. He affirms positively that in Newfoundland he has many times seen the arch nearly due east, that is, as he explains, with "the extremities pointing N.N.W. and S.S.E." I would suggest, however, in reply to this statement, that in an extensive auroral display there may be fictitious arches, produced by the accidental correspondence of streamers on either side of the "cupola." An arch of this kind may easily extend from N.N.W. to S.S.E., spanning the entire heavens. It is essentially different from the true auroral arc, which, until much stronger evidence to the contrary is adduced, I shall still believe to be invariably transverse to the magnetic meridian. Obviously, Mr. Cubitt's arc was not of the kind that Mr. Reeks describes.

I pass on to a record of daylight aurora, which, more than any other that I have seen, demands a careful investigation. I refer to "An Account of an Aurora Borealis seen in full Sunshine, by the Rev. Henry Ussher, D.D.," said to be taken from the Transactions of the Royal Irish Academy for 1788, and quoted by the Rev. T. W. Webb in NATURE for May 11. Dr. Ussher's account, it must be admitted, is most particular and complete. He describes "whitish rays ascending from every part of the horizon, all tending to the pole of the dipping needle, where, at their union, they formed a small thin and white canopy similar to the luminous one exhibited by an aurora at night." Nothing can be more precise. But is it not also a trifle too wonderful? Surely, if any part of an aurora is to be seen by daylight, it must be just one here and there of the most vivid beams. That the whole phenomenon should be visible at noon-day in all its completeness, just as at night, even to the faint extremities of the streamers in the magnetic zenith, is to my mind so entirely inconceivable that not even the authority of a doctor of divinity can command my faith in it. I can much more easily believe that the sky presented a remarkably symmetrical arrangement of radiating cirri, and that the observer, impressed by the recollection of the aurora of the previous evening, persuaded himself that the "rays coruscated from the horizon to their point of union." The confirmation by "three different people" is of little value unless their observations were independent.

To those who have no clear conception of the difference between cirrus and aurora, the foregoing arguments will be meaningless. Some persons write very loosely of "luminous cirri," and I have even seen described the transformation of cirrus cloud into aurora as it grew dark. I believe that there is no connection between the two phenomena beyond an occasional and purely accidental similarity of form, and that when the two co-exist, the cirrus, instead of being the seat of the aurora or deriving luminosity from it, only serves to obscure its brightness, and, if dense enough, may appear in the form of dark bands across the auroral light, the latter being, as I conceive, at a very much greater elevation.

I adverted in my former letter to the argument that may be drawn from the non-visibility in the day-time of other lights comparable with the aurora, and I will only now add the following suggestion. If the auroras that occur in this country are occasionally visible in daylight, it might be supposed that the much grander displays of the Arctic regions would be habitually visible in daylight. But is the fact so?

Clifton, May 23 GEORGE F. BURDER

* NATURE, vol. iii. p. 126.

Aurora Australis

TRAVERSING the Indian Ocean 44° S. 65° E., I observed, September 24th, 1870, 4h. till 13h. Greenwich time, a south polar light of great intensity and splendour. After my arrival at Manado (Celebes) I was just writing a few lines about it for the readers of NATURE, with the purpose of knowing whether at the same time an aurora, or at least disturbance of the magnetic needle, had been observed on the northern hemisphere, when I saw in NATURE (Nos. 49, 50, and 51, 1870), several interesting descriptions of aurora borealis observed September 24 in England, &c. I am not aware whether many observations of southern polar lights have been recorded, but I remember that those which Cook described in the year 1773 were coincident with aurora borealis observed in Friesland, and others observed in 1783 at Rio Janeiro were coincident with polar lights in the northern hemisphere. At all events I believe that the attention of men of science is not sufficiently directed to this coincidence of northern and southern polar lights, at least not as much as it deserves in respect to the theory of polar lights at all; and I should be very glad if, in consequence of this notice, authorities would discuss this highly interesting phenomenon in NATURE.

I shall later, according to my diary, accurately describe the display of this splendid aurora australis, and mention the influence which it perhaps or probably had on the abnormal meteorological phenomena, which I observed during the succeeding days.

ADOLF BERNHARD MEYER

Manado (Celebes), January 9

P.S.—I beg to contribute to the records in NATURE of earthquakes, &c., over the whole globe:—

November 20, 1870, afternoon, an at first vertical, then horizontal, rather heavy shock at Manado.

January 28, 1871, 4h., a slight, very local shock in a part of Manado.

Manado (Celebes), March 5

The Eclipse Photographs

As an ardent and not inexperienced votary of photography, I am fully alive to the value of photographic evidence, and regard with enthusiasm each fresh victory which photography achieves, yet I cannot myself look with any very great degree of satisfaction upon the photographs of the late solar eclipse either as examples of photography or as evidence contributing to our knowledge of solar physics. In saying this I make no reflection whatever upon the ability or efforts of those by whom the pictures were produced. On the contrary, I am aware that when these pictures were taken the first grand requisite of photographic success—a clear view of the object to be represented—was scarcely to be obtained. Briefly; from a technical point of view, the pictures are of but indifferent definition, and the identity of the coronal rifts in the Cadiz and Syracuse photographs not satisfactorily conclusive, in addition to which in the picture by the American observers, the so-called coronal light extends a long way over the lunar disc, which seems to me to preclude the possibility of its being other than a phenomenon of terrestrial meteorology. A few weeks ago, when the sky appeared almost cloudless, I observed a beautiful lunar halo, very much resembling the so-called corona, which I apprehend no one would attribute to anything but atmospheric moisture. Why, then, in the instance of a sky burdened with innumerable clouds, should we attribute the halo of light surrounding the solar disc to other than atmospheric causes, even though there should be something which might be mistaken for a coincidence in two distinct photographs of one or other of the rifts which were characteristic of that halo?

Manchester, May 26 D. WINSTANLEY

Eozöon Canadense

PERMIT me to state that the presumed "important bearing" on the so-called "Eozöon Canadense," of the principal fact noticed in the communication entitled Palæozoic Crinoids, which appears in NATURE of May 25th, is discussed in a paper by Dr. Rowney and myself, contained in the forthcoming number of the Proceedings of the Royal Irish Academy, now on the eve of publication. The paper referred to is a reply to the articles by Drs. Dawson and Sterry Hunt, which appeared in the last (second) number of the Proceedings.

Glenoir, near Galway, May 29 WILLIAM KING

WITHOUT going into the vexed question as to whether Eozöon Canadense is or is not of organic origin, I may be permitted to express some surprise at the new, and, to say the least of it, startling theory broached by Mr. Perry in last week's NATURE, of the vaporous formation of a certain limestone. The only facts brought forward in support of this view are, its occupying pockets, its foliations, and its conformation with irregularities of surface in the pre-existing rock. All these could be as well accounted for on the supposition of deposition from aqueous solution, without doing violence to the fact that carbonate of lime is not volatile at any temperature. E. T. H.

THE INEQUALITIES OF THE MOON'S MOTION

THE following is an abstract of the method of computing the inequalities in the motion of the moon which are due to the action of the planets, proposed by Prof. Newcomb in the paper presented to the Academy of Sciences of Paris on April 3.

When we consider the movements of the sun, moon, and earth, under the sole influence of their mutual attraction, the position of each of these three bodies in space will be given in terms of eighteen arbitrary constants, and of the time. The problems of the relative movement of the moon around the earth, and of the movement of the centre of gravity of the earth and moon around the sun, have been solved with a degree of approximation sufficient at least for the purposes of astronomy. Thus, we have the co-ordinates of any two bodies relatively to a third, or relatively to the centre of gravity of the system, in terms of twelve elements and of the time. It only remains to add the expressions for the uniform movement of the centre of gravity in a straight line, to have the general expressions for the co-ordinates of each body.

We have then only to consider the action of the planet to vary the eighteen elements according to the method of Lagrange, to have the movements of each of the three bodies under the influence of the attraction of the planet. Unfortunately, the expressions thus obtained are at first extremely complicated. We have to compute a coefficient corresponding to each combination of the elements taken two and two. The entire number of the coefficients is, therefore, $\frac{17 \times 18}{2} = 153$. And each coefficient contains eighteen products of the partial differential coefficients of the co-ordinates of the three bodies relatively to the elements. These latter differential coefficients are so complex that the formation of any one product would be a considerable labour. The direct formation of the coefficients required is therefore impossible. The paper in question is principally devoted to an explanation of the simplifications which may be introduced into the problem.

It is first shown that all the coefficients formed by combining any one of the six elements which fix the position of the centre of gravity with any of the twelve elements of the relative motion, vanish identically, while the combinations of those six elements with each other give only the principle of the conservation of the centre of gravity. This leaves only sixty-six combinations. It is then shown that, if the elements are divided into two classes, the first class being the mean longitudes, the longitudes of the perigees, and the longitudes of the nodes of the sun and moon, and the second the mean distances, eccentricities, and inclinations, the coefficients vanish whenever the two elements combined belong to the same class. The number of coefficients is thus reduced to thirty-six, and they are simply the differential coefficients of six functions of the elements of the second class. These functions are formed an extremely simple process when we have the rectangular co-ordinates expressed as functions of the elements and the time.

The remainder of the process is simply one of the development of a very complex perturbative function, and is of no especial interest.

THE HELIOTYPE PROCESS

AT one of the recent *soirées* of the Royal Society given by General Sabine at Burlington House, Messrs. Edwards and Kidd exhibited at work the new heliotype process, whereby photographic pictures can be very rapidly copied in by the aid of the printing-press. The process is very inexpensive, and so rapid that if one of the pages of NATURE were sent to the works, it could be copied by photography, and within two or three hours after receipt, pictures could be turned out as fast as the printing-press could work them off. A few days ago I went over the works to examine the process, and a gentleman, who brought an engraving to the proprietors just as I arrived, saw the press printing off very good copies before I left, the interval being about two hours. The works are at some distance out of London, free from the smoke and dust.

The following is an outline of the history of the process:—Mr. Mungo-Ponton, of Clifton, discovered some years ago that if a dried film of gelatine and bichromate of potash be exposed to light, the film is afterwards insoluble in warm water. M. Poitevin afterwards noticed that where light had acted upon such a film, it took greasy ink just like a lithographic stone, whereas those parts on which light had not acted, absorbed water. In the attempt to produce pictures on this principle, he poured a mixture of warm gelatine and bichromate of potash over a lithographic stone, or plate of metal, and when the film was dry he exposed it to light under a negative. Where the light had acted the film became waterproof, and where it had not acted the gelatine swelled up like a sponge. This surface of hills and valleys prevented him from getting good pictures when he attempted to print from it on the lithographic principle.

Messrs. Tesse du Motay and Marechal tried the process just mentioned, and by carefully selecting their subjects, choosing those only in which there was little contrast of light and shade, they reduced the elevations and depressions on the surface of the film to a minimum, and thus obtained some very fair pictures, but after a very few had been printed off, the gelatine printing surface broke up. The next man who took up the process was Albert of Munich. Before his time, whenever a sufficiently thick film of gelatine to stand wear and tear had been used, the elevations and depressions were so great that the film could not be inked. Albert took a plate of glass about half an inch thick, covered it with a thick coating of bichromated gelatine, and after it was dry exposed it all over to light to make it insoluble. Afterwards he covered the surface thus prepared with a very thin coating of sensitive gelatine, on which the picture was printed from the negative. By this process he obtained some exceedingly beautiful and perfect pictures, and he is producing them by this plan at the present time.

Mr. Ernest Edwards took up the process at this point about a year ago. He made a thick leathery film at the outset by adding alum to the warm gelatine solution. He found that films so prepared still retained the lithographic-stone-like property; they will scarcely swell up in water at all. They are insoluble, and they resist the wear and tear of the printing-press very satisfactorily.

The working details of the heliotype process are as follows. The films are prepared upon large sheets of accurately levelled finely ground glass, technically known as "greyed glass"; about 22 inches by 18 inches is a convenient size. The surface of the glass is first polished by means of a clean piece of rag, with a little solution of wax in ether; the exceedingly thin film of wax thus left upon the glass permits the dried gelatine film to come off easily. The glass plates after being waxed are levelled, and then a measured quantity of a warm mixture of gelatine, bichromate of potash, chrome alum, and water, is poured upon each plate from a jug with a piece of muslin tied over its mouth. The temperature of the solution in the jug is about 150° Fahrenheit, and after it is poured over the plate it sets in a very few minutes, but it requires a much longer time to dry. Curiously enough, until it is dry it is not sensitive to light; this fact was found out accidentally, for at first this part of the operations was carefully carried on in yellow light.

After the film has set, the plates are taken into a dark room to dry. If any of the fumes given off by burning gas escape into this room, they act upon the film just as light would do, therefore although a gas stove is used to dry the plates, the products of combustion are very carefully carried off. The gas stove used in the works was invented by Mr. George, a dancing master at Kilburn. It is a closed iron cylinder, into which air is admitted by one pipe coming from outside the house, and the products of combustion are carried off by another. A third iron air pipe enters the bottom of the stove, curves round its sides in a spiral, and then emerges through the iron plate forming the top. Air from outside the house is warmed in this spiral, after which it escapes into the drying-room, which is kept at a temperature of from 90° to 120°. At a temperature of 90° the films take about twenty-four hours to dry. As they dry they contract slightly, and thus separate themselves from the glass. These dried films are technically termed "skins"; they are of an orange colour, and about one-tenth of an inch thick. The picture is printed on them from a negative, and a faintly visible image is formed; when this image is fully out the films are removed to a dark room.

Here each skin is floated in water, and caught upon the surface of a thick plate of zinc; a flat piece of wood, edged with india-rubber is then scraped with considerable pressure over the film, so as to squeeze out all the water between the skin and the zinc. As the film still continues to absorb moisture, it is thus fixed to the zinc with the whole pressure of the atmosphere. After this the zinc with its attached film is left for half-an-hour at least in a large vessel of water, for the superfluous bichromate of potash to soak out, and then the film is no longer sensitive to light. If the film be thus soaked for several hours, or even days, it does not suffer.

The film, upon its zinc plate, is now ready for the printing press. It is damped between each impression, just like a lithographic stone. Then it is inked, and the best roller for the purpose is found to be one made of india-rubber, backed inside with "india-rubber sponge" to give additional softness. Ordinary lithographic ink is used. If stiff lithographic ink be employed, the surface will only "bite" where light has acted most; if thin ink be used, the leathery surface will only bite in the half tones of the picture; hence each picture is produced by at least two inkings, and advantage is taken of this circumstance to use two colours, and get warm shades in the half tones. It is very interesting to see the picture gradually growing under the inking process. By this method double-printing is executed with a single pull at the press. Ordinary Albion hand printing presses are used.

The negatives worked from in this process have to be "reversed," and they may either be reversed at the time they are taken, or afterwards. In the former case, instead of the lens of the camera being pointed direct at the object or picture to be photographed, a mirror, silvered on its front surface, is interposed at an angle of 45°. Another method of reversal is to take an ordinary unvarnished negative, and coat it either with a solution of india-rubber, or a solution of gelatine and alum. When the film is dry the plate is accurately levelled; it is then coated with a pool of collodion as thick as it will hold, and this collodion is then allowed to dry. Next the film is cut through with a penknife near the edges of the picture, and the plate is placed in water, where the negative soon floats off the glass, after which it is dried between blotting paper. The flexible negatives thus obtained are very durable, except when bad india-rubber is used in reversing them.

When a batch of pictures has been printed from any particular skin, the film is taken off the zinc plate, and put away until wanted again. Mr. Edwards says the skins will stand a vast amount of wear and tear, and he showed me one from which he said 1,500 pictures had been printed, the last impression being as good as the first, and the skin ready for further work if necessary.

By this process many of Mr. Nasmyth's lunar pictures have been copied, and while on the premises I saw some work then being executed for Mr. Ruskin, and others known in the world of art and science. Bones, and some descriptions of anatomical specimens, are very easily photographed and printed by this process, which is also well adapted for landscapes and architectural subjects. If it be desired, a glaze is given to the finished prints in a very simple way. A little powdered magnesia is sprinkled over the surface of the print, and it is then placed on a smooth board and rubbed with a pad of flannel. Magnesia belongs to the soapstone family, and when used in this way it very readily gives a surface polish to paper.

WILLIAM H. HARRISON

PARIS NEWS

FOR some time past the Académie des Inscriptions et Belles Lettres has held no regular sitting, almost all the members being refugees in Versailles or elsewhere. A special commission has been given to M. Rénan, one of its most distinguished members, to inspect the ruins of the Parisian monuments which have been destroyed by the Communists. M. Rénan, before publishing his last books on religious matters, had been sent to Mesopotamia to do the same work as Mr. Layard. Private letters received from the distinguished commissioner have been read before a group of members of different academical bodies at Versailles, sitting almost in an official capacity as the Academy for Inscriptions and Belles Lettres. It was stated that the Louvre buildings had escaped, and the bulk of artistic works will be saved from the conflagration. But the private Imperial Library in the old building of the Ministry of State has been destroyed. The value of this collection was chiefly historical, a number of the volumes being of special value from the fact of their having been presented to the several Kings and Emperors of France during the last three centuries. There were also some manuscripts of value, and collections of drawings for the study of art in the Museum. It was intended to open it shortly as a special art library for the use of students at the Louvre. The National Library, formed by Richelieu, was not burned down as has been rumoured; the building has entirely escaped. But it appears that steps had been taken by the insurgents to destroy it like the Serapion was at Alexandria when Omar took possession of the city. The Luxembourg buildings and Museum were saved only by the prompt exertions of the troops, when the insurgents were actually setting fire to them. The Luxembourg holds within its precincts a valuable library, where have been collected parliamentary documents from every nation and of every period. It was said to be the most valuable in the world in this respect. The collection of pictures is the richest in the world for works of living painters belonging to the French school. Courbet, the member of the Commune, had not been admitted to this, esteemed the highest honour by French artists. The Sorbonne is almost entirely saved, the walls only having been pierced by gun-shot or shells. The collections are most valuable, and very serviceable for students. We have no special news from Sainte Geneviève, a library largely used for law purposes on the Place du Panthéon; but it is supposed that the library is safe, as the insurgents were prevented from exploding it, though an immense quantity of powder had been deposited in the cellars, and it was used as an arsenal during the whole of both sieges. The Institute is safe, although it appears steps had been taken for its destruction. The Mazarine Library close to it is most valuable for works of the 17th and 18th century, as well as the library of the Arsenal. But according to every probability this last establishment will be entirely lost, owing to the vicinity of the *Grênier d'abondance*, a place where an immense number of goods were collected, and which was ignited. A commission of the Academy of Sciences will be issued to study the different processes used by the insurgents for burning the Tuileries, Palais Royal, &c. Hay, soaked with petroleum, appears to have been very often resorted to, as well as canisters full of the same substance. In some instances petroleum had been poured from outside into the cellars, and an ignited match thrown into the impregnated air. The stories of firemen throwing petroleum from fire-engines are, we are happy to say, unfounded.

DREDGING OF THE GULF STREAM

WE are much gratified to learn from *Harper's Weekly* that preparations are now being made, under the direction of the Superintendent of the Coast Survey, for a very complete and thorough investigation of the deep-sea bottom, and especially of the channel of the Gulf Stream off the eastern coast of America, with an examination also of the Straits of Magellan and of a part of the Pacific Ocean. A steamer is now being built, which will shortly be launched, with the special object of continuing the deep-sea dredgings which, under the direction of Count Pourtalès, have given the Survey so much reputation.

It is expected that the arrangements will be completed by the end of August, and that the whole matter will be specially in charge of Prof. Agassiz, assisted by Count Pourtalès, whose experience eminently qualifies him for the post.

The plan of operations is, first, to run a line of dredging across the Gulf Stream between New York and Bermuda, and, if necessary, far enough eastward to completely cross the Gulf Stream current. The course will be thence to Trinidad, where a careful examination will be entered into to ascertain whether there is any difference in the deep-sea fauna of the adjacent waters and that of the coast of Florida. The expedition will then probably proceed to San Paulo for the purpose of examining the deepest known portion of the Atlantic, reaching to, at least, five thousand fathoms. From San Paulo it will again cut across the Brazilian current, and after possibly spending some time on the coast between Buenos Ayres and the Straits of Magellan will proceed by a zigzag course to the Falkland Islands, in the neighbourhood of which the expedition will remain for some time, for the purpose of solving certain important problems relating to both the deep-sea fauna and to that of the coast. It is next proposed to spend, at least, a month in the Straits of Magellan during the summer season of that portion of the globe. The work at the Straits being completed, the party expect to pass up along the western coast of Chili, next to the island of Juan Fernandez, and thence across to Callao. From this point the course will be to the Gallapagos, and thence across the Chilian current to some point on the west coast of Mexico—possibly to Mazatlan. The Revillagigedo Islands will next be visited, whence the party will proceed to San Francisco.

The entire exploration will probably occupy ten months, and bids fair to be the most important attempt ever made at determining the character of the fauna of the deep seas. The experience gained in all the former American and foreign expeditions of this kind will be freely used on this occasion; and no pains will be spared in the way of outfit to render the whole undertaking an entire success.

The fact that this expedition is under the direction of the Coast Survey is sufficient guarantee that nothing will be neglected to secure satisfactory results in the way of investigations upon the physics of the ocean, as well as its natural history, as it is intended to make use of the most approved apparatus for the determination of depths, temperatures, specific gravity, and chemical composition of the waters, &c.

THE SPECTRUM OF URANUS *

IN the paper "On the Spectra of some of the Fixed Stars," † presented conjointly by Dr. Miller and myself to the Royal Society in 1864, we gave the results of our observations of the spectra of the planets Venus, Mars, Jupiter, and Saturn; but we found the light from Uranus and Neptune too faint to be satisfactorily examined with the spectroscope.

By means of the equatorial refractor of 15 inches aperture, by Messrs. Grubb and Son, recently placed in my hands by the Royal Society, I have succeeded in making the observations described in this paper of the remarkable spectrum afforded by the light of the planet Uranus.

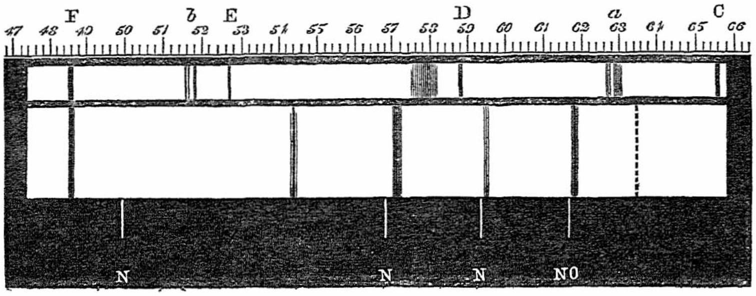
It should be stated that the spectrum of Uranus was observed by Father Secchi in 1869.‡ He says, "Le jaune y fait complétement défaut. Dans le vert et dans le bleu il y a deux raies très larges et très noires." He represents the band in the blue as less refrangible than F, and the one in the green as near E.

The spectrum of Uranus, as it appears in my instrument, is represented in the accompanying diagram. The narrow spectrum placed above that of Uranus gives the relative positions of the principal solar lines, and of the two strongest absorption-bands produced by our atmosphere, namely, the group of lines a little more refrangible than D, and the group which occurs about midway from C to D. The scale placed above gives wave-lengths in millionths of a millimetre.

The spectrum of Uranus is continuous, without any part being wanting, as far as the feebleness of its light permits it to be traced, which is from C to about G.

On account of the small amount of light received from this planet, I was not able to use a slit sufficiently narrow to bring out the Fraunhofer lines. The positions of the bands produced by planetary absorption, which are broad and strong in comparison with the solar lines, were determined by the micrometer and by direct comparison with the spectra of terrestrial substances.

The spectroscope was furnished with one prism of dense flint-glass, having a refracting angle of 60°, an observing telescope magnifying 5½ diameters, and a collimator of 5 inches focal length. A cylindrical lens was used to increase the breadth of the spectrum.



The remarkable absorption taking place at Uranus shows itself in six strong lines, which are drawn in the diagram. The least refrangible of these lines occurs in a faint part of the spectrum, and could not be measured. Its position was estimated only, and on this account it is represented in the diagram by a dotted line. The positions of the other lines were obtained by micrometrical measures on different nights. The strongest of the lines is that which has a wave length of about 544 millionths of a millimetre. The band at 572 of the scale is nearly as broad but not so dark; the one a little less refrangible than D is narrower than the others.

The measures taken of the most refrangible band showed that it was at, or very near, the position of F in the solar spectrum. The light from a tube containing rarefied hydrogen, rendered luminous by the induction-spark, was then compared directly with that of Uranus. The band in the planet's spectrum appeared to be coincident with the bright line of hydrogen.

Three of the bands were shown by the micrometer not to differ greatly in position from some of the bright lines of the spectrum of air. A direct comparison was made when the principal bright lines were found to have the positions, relatively to the lines of planetary absorption, which are shown in the diagram. The band which has a wave-length of about 572 millionths of a millimetre is less refrangible than the double line of nitrogen which occurs near it. The two planetary bands at 595 and 618 of the scale appeared very nearly coincident with bright lines of air. The faintness of the planet's spectrum did not admit of certainty on this point; I suspected that the planetary lines are in a small degree less refrangible. There is no strong line in the spectrum of Uranus in the position of the strongest of the lines of air, namely, the double line of nitrogen.

As carbonic acid gas might be considered, without much improbability, to be a constituent of the atmosphere of Uranus, I took measures with the same spectroscope of the principal group of bright lines which present themselves when the induction-spark is passed through this gas. The result was to show that the bands of Uranus cannot be ascribed to the absorption of this gas.

There is no absorption-band at the position of the line of sodium. It will be seen by a reference to the diagram that there are no lines in the spectrum of Uranus at the positions of the principal groups produced by the absorption of the earth's atmosphere.

WILLIAM HUGGINS

* From the Proceedings of the Royal Society.

† Phil. Trans. 1864, p. 413; and for Mars, Monthly Notices R. Ast. Soc. vol. xxvii., p. 178.

‡ Comptes Rendus, vol. lxviii. p. 761, and "Le Soleil," Paris, 1870, p. 354.

NOTES

WE understand that the contributors to the next volume of the *Zoological Record* are as follows:—Mammalia, Reptilia, and Pisces, Dr. Albert Günther, F.R.S.; Aves, Mr. H. E. Dresser, F.Z.S., and Mr. R. B. Sharpe, F.L.S.; Mollusca, Molluscoidea, and Crustacea, Dr. Edward von Martens, F.M.Z.S.; Arachnida and Myriapoda, Mr. O. Pickard-Cambridge, C.M.Z.S.; Insecta generally and Coleoptera, Mr. E. C. Rye; Lepidoptera, Mr. W. F. Kirby; Diptera, G. A. Verrall; Neuroptera and Orthoptera, Mr. M'Lachlan; Rhynchota, Mr. John Scott; Vermes, Mr. E. Ray Lankester; Echinodermata, Cœlenterata, and Protozoa, Prof. Traquair. Respecting the work and the association which has been formed to prosecute it, we spoke at some length a few weeks ago, and will only remind our readers that the secretary of the Zoological Record Association is Mr. H. T. Stainton, F.R.S., to whom all applications for further information on the subject should be addressed.

WE are glad to see that the higher examination for women at the University of London has this year been successfully taken by one candidate in the department of Natural Philosophy and Chemistry. She is from the Cheltenham Ladies' College.

WE have received the Anniversary Address of Sir Roderick Murchison, as retiring President of the Royal Geographical Society, dealing with the history of geographical progress during the past year.

A LETTER has been received from Dr. Hooker from Mogadore, bringing down the narrative of his journey to April 26. The party had visited Ceuta, Gibraltar, and Casa Blanca, but had found no very striking novelties since the last report.

MR. THOMAS MOORE, the Curator of the Chelsea Botanic Gardens, is now delivering in the gardens a course of six lectures on Botany for medical students.

A SCIENTIFIC society has been formed at Middletown, Connecticut, and Dr. John Johnson, of the university, elected president, Prof. Rice, corresponding secretary, and Prof. John M. Van Vleck, treasurer.

PROF. NEWTON of Cambridge is engaged upon a revised edition of Yarrell's well-known "History of British Birds," which has long been justly regarded as the standard work on the ornithology of these islands. It is now more than thirty years ago since the late Mr. Yarrell began the first edition of this book, and as the literature of the subject has in that time been doubled if not trebled, the editor will have enough to do to bring the work up to the level of the information of the present day. It is announced that the mode of publication will be in monthly numbers, the first of which may be expected in the course of a few days.

AT a recent meeting of the Asiatic Society of Bengal, Col. Strachey made a communication to the effect that the Government of India have lately resolved to place four lacs of rupees in deposit, which sum should be available for completing the new Museum building at Calcutta. He regretted the delay which has been caused in the construction of the building, and stated that it was greatly due to the financial difficulty in which the Government of India found themselves a short time ago. Col. Strachey mentioned that the original approximate estimate amounted to about three and a half lacs of rupees. This sum had been sanctioned by Government, and the work for the new building was commenced. Subsequently the regular estimate came up, and it amounted to about seven lacs. After about four lacs had already been spent, a revised estimate was called for, and this rose to about ten lacs. It was, therefore, not surprising that the Government stepped in and inquired into the whole matter carefully, and this caused such a delay that it became impossible to complete the Museum within the appointed time, 23rd March, 1871. However, Col. Strachey hoped that the present action taken by Government in the matter would bring the building to its desired completion at as early a date as possible.

AT Plymouth, one of the largest provincial centres of science instruction in connection with the South Kensington department, this year's examinations have just been brought to a close, the arrangements having been carried out under the direction of the School Board for the town, in compliance with the request of the department in their circular of March 7th In accordance with it the School Board appointed their clerk, Mr. Henry Soltan, as the examination secretary, and the assistants were selected principally from the secretaries of the various Science and Art Schools of the town. These gentlemen have been occupied every evening during last month, up to Monday last (22nd), in superintending the conduct of the various examinations which have all been held at the Public Free School, and which have comprised no less than twenty-two subjects in science, besides the four branches of art study, of the second grade, the examinations in which in previous years have been held in March. The details show that a total of 846 papers has been worked, 257 in Art and 589 in Science. The largest numbers of papers in any special subjects were Physical Geography 151, and Freehand Drawing, and Mathematics, stages 1, 2, and 3, each 112.

A CONSIDERABLE portion of modern astronomical literature appears to be designed to bring science into contempt. A paper has been circulated, containing an "extract from a letter addressed to John Hampden, Esq., Chippenham, Wilts," attributing the death of Sir John Herschel to "the chagrin he has for some time evinced at the severe attacks recently made on the astronomical theories with which his name has been so long and prominently associated." It is only the most uncleanly of animals that howl around the bodies of the dead; we should have thought that any "gentleman" receiving such a letter would have consigned it at once to the fire instead of sowing it broadcast over the land.

THE Scotch papers report a mirage at the mouth of the Forth on Sunday the 21st inst. The weather was remarkably warm, and in the afternoon there was a dull deceptive haze. The sea presented almost the appearance of a mirror, and the vessels upon it seemed to have a double reflection from the sea and the background beyond. At one time the masts and rigging seemed elongated to four or five times their natural length, and then in the course of a few minutes they were reduced so as to be scarcely visible. At other times the vessels appeared to be sailing double—one ship in sea and one in air. Extraordinary appearances were assumed by the May Island, which rose and fell and changed to all manner of shapes in the course of a few minutes. At one time it appeared a perpendicular wall, rising to the height of several hundred feet, and shortly afterwards it appeared to be flat on the surface of the sea All the other objects which came within the range of the refraction underwent similar changes, and the illusion lasted with varying features for several hours.

DR. MARSHALL HALL has contributed to the *Devizes Gazette* a letter, calling attention to the importance, at the present time, of establishing local centres for instruction of farmers in agricultural chemistry and sciences connected therewith. In connection with the circular of Mr. Little's, printed last week, we hope the subject is now at length attracting the attention it deserves.

THE *Engineer* of last week contains a drawing of the machinery used in the casting of the Vendôme Column, lately thrown down by the Paris Communists.

AT Derajat, in India, a supply of water has been obtained by artesian boring at the depth of 400 feet.

"TO the Rt. Hon. Earl Granville, K.G., H.M.'s Secretary of State for Foreign Affairs," &c., &c. Suppose there were addressed to his Lordship a rather pressing letter from the Anthropological Society of Honolulu or Bolivia, requesting him to furnish male and female skulls of each of the tribes of Irish, Welsh, Erse, Lowlanders, Danes, English, &c., inhabiting these islands; a special application made for skulls from St. Giles's, and the Liberties of Dublin; skulls of extinct tribes earnestly sought; and his Lordship expected to supply a consignment of 200 or 300 skulls, besides their flint and other implements, tobacco pipes, pottery, and so forth, ancient and modern. What would be the commotion here, and what the comments in our newspapers! and yet such a request has been made by the zealous Secretary of the Smithsonian Institute to the Government of the United States of Colombia, and he is likely to prefer the same hint here, now that the *Alabama* claims are in happy way of settlement. The Colombian Government has taken the matter coolly, and published the application in the *Gaceta Oficial*, calling to it the attention of the local authorities. How the local authorities will persuade the local Indians to hand over the solicited pair of recent male and female skulls, appears rather doubtful and dangerous, as will be the polite request of the resident magistrate, or other authority, to the tribes of Achill Island or Connemara. Some of the Indian tribes in Colombia are much more likely to place the skulls of the Government or their emissaries in their own local museums.

IT is stated from India that a number of experiments have shown that the madar plant, when mixed with opium, is an excellent substitute for ipecacuanha in dysentery.

THE Kava or Ava is well known as a favourite intoxicating drink of the South Sea Islanders. The extraordinary and disgusting mode of preparing this beverage, by chewing the root, ejecting the saliva into a bowl and fermenting it, has been the means of giving it a greater amount of publicity than it would have otherwise obtained. Many stories have been told about its uses and effects. It appears that at one time before the intercourse with foreigners the only intoxicating drink known to the natives was the water in which the roots of the kava plant (*Macropiper methysticum*) had been macerated, and this was comparatively little used, except as a medicine, as it was supposed to prevent corpulence. Since the introduction of foreign spirits into the islands the use of kava has much diminished, though intoxication is none the less common, and with many of the natives kava is still much appreciated, and even many of the lower classes of white people are confirmed kava-drinkers. It is said that, if drunk in excessive quantities, it produces numerous cutaneous diseases, but if taken in moderation has no ill effect upon the system. A drink, prepared in a similar filthy manner, is the South American Piwarri, which is produced by first chewing a sufficient quantity of cakes made of cassava meal (*Manihot utilissima*), and then putting the masticated material into a bowl with water, where it is left to ferment for some days, and finally boiled.

SO much has been said about the adulteration of butter and the frequent substitute of a compound for that useful article, in which no trace of true butter exists, that the introduction of Australian butter into this country is a matter not only of commercial importance, but must also give general satisfaction to all classes. The butter is of good quality, and arrives after its lengthened voyage in good condition. It is produced in different parts of the Australian colony, so that we may hope to receive large and continuous supplies. It is a fact worth noting that whereas at one time large supplies of butter were exported from Cork into the colonies, one of these same colonies is now sending its produce to us.

THE cultivation of the beet in England for the manufacture of sugar seems in a fair way of at last becoming a *fait accompli*. Besides the works already in operation at Lavenham in Suffolk, Buscote in Berkshire, and other places, a new beetroot sugar company has ust been formed under good prospects at Sandwich Kent.

PROF. WYVILLE THOMSON'S INTRODUCTORY LECTURE AT EDINBURGH UNIVERSITY

(*Continued from page 76*)

THE distinction between inorganic bodies and organised beings instinct with life seems clear enough. Between the animal and the vegetable kingdoms it is impossible to draw a definite line. It is to solve this difficulty that Ernst Haeckel has proposed his fourth kingdom, and we have now to consider whether or not the solution is satisfactory or legitimate.

Plants have the power of secreting and storing in organs which are specially fitted for its reception, usually the leaves, a substance called "chlorophyll," by means of which, acting probably as a ferment or in some way which is not yet thoroughly understood, the plant can, under the influence of light, absorb carbonic acid from the atmosphere, decompose it, and, while the carbon is in the nascent condition, combine it with the elements of water, or with the elements of water and of ammonia, reduced likewise to the nascent state by the same agency. The plant thus gains from the inorganic world, from the water contained in the soil and the inorganic substances dissolved in it, and from the atmosphere, various elementary substances, chiefly, however, oxygen, hydrogen, carbon, and nitrogen, and these it recombines into ternary, quaternary, and still more complex organic compounds.

The operations which take place within the chlorophyll cell of the plant are briefly these:—

1. Water, $H_2 O$, which has been absorbed by the root of the plant from the soil, and pumped up by endosmosis or capillary attraction to the leaf, is decomposed, and its elements are reduced to the nascent condition.

2. Carbonic acid, $C O_2$, which exists as a gas in the atmosphere to the amount of about 1 vol. to 2,500 of air, and which is consequently in contact with the surface of the leaves, is absorbed, and its elements are reduced to the nascent state.

3. Ammonia, $N H_3$, an abundant product of the decomposition of organised bodies, which exists in small quantities in the water of the soil, and much more abundantly as a gas in the atmosphere (in the proportion of about 1 vol. to 1,000,000 of air), is decomposed, and its elements are reduced to the nascent state.

4. The nascent carbon of the carbonic acid is combined with the elements of water in varying proportions, and by the re-combination of the elements of these binary compounds, ternary compounds; for example, cellulose, starch, dextrine, and gum, which have all apparently the same composition, $C_6 H_{10} O_5$, or some multiple of these proportions; sucrose or cane sugar $C_{12} H_{22} O_{11}$; dextrose and levulose, grape and fruit sugar, $C_6 H_{12} O_6$; the glucosides, such as tannine, $C_{27} H_{22} O_{17}$; the fixed and essential oils, some of which latter, however, as turpentine and its isomers, are binary, $C_{10} H_{16}$; and many like substances, are produced.

5. The nascent nitrogen of the ammonia, sulphur, which is probably derived from a trace of sulphuretted hydrogen in the atmosphere; phosphorus, derived from the decomposition of certain minerals contained in the soil or of vegetable or animal matter, are united to form quarternary, quinary, and more complex compounds, such as vegetable albumen, fibrin, casein, and protoplasm.

	Albumen.	Casein.	Fibrin.
Carbon	53·5	53·8	52·7
Hydrogen	7·0	7·2	6·9
Nitrogen	15·5	15·6	15·4
Oxygen	22·0	22·5	23·5
Sulphur	1·6	0·9	1·2
Phosphorus	0·4	0·0	0·3

and the vegetable alkaloids, for example, nicotine $C_{10} H_{14} N_2$, morphine $C_{17}H_{19} N O_3 + H_2O$, and narcotine $C_{22} H_{23} N O_7$. Under the guidance of the vital property, and through the medium of a peculiar substance called protoplasm, whose exact composition it is difficult to determine, but which seems to be closely related to albumen, which is constantly present where vital actions are going on, and in which alone apparently the peculiar property called life resides, these different substances are adjusted as to their related proportions, and selected and applied each to its destined object in the plant economy, to enlarge the cell or to strengthen the cell-wall, to lay the foundation of a new cell, to be stored up in a special reservoir for future use, to contribute, in short, to the development or maintenance of the special specific form of the organism of which it forms a part.

A plant cannot assimilate pure carbon, or hydrogen, or nitrogen; it seems that it can assimilate no elementary substance except oxygen, unless it be presented to it in the nascent condition. An animal stands in precisely the same relation to the binary compounds, carbonic acid, water, and ammonia. However abundantly, therefore, it might be supplied with these binary compounds which actually contain all the elements necessary for its sustenance, it would surely die of inanition. In order to be capable of affording nourishment to the animal kingdom, these substances must be elaborated to the condition of ternary and quaternary compounds, and this can only be done in the cells of plants. This, then, is the broad and practical distinction between the vegetable and the animal kingdoms. Plants have the power of absorbing, modifying, and organising inorganic substances, while animals are entirely dependent upon the organic substances thus prepared for their support. Taken in this sense, the distinction between the two kingdoms is most marked, and of the highest practical value; but when we set aside this one peculiar property, which is possessed only by some plants, and only by certain parts of those plants at certain periods of their life, and especially when we observe certain minute forms, of low organisation, on the verge of either kingdom, it becomes absolutely impossible to assign any definite distinctive character. The character which is, perhaps, most palpable and universal, is that a mass of vegetable protoplasm is at some time during its existence, inclosed in a cell-wall, which is composed of cellulose, or some very nearly allied ternary compound. Animal protoplasm is rarely, if ever, confined in this way; that is to say, in nucleated cells, with cellulose walls, which are found in all plants, and are not found in the animal kingdom.

The protoplasm of the cells of plants has the power, without developing colour and without the aid of light, of absorbing, decomposing, and assimilating the elements of already prepared organic ternary and quaternary products, whether they be derived from the soil or surface to which the plant is attached, as in the case of fungi; or from sap already elaborated by another part of the same plant, as in the growing root; or even of another plant, as in pale parasites. Most growing points in plants are pale, and the protoplasm in the cells of a pale shoot, or of a colourless plant, has precisely the same vital powers and relations as animal protoplasm. It is only in cells in which protoplasm elaborates and incorporates with itself endochrome, which seems to be a more powerful catalytic agent, capable of disengaging the component atoms of the more stable binary compounds when loosened by the vibrations of light, that the special function of the vegetable cell is performed. The cells of a pale growing point develop the characteristic cellulose wall, but the supply of material is abundant, for the protoplasm of these cells is either confluent, through the porous cell-walls, with the protoplasm of chlorophyll cells, or, as in the seed, it is in connection with a *cache* of preserved food, prepared and stored up by the protoplasm and endochrome of previous leaves.

The difference between the great mass of plants, as represented by their familiar and higher groups, and the higher animals, is very palpable; it is only among the comparatively obscure forms near the limits of either kingdom that a difficulty occurs. The general chemical composition of plants differs markedly from that of animals. A plant consists mainly of ternary compounds, cellulose, dextrine, starch, &c. In an animal, ternary compounds, although some of them—such as oils and fats—are of great importance in its economy, play altogether a subordinate part, and the bulk of the body is made up of substances of the albumen series—albumen, fibrin, and gelatin. Until lately this chemical distinction was regarded as absolute, and still it holds good generally, though glycogen and glucose (animal starch and sugar), and now recognised as being universally present in the tissues of the higher animals whether in health or in disease. A colouring matter apparently undistinguishable from chlorophyll is found in the green bodies of Hydra and Stentor, though there is as yet no evidence that this animal endochrome possesses the power of decomposing carbonic acid; and cellulose, perhaps the most special of the vegetable products, is found in the testa of ascidians. Endochrome is absent in many fungi.

The intimate structure of plants is usually very different from that of animals. Plants and all their parts consist of but one histological element, the cell with a cellulose wall, and its very simple modifications; the texture of plants is therefore to a great degree homogeneous. Animals, on the contrary, consist of many tissues highly differentiated, among which the nucleated vesicle with a definite wall, and tissues simply derived from it, are comparatively rare. The structure test fails also, however, on the borderland, for the most simple animals, such as Amœba and Gromia, are mere minute masses of jelly-like sarcode, which show no structure and no differentiation of tissues, and seem to differ only from the unicellular fungi and algæ in the absence of the cellular wall, while the free amœbiform cell-contents of the myxomycetous fungi are perfectly undistinguishable from such animals without a knowledge of their history.

The general plan of the organs of nutrition is strongly contrasted in the two kingdoms. One of the higher plants absorbs its peculiar food and assimilates it by means of an enormously extended external surface of roots and leaves. An animal, on the other hand, receives its prepared nourishment into the interior of its body by a mouth, and then subjects it to complicated processes of digestion and assimilation in contact with an extended internal absorbing and secreting surface, of which special portions are organised for the performance of the several steps in the process, till at length the unassimilable residue is rejected; but as we descend in the animal series, the digestive cavity becomes more and more simple, and in certain undoubted animal forms, such as the cestoid worms, the gregarinæ, and the foraminifera, it is entirely wanting, and nutrition is effected by absorption through the external surface as in plants.

No special character can be derived from the function of reproduction. Most plants, and many of the lower animals, are multiplied by gemmation and fission, and probably all animals and plants are propagated under certain circumstances by a nearly uniform process of sexual generation. The reproductive elements are produced internally in the higher animals and externally in plants; but even this minor distinction fails very early, for in large groups of the simpler animals ovaries and testes are external.

It is true in the general sense that animals possess organs and functions of relation which are absent in plants. Most plants remain permanently rooted to the ground, and neither the whole plant nor any visible part of it exhibits any spontaneous movements, either voluntary or automatic. It never performs any independent or consequent acts, from which one would deduce the existence of consciousness, intelligence, or will; still, there are certain phenomena even among the higher plants, connected with the habits of climbing plants and with the functions of fertilisation, which it is very difficult to explain without admitting some low form of a general harmonising and regulating function comparable to such an obscure manifestation of reflex nervous action as we have in sponges and in other animals in which a distinct nervous system is absent. The protoplasm in the interior of the vegetable exhibits movements so characteristic and special as almost to be sufficient, were other evidence wanting, to prove its absolute identity with the sarcode of the rhizopods; and when we reach the confines of the two kingdoms, the test of locomotion fails like the others, for the branched and plant-like sponge remains permanently rooted to the ground, while the freed cell-contents of many of the lower plants move actively, either by contractility of the sarcode substances, as in the plasmodia of the myxomycetous fungi, or by circlets or bunches of cilia, as in the zoospores of *Volvox*, *Euglena*, and *Chætophora*.

If we take a water-reed from a pond in summer, and carefully scrape off the slimy matter adhering to it upon a slip of glass and place it under a microscope, we may probably see in the field some minute oval bodies like the small seeds of a plant. These are the bodies of an animal belonging to the genus Gromia, a genus of the Rhizopoda, grouped among Haeckel's Protista, but usually regarded as true animals. If we break up one of them under the microscope, we find it to consist of a little mass of apparently perfectly structureless viscid semi-fluid jelly inclosed in a thin membranous oval shell, with a large opening at one end. This gelatinous mass is under the highest powers destitute of anything which can be called structure. A transparent colourless matrix contains extremely small globules and fine granules scattered through it, and here and there are rounded spaces, which seem to contain a homogeneous liquid. If instead of breaking up the animal we allow it to remain quiet in the water, probably in a few minutes we see a set of very delicate threads protruded from the opening in the test. These threads increase in length, and spread like a branching root in the water. When we examine these closely, we find that they are continuous with the jelly of the animal, that they are, in fact, mere processes of that jelly. When two of the threads touch they flow together, and coalesce, as two streams of treacle might do, showing that they are bounded by no membranous wall; and, when one of the threads comes in contact with an organic particle in the water, the particle sinks into it, and then the thread begins to flow back again into the body of the animal bearing the particle with it, as a stream of treacle might entangle, and carry along a crumb of bread.

The organic particles are introduced into the body, into any part of it, and there they are dissolved and assimilated. I believe that the granules observed in the gelatinous substance are particles of the various products of this assimilation, and that the living matter is perfectly homogeneous and transparent. If the creatures be kept for a few days in water nearly pure, they become less and less granular.

If the thread-like pseudo-podia, as they are called, be rudely touched, they at once contract, and flow rapidly back into their test. The membranous test cannot be truly regarded as a part of the animal, it is a mere excreted defensive covering incapable of any further change, or of manifesting any of the phenomena of life. The body of the animal can be easily squeezed out of it entire, and in that case it shortly begins the excretion of a new shield.

Here, then, we have a homogeneous substance which has the power of inducing and controlling chemical and physical forces, and of moulding into indefinite form the products of the regulated changes taking place within it, which therefore possesses life. The gelatinous matter which in this animal and in the whole sub-kingdom to which it belongs can thus feed and digest without a mouth or stomach, contract without muscles, display irritability without a nervous system—in fact, exhibit all the essential phenomena of living beings without a trace of organisation, is Protoplasm.

If now, laying aside the Gromia, we examine with the microscope the water-plant on which we found it, we find that the whole plant from end to end and in all its parts is honeycombed, that is to say, composed of a congeries of minute chambers separated from one another by well-defined walls, the walls giving the plant its support and consistency.

We place in the field of the microscope a small portion of the growing point of a leaf or stem, and we easily make out that the chambers are minute vesicles each complete in itself, adhering according to a definite arrangement to one another. As these cells have occupied a very prominent position in modern histological and physiological speculation, having been regarded, and being still regarded by many as the units of organisation, the centres and sources of all vital activity, I should wish to sketch distinctly their structure and properties. It is of no consequence whence the cell is selected. All vegetable cells appear to have the same structure at first, during their growth and while their vitality lasts; subsequently most of them undergo great changes, their walls being thicker and their cavities clogged with various secretions. There are some beautiful transparent-beaded hairs at the bottom of the flower cup of the white variety of the Virginian spider-wort. If we place one of these hairs in a drop of water in the field of the microscope, we find that it is simply composed of a row of oval cells attached end to end. The cell is in this case a minute vesicle with an extremely thin transparent wall. This wall consists of cellulose, a substance composed of thirty-six parts of carbon and thirty parts of water. The membrane is perfectly structureless under the highest powers of the microscope, and apparently continuous. It must, however, be minutely perforated, for water and various secretions and excretions pass through it freely. From its composition and structure it is impossible to imagine that vital force should reside in the vegetable cell-wall. We must regard it as an excretion of dead matter moulded as a boundary wall to the cell cavity by some external agent, but incapable of originating any vital action. The cell is full of water or mucous solution, and watching carefully with a proper arrangement of the light, and a moderately high power, we can distinctly trace threads of dense gelatinous matter moving slowly into the inner surface of the cell-wall. These streams commence wider in the region of a nucleus, which was at one time regarded as the heart of the cell, as it were, the centre of its vital activity, and gradually branch and diminish at a distance from it. Under the microscope granules appear in these streams, and with these granules embedded in them, as crumbs are embedded in a stream of treacle, the currents flow round and round the cell, the granules gradually disappearing and being absorbed. The observations of Prof. Max Schultze and of others have, I think, placed it beyond a doubt that this gelatinous substance occurring within the living cell, and forming, at all events, a large proportion of the cell-contents, is identical with the protoplasm which forms the entire substance of such an animal form as *Gromia*.

The necklace-like hair of the spider-wort is, in fact, a chain of cells with dead cellulose walls, and each with a living Gromia body imprisoned within it.

Now, although the power which plants possess of fixing carbon and combining it with the elements of water, is the character which practically distinguishes the Vegetable from the Animal kingdom, I have already shown that we cannot regard this as by any means a universal test. In this respect broomrapes and dodders are animals.

When we pass down by any path we choose, either through animals or plants, we come equally to a great series of very simple forms—mere little masses of protoplasm with a nucleus. Some of these contain peculiarly formed masses of bright colouring matter, green, scarlet, or yellow, and with the possession of such pigment we usually associate the power of decomposing carbonic acid. Many of these bodies have, however, no colouring matter at all, except what is derived from their food. A large number of these simple forms are enclosed in a wall of cellulose, but very many of them are naked or merely covered with a pellicle of firmer protoplasm; while some, such as the plasmodia of the myxogastric fungi are, for some part of their lives, enclosed in a cellulose wall, and for another part, naked. Going still lower, we have Haeckel's Monera, differing from the others merely in the absence of a nucleus and the total want of differentiation of any part. Even these last are sometimes coloured, and from their chemical reactions it seems very likely that they possess some low form of the peculiar vegetable power. Now, the question is, whether all these considerations lead in any way in the direction of establishing a separate kingdom for these simple beings. I think decidedly not, but it seems to me that they prove almost to demonstration that organic nature must be taken as one whole, that the Animal and Vegetable kingdoms are absolutely continuous, and that a tree flinging its green flags into the sunshine and feeding on the winds of heaven, is essentially nothing more than a vast colony of a protozoon, comparable to a gigantic nummulite, only building a cellulose instead of a calcareous shell, and developing a special secretion in special organs for the purpose of enabling it to do so.

MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY

HAVING now for the tenth time the honour of addressing you from this chair on the occasion of your annual gathering, it has been my wish to lay before you a general sketch of the progress making in systematic Biology, the foundation upon which must rest the theoretical and speculative, as well as the practical, branches of the science, to report upon the efforts made further to investigate, establish, and extend that foundation, and to convert the numerous quicksands with which it is beset into solid rock. This subject formed the chief portion of my address of 1862, and again of those of 1866 and 1868; but on the present occasion I have had some difficulties to contend with. Mr. Dallas, to whose kindness I owed the zoological notes I required, has now duties which fully absorb his time, and I have been obliged to apply to foreign correspondents, as well as to my zoological friends at home, for the necessary information. They have one and all responded to my call with a readiness for which I cannot too heartily express my thanks; and if there is some diversity in the extent and nature of the information I have received from different countries, which may prevent any very correct estimate of the comparative progress made in them, it is owing to the questions which I put having been stated too generally, and, though sent in the same words to my different correspondents, they have been differently understood by them. In such a review, however, as I am able to prepare, I propose chiefly to consider the relative progress made by zoologists and botanists in the methods pursued and the results obtained, in the first place as to general works common to all countries, and secondly as to those which are more particularly worked out in or more specially relate to each of the principal states or nations where biological science is pursued, prefacing this review by a few general remarks supplementary to those I laid before you in my first address in 1862.

Since that time systematic biology has to a certain degree been cast into the background by the great impulse given to the more speculative branches of the science by the promulgation of the

Darwinian theories. The great thunderbolt had indeed been launched, but had not yet produced its full effect. We systematists, bred up in the doctrine of the fixed immutability of species within positive limits, who had always thought it one great object to ascertain what those limits were, and by what means species, in their never-ending variations and constant attempts to overstep those limits, were invariably checked and thrown back within their own domain, might at first have been disposed to resist the revolutionary tendency of the new doctrine; but we felt shaken and puzzled. The wide field opened for the exercise of speculative tendencies was soon overrun by numerous aspirants, a cry of contempt was raised against museum zoologists and herbarium botanists, and nothing was allowed to be scientific which was not theoretical or microscopical. But this has been carried, in some instances, too far. If facts without deductions are of little avail, assumptions without facts are worse than useless. Theorists in their disputes must bring forth the evidences they rely upon, and these evidences can only be derived from and tested by sound systematic biology, which must resume and is resuming its proper position in the ranks of science, controlled and guided in its course by the results of those theories, for which it has supplied the basis.* If the absolute immutability of races is no longer to be relied upon, the greater number of them (whether genera, species, or varieties) are at the present or any other geological period, practically circumscribed within more or less definite limits. The ascertaining those limits in every detail of form, structure, habit, and constitution, and the judicious appreciation of the very complicated relations born to each other by the different races so limited, is as necessary as the supplementing the scantiness of data from the depths of Teutonic consciousness by the vivid flashes of Italian imagination, or as the magnifying minute as yet undeveloped organisms, with a precision beyond what is fully justified by our best instruments.

I am, however, far from denying on the one hand how much biological science has of late been raised, since it has been brought to bear through well-developed theories and hypotheses upon the history of our globe, and of the races it has borne; and on the other, how very much the basis upon which it rests has been improved and consolidated by the assiduous use of the microscope and the dissecting knife; but I would insist upon the necessity of equal ability being applied to the intermediate process of method or nomenclature and classification, which forms the connecting link between the labours of the anatomist and the theorist, reducing the observations of the one to forms available for the arguments of the other. All three, the minute observer, the systematist, and the theorist, thus assisting each other, equally contribute to the general advancement of science, and for all practical application, the systematist's share of duty is certainly the most important.

The quicksands to which I have alluded to as besetting thus the foundation of biological science, may be classed as imperfect data and false data, imperfect method and false method. To show what progress is making in removing or consolidating them, it may be useful to consider what these data are, and what are our means of fixing them so as to be readily available for use.

It must, in the first place, be remembered that the races whose relations to each other we study, can only be present to our minds in an abstract form. In treating of a genus, a species, or a variety, it is not enough to have one individual before our eyes, we must combine the properties belonging to the whole race we are considering, abstracted from those peculiar to subordinate races or individuals. We cannot form a correct idea of a species from a single individual, nor of a genus from a single one of its species. We can no more set up a typical species than a typical individual. If we had before us an exact individual representative of the common parent from which all the individuals of a species or all the species of a genus have descended—or if you prefer it an exact copy of the model or type after which the whole species or genus had been created, we should have no possible means of recognising it. I once heard a lecture of a German philosophical naturalist of considerable reputation in his day, in which he thought he proved that the common Clover was the type of Papilionaceæ. His facts were correct enough, but his arguments might have been turned in favour of any other individual species that might have been selected. Suppose two individuals of a species, two species of a genus, two genera of a family, in one of which certain organs are more developed, more differentiated, or more consolidated than in the other, if we agree upon the first question of which is the most perfect, a point upon which naturalists seldom do agree, how are we to determine which represents the common parent or model? whether the perfect one is an improvement upon, or an improved copy, or the imperfect one a degeneracy from or a bad imitation of the other? No direct evidence goes beyond a very few generations, reasoning from analogy is impossible without direct evidence to start from, and the imagining a type without either is the business of the poet, not of the naturalist.

It follows that every such abstract idea of a race must be derived from the observation, by ourselves or by others, of as large a number of the constituent individuals as possible. However fixed a race may be, if fixed at all in Nature, that is not the case with our abstract idea of it, no species or genus we establish can be considered as absolutely fixed, it will ever have to be completed, corrected, or modified, as more and more individuals come to be correctly observed. Hence it is, that a species described from a single specimen, and even a genus established on a single species, always excites more or less of suspicion unless supported by strong reasoning from analogy or confirmed by repeated observation.

Our means of observing and methodising biological facts, of establishing and classifying those abstract ideas we call varieties, species, genera, families &c., consist in the study (1) of living individual organisms; (2) of preserved specimens; (3) of pictorial delineations; and (4) of written descriptions. Each of these sources of information has its special advantages, but each is attended by some special deficiencies to be supplied by one or more of the others.

1. The study of living individuals in their natural state is without doubt the most satisfactory, but very few such individuals can be simultaneously observed for the purpose of comparison, and no one individual, at any one moment, can supply the whole of the data required relating even to that individual. Some additional facilities in these respects are given by the maintenance of collections of living animals and plants, particularly useful in affording the means of continuous observation during the various phases of the life of one and the same individual, and sometimes through successive generations, or in facilitating the internal examination of organisms immediately after death, when the great physiological changes consequent upon death have only commenced. But there are drawbacks and difficulties to be overcome, as well as a few special sources of error to be guarded against, and in this respect, as well as in the progress recently made in their application to science, there is a marked difference between zoological and botanical living collections, or so-called gardens.

The great drawback to living collections, especially zoological, is their necessary incompleteness. At the best it is individuals only, not species, and in a few cases genera that are exposed to observation; genera, indeed, can always be better represented than species, for a few species bear a much larger proportion to the total number contained in a genus, than a few individuals to the total number which a species contains. Whole classes are entirely wanting in zoological gardens, which are usually limited to vertebrata. Of late years means have been found to include a few aquatic animals of the lower orders, but insects, for instance, those animals which exercise the greatest influence on the general economy of nature, the observation of whose life and transformations is every day acquiring greater importance, are wholly unrepresented in zoological gardens. The shortness of duration of their individual lives, their enormous power of propagation, the different mediums in which they pass the different stages of their existence, will long be obstacles to the formation of living entomological collections on anything like a satisfactory scale. The cost also of the formation and maintenance of living collections is very much greater in the case of animals than of plants; but on the other hand zoologists have the advantage of the attractiveness of their menageries to the general unscientific but paying public, and, under judicious management, some sacrifices to popular tastes are far outweighed by the additional funds obtained towards rendering their collections useful to science.

The false data or errors to be guarded against in the observation of living zoological collections are chiefly owing to the unnatural conditions in which the animals are placed. Ungenial climate, unaccustomed food, want of exercise, &c., act upon their temper, habits, and constitution, and confinement materially

* The great importance of morphology and classification, the elements of systematic biology, has been forcibly illustrated by Prof. Flower in his last year's introductory lecture at the Royal College of Surgeons.

modifies circumstances connected with their propagation. Such errors or false data are, no doubt, as yet very few and unimportant compared to those which have arisen from the reliance on garden plants for botanical observations, but, as zoological gardens multiply and extend, they will have to be more and more kept in view.

In my younger days there were already a number of small collections of living animals, but almost all either travelling or local menageries exhibited for money by private individuals, or small collections kept up as a matter of curiosity for the benefit of the public, such as those of the Pfauen-Insel at Potsdam, the park at Portici, or our own Tower menagerie. At Paris alone, at the Jardin des Plantes, in the flourishing days of the Jussieus and Cuviers, was the living zoological collection rendered essentially subservient to the purposes of science. Since then, howeven, matters have much changed: the Jardin des Plantes, which so long reigned supreme, has, by remaining stationary, sunk into a second rank. She may indeed be as justly as ever proud of her Milne-Edwards, her Brongniart, her Decaisne, and many others, but long out of favour with the Government and the paying public, who transferred their patronage to the high-sounding Jardin d'Acclimatation, now no more, she has been almost abandoned to the resources of pure science, always of the most restricted in a pecuniary point of view. We in the mean time, and after our example several continental states or cities, have made great advances. The formation of our Zoological Society and Gardens opened a new era in the cultivation of the science. After various vicissitudes, the Society had the good fortune to secure the services of one who combined in the highest degree zoological eminence with administrative ability, and thus our great living zoological collection is now raised to the proud relative position which the Jardin des Plantes once held, and which there seems every reason to hope it will long maintain. With an annual income of about 23,000*l.* the Zoological Society is enabled to maintain a living collection of about a thousand species of Vertebrata, and although some portion of the surplus funds is necessarily applied for the sole gratification of the paying public, yet a fair share is devoted to the real promotion of that science for which all the fellows are supposed to subscribe, the accurate observation of the animals maintained, the dissection of those that die, and the publication of the results. Physiological experiments are either actually made in the garden, or promoted and liberally assisted, such, for instance, as those on the transfusion of blood, the effects or non-effects of which were recently laid before the Royal Society by Mr. F. Galton. A very rich zoological library has been formed, and last year's accounts show a sum of about 1,800*l.* expended in the Society's scientific publications.

Zoological gardens, after the example of the London one, have been established not only in several of our provincial towns, but in various continental cities, amongst which the more important ones, as I am informed, are those of Amsterdam, Antwerp, Hamburgh, Cologne, Frankfort, Berlin, Rotterdam, and Dresden; the receipts of the one at Hamburgh, for instance, amounting annually, according to the published reports, to between 8,000*l.* and 9,000*l.* There are also so-called gardens of acclimatisation; but these have not much of a scientific character; their professed object indeed is not so much the observation of the physiology and constitution of animals as their modification for practical purposes, and practically they are chiefly known as places of recreation, and are not always very successful. The great one in the Bois de Boulogne, now destroyed, out of an expenditure in 1868 of about 7,200*l.* showed a deficit of about 1,600*l.*; a smaller one at the Hague is enabled to pay an annual dividend to its shareholders.

Living collections of plants have great advantages over those of animals, they can be so much more extensively maintained at a comparatively small cost. In several botanical gardens several thousand species have been readily cultivated at a comparatively small cost, and species can be represented by a considerable number of individuals, a great gain especially where instruction is the immediate object, the lives of many can be watched through several successive generations, and great facilities are afforded for physiological experiments and microscopical observations on plants and their organs whilst still retaining more or less of life. On the other hand the false data recorded from observations made in botanical gardens have been lamentably numerous and important. A plant in the course of its life so alters its outer aspect that each one cannot be individualised by the keeper of a large collection, and at one period, that of the seed in the ground, it is wholly withdrawn from his observation. He is therefore obliged to trust to labels, these are often mismatched by accident or by the carelessness of the workmen employed, or again, one seed has been sown and another has come up in its place, or a perennial has perished and made room for a sucker or seedling from an adjoining species. The misnomers arising from these and other causes have become perpetuated and sanctioned by directors who, for want of adequate libraries or herbaria, or sometimes for want of experience or ability, have been unable to detect them. Plants have also been so disguised or essentially altered by cultivation that it has become difficult to recognise their identity, and new varieties or hybrids, which, if left to themselves, would have succumbed to some of the innumerable causes of destruction they are constantly exposed to in a wild state, have been preserved and propagated through the protective care of the cultivator, and pronounced at once to be new species. If, moreover, a misplaced label indicates that the seed has been received from a country where no plants of a similar type are known to grow, the director readily notes it as a new genus, and, proud of the discovery, gives it a name and appends a so-called diagnosis to his next seed-catalogue, adding one more to the numerous puzzles with which the science is encumbered. So far, indeed, had this nuisance been carried in several Continental gardens in the earlier portion of the present century, that, excepting perhaps Fischer and Meyer's and a few other first-rate indexes, the great majority, perhaps nine-tenths, of the new species published in these catalogues have proved untenable, and, from my own experience, I am now obliged, *a priori*, to set down as doubtful every species established on a garden plant without confirmation from wild specimens. Fortunately the custom is now abating, and directors of botanic gardens are beginning to perceive that they do not add to their reputation by having their names appended to those of bad species.

Living collections of plants, or botanical gardens, are of much older date than zoological ones, and since the sixteenth century have been attached to the principal universities which have medical schools, that of Padua, dating from 1525, that of Pisa, from 1544, and of Montpellier, from 1597. The Jardin des Plantes of Paris, which in botany, even more than in zoology, so long reigned supreme, was established in 1610, our own first one at Oxford in 1632. These university gardens having been generally more or less under the control of eminent resident botanists, have contributed very largely to the means of studying the structure and affinities of plants, especially in those Continental cities where a milder or more steady climate has facilitated the maintenance of large collections in the open air or with little protection. Continental gardens have also been long and are still made largely available for the purpose of instruction as well as of scientific experiments, of which the recent labours of Naudin and Decaisne are an excellent illustration. For these scientific purposes the arrangement in large and small square compartments is peculiarly suitable, and I confess that I have frequently had greater pleasure in witnessing the facilities afforded to zealous students in following up, book in hand, the straight rows of scientifically-arranged plants in these formal university gardens, than in watching the gay crowds that flock to the more ornamentally laid out public botanic gardens.

I do not think that generally much advance has been made of late years in Continental botanical gardens. Those that I first visited in 1830 appeared to me to be but little improved when I again went over them in 1869. Some have acquired additional space, others have paid more attention to ornament, but most have remained nearly stationary, and a few have even fallen back. In our own country we have mnde great progress. Kew Gardens had indeed, in former days, rendered assistance to the investigations of Robert Brown and a few other favoured individuals, but they were the Sovereign's private property, and were kept very close, with little encouragement to science at large. But thirty years' unceasing exertions on the part of its distinguished directors, the two Hookers, father and son, have raised them to a point of scientific usefulness far beyond any other establishment of the kind at home or abroad. Of the large sums annually voted for it by Parliament, a portion has indeed to be applied to mere ornament and to the gratification of visitors, but yet, with all the drawbacks of our climate and consequent expenditure in houses, the largest named collection of species ever brought together in one spot, representatives of all parts of the globe, are there maintained, freely exhibited to the public, and submitted to the examination of scientific botanists.

(*To be continued*)

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 25.—"Some Remarks on the Mechanism of Respiration." By F. Le Gros Clark.

The author commences his paper by narrating some experiments on recently slaughtered animals, in the course of which the remarkable tension of the diaphragm was noticed; and the varying condition of that muscle, and of the lungs and pleura, with their mutual relations, are commented on.

The importance of this passive tension of the diaphragm is indicated and exemplified both physiologically and pathologically. It is essential in retaining the supplemental air within the lungs, in restoring the equilibrium of repose, in economising active muscular power, and in maintaining the pericardial space, &c.

The action of the diaphragm in relation to the walls of the chest and to other muscles is next discussed; and the influence of the diaphragm in drawing in the chest-walls, under certain circumstances, is pointed out, and illustrated by cases of injury to the spinal cord.

The action of the intercostal muscles, as necessary adjuncts to the diaphragm and as muscles of inspiration, is insisted on and illustrated by diagrams; and a summary of their action is given.

The agency of the serratus magnus is then discussed; and reasons are advanced, supported by observation and experiment, to show that it is only under special conditions and to a limited extent that it can be regarded as taking any part in the act of inspiration.

The mobility of the different costal regions and of the sternum is exemplified by observation and experiment.

Lastly, the question of abdominal and thoracic breathing, severally in the male and female, is considered; and reasons are adduced for concluding that the received opinions on this subject are erroneous.

"Spectrum of Comet I." By Dr. William Huggins, F.R.S.—On April 7 a faint comet was discovered by Dr. Winnecke. I observed the comet on April 1 and May 2. On both days the comet was exceedingly faint, and on May 2 it was rendered more difficult to observe by the light of the moon and a faint haze in the atmosphere. It presented the appearance of a small faint coma, with an extension in the direction of the sun. When observed in the spectroscope, I could detect the light of the coma to consist almost entirely of three bright bands. A fair measure was obtained of the centre of the middle band, which was the brightest; it gives for this band a wave-length of about 510 millionths of a millimetre. I was not able to do more than estimate roughly the position of the less refrangible band. The result gives 545 millionths. The third band was situated at about the same distance from the middle band on the more refrangiele side. It would appear that this comet is similar in constitution to the comets which I examined in 1868.*

"Researches on the Hydrocarbons of the Series $C_nH_{2n} + _2$."—By C. Schorlemmer.

In a former communication I have shown that the paraffins, the constitution of which is known, may be arranged in four groups. The first group, which I called *normal paraffins*, contain the carbon atoms linked together in a single chain. Of these I have obtained some new ones, which I shall describe more fully in a further communication. The normal paraffins which I have so far studied are given, together with their boiling points, in the following table:—

	From petroleum.	From the acids of the series $C_nH_{2n2} - {}_2O_4$	So-called alcohol radicals.	
C_5H_{12}	37° – 39°	——	——	
			Dipropyl.	From Mannite.
C_6H_{14}	69° – 70°	69°·5	69° – 70°	71·5°
C_7H_{16}	98° – 99°	100°·5	——	——
			Dibutyl.	From methylhexylcarbinol.
C_8H_{16}	123° – 124°	123° – 124°	123° – 124°	124°

That these paraffins have really the constitution which I have ascribed to them follows partly from their mode of formation; thus dipropyl was obtained from the normal propyl iodide, and dibutyl from normal butyl iodide. The constitution of the others was determined by converting them into alcohols and studying the oxidation products of the latter; thus the hexyl hydride from petroleum, as well as that obtained from mannite, was transformed into secondary hexyl alcohol, which on oxidation yielded acetic acid and *normal* butyric acid.

* Phil. Trans. 1868, p. 555; and Proc. Roy. Soc. vol. xvi. p. 386.

In the communication above referred to, I placed the hydrocarbon C_8H_{18} from methyl-hexyl carbinol amongst another group; but I have found now that this body is identical with dibutyl and also with the hydrocarbon, which Zinke obtained from primary octyl alcohol. This chemist prepared also dioctyl, $C_{16}H_{34}$, which consequently is a normal paraffin; and it appears probable that dibexyl, which Brazier and Goslett obtained by the electrolysis of œnanthylic acid, belongs to this group too.

We are now acquainted with the following normal paraffins:—

	Boiling-points. Found (mean).	Calculated.	Difference.
$C\ H_4$	——	——	
$C_2\ H_6$	——	——	
$C_3\ H_8$	——	——	
$C_4\ H_{10}$	1°	1°	
$C_5\ H_{12}$	38°	38°	37°
$C_6\ H_{14}$	70°	71°	33°
$C_7\ H_{16}$	99°	100°	29°
$C_8\ H_{18}$	124°	125°	25°
$C_{12}H_{26}$	202°	207°	4 × 19°
$C_{16}H_{34}$	278°	278°	4 × 19°

From this it appears that the boiling-point is not raised 31° for each addition of CH_2, as I formerly assumed, but that, as the calculated numbers show, the difference between the boiling-points of the lower members decreases regularly by four until it becomes the well-known difference of 19°.

Chemical Society, May 18.—Prof. Frankland, F.R.S., president, in the chair. Messrs. T. Greenish and J. E. Mayall were elected Fellows. The following papers were read:—"On a new double salt of thallium," by R. T. Friswell. The author wishing to prepare thallic platino-cyanide, mixed hot solutions of thallic carbonate and potassic platino-cyanide, and obtained on leaving the mixture to cool masses of splendid crystals, which appeared by transmitted light of a magnificent crimson red, whilst their reflected colour was a bronzy green of strong metallic lustre. Analysis showed that they are a compound of thallic carbonate with thallic platino-cyanide, Fl_2 Pt Cy_4, CO Fl O_2. On treating this salt with acids carbonic acid is set free, and a pale pink residue left, which on examination was found to be thallic platino-cyanide.—The next paper read was "On the action of nitric acid on dichloro-phenolsulphuric acid," by Dr. Armstrong.

Geologists' Association.—The excursion of this Society to Oxford took place on the 12th and 13th inst. On the first day the numbers assembled at the beautiful new University museum at noon, and were introduced to Professor Phillips, who commenced a descriptive lecture on the museum, its arrangement and contents. There is a peculiar double arrangement of the palæontological collection by which the student may with equal ease make himself acquainted with the organisms derived from any one geological formation or devote himself to the study of the fossil remains of a single class or order of the animal or vegetable kingdoms. The museum is not crowded, but contains good specimens of those species which are most typical or characteristic. These, too, with the fossil remains of saurians and mammals are the complete skeletons of analogous living genera, an arrangement most advantageous to the student. The speciality of the Oxford museum is the unique collection of the remains of *Cetiosaurus*. A most interesting description of the enormous bones of this genus was given by Prof. Phillips, who, by means of corresponding crocodilian bones, gave a clear idea of the vast size to which these huge creatures attained. The estimate made by the Professor was that the Cetiosaurus was 40 feet long and 12 feet in height, the femur being fully 60 inches long, while the femur of a crocodile, with which it was contrasted, being no more than nine inches in length. In the afternoon the party proceeded to Shotover Hill, examining by the way the excavations in the Oxford clay near the city, and the exposures of the coral rag and the Kimmeridge clay on the side of the hill. Near the top of Shotover, Portland sands and a thin band of Portland rock are seen, and above these beds and forming the summit of the hill are the "Iron Sands," which have been the subject of much dispute. These highly ferruginous beds were considered to be lower greensand, but the finding of a considerable number of fresh-water species of Mollusca has induced Prof. Phillips to conclude the "Iron Sands" to be of Wealden age. From these sands at the summit of the hill ochre is obtained in large quantities. In the evening a soirée was given by Mr. James Parker, of Oxford, who most hospitably entertained the members of the Association. The magnificent collection of reptilian remains and other fossils from the neighbourhood of Oxford was shown to the visitors, and described by Prof. Phillips and Prof. Morris.—Saturday's proceedings were commenced by an early visit to Merton College, for the purpose of inspecting the very fine collection of fossils which Mr. Earwaker of that college has brought together. Afterwards the party, with Profs. Phillips and Morris, started by carriage for Islip, Enslow Bridge, and Kidlington. At Islip a very fine section of the Forest Marble and Cornbrash is exposed, and the usual fossils of these formations are here found. The village of Islip is, however, interesting to geologists on other grounds, for here lies Buckland. Around the tomb of the great geologist with his distinguished successor at their head the party assembled. The tomb is of polished Aberdeen granite, and the inscription briefly records the fact that there lie the remains of Dr. Buckland, Rector of Islip, Dean of Westminster, and First Reader in Geology in the University of Oxford. The quarries at Enslow Bridge, which have yielded a large number of the Saurian bones in the University Museum, was then visited, and here the visitors were highly gratified to find that during the morning a very fine *Teleosaurus* had been found, and the head, taken out of the bed in which it had lain for untold ages, was exposed to view. This quarry is in the great Oolite, the lower and uppermost strata of which in Oxfordshire yield remains of *Megalosaurus*, while in the middle beds we find *Teleosaurus*. A very remarkable bed of about twelve inches thick occurs a little above the Teleosaurian cave, crowded with *Terebratula maxillata* to the exclusion of every other species. Several other sections of the Great Oolite, Forest Marble, and Cornbrash were examined, and the weather being very fine the drive through the beautiful country was much enjoyed, and the return to Oxford effected in time to allow of the party taking their departure for London by the evening train.

DUBLIN

Royal Dublin Society, May 13.—Dr. J. Emerson Reynolds, analyst to the Society, delivered the concluding lecture for the session 1870-71. The subject of the lecture was the "Chemistry of Milk." The lecturer, referring to cow's milk more particularly, described the constituents of the fluid at considerable length, and showed the precise quantities of butter, casein, sugar, and salts obtained from a known amount of milk of good quality. A number of new facts bearing on the chemical constitution of the different substances present in milk were then stated, and the relations of casein and sugar to the several parts of the animal orgnnism were pointed out. It was proved by a large number of analyses of milk taken from cows fed in various ways in different parts of the country, that milk is naturally subject to very wide variations in the proportions of its constituents, and hence that it is extremely difficult, if not impossible, to state with precision that a given sample of milk had been adulterated with a certain amount of water. Under these circumstances the lecturer suggested that milk should in future be judged according as it might reach or fall below a certain standard quality, fair alike to the vendor and the purchaser, but that milk falling below the standard should not necessarily be stigmatised as adulterated, but simply have a lower commercial value attached to it. Dr. Reynolds stated, as the result of his experience, that milk sold at the present price per quart may fairly be expected to have the following composition in one hundred parts:—

Water	87·0
Butter	3·5
Casein	4·0
Sugar	5·0
Salts	·5

The proportion of fatty matter can be easily ascertained by the rapid methods of Sir Joseph Banks or Dr. Min chin, and the sugar determined in a few minutes by the aid of the polariscope; but it has been hitherto impossible to speedily measure the proportion of the valuable casein of milk without recourse to elaborate chemical analysis. By means of a very simple contrivance, which was exhibited at the le cture, the proportion of casein can, however, now be speedi ly ascertained; we are, therefore, for the first time in a po sition to form quickly a sufficiently precise estimate of the nutritive value of a given sample of milk. The lecturer concluded by expressing a hope that the public would now use the means placed in its hands for guarding against imposition on one side, or the hasty condemnation of the honest trader on the other.

May 22.—Prof. Dyer in the chair. Prof. R. Ball read notes on Kater's Pendulum and on a new Hydraulic Press.—Dr. J. Emerson Reynolds read notes of Experiments on the flow of Liquids through Capillary Tubes.—Mr. W. F. Kirby communicated a list of the species of *Papilionidæ* or Swallow-tailed Butterflies in the collection of the Society, and exhibited specimens in illustration. Among these were a long series of *P. cymochles* Gray and *P. idalion* Felder, which Mr. Kirby believed to be sexes of one species. He also called attention to a remarkable variety (?) of *P. polymnestor* Cram., in which the blue colouring of the hind wings was reduced to a band.—Prof. Dyer read a paper on Bud scales, in the course of which he objected to the word mimicry being used for the resemblance borne by a plant belonging to one natural family to a plant belonging to a different natural family, except in such cases as where the plants were found living side by side.—Mr. A. G. More exhibited for Dr. Cartt a number of additions to the museum, and told some of the more remarkable stories known about each species. Among the more interesting specimens exhibited, one of the American Goshawk shot in Tipperary and one of *Cygnus bewickii* may be mentioned.

DIARY

THURSDAY, June 1.

Linnean Society, at 8.—On Some Plants from Northern China: Dr. Hance.—On South American *Hippocrateaceæ*: Mr. Miers.
Chemical Society, at 8.—On Ozone: Dr. Debus, F.R.S.
Royal Institution, at 3.—On Sound: Prof. Tyndall, F.R.S.

FRIDAY, June 2.

Geologists' Association, at 8.—On Flint: M. Hawkins Johnson, F.G.S.
Royal Institution, at 9.—Gaseous and Liquid States of Matter: Prof. Andrews.

SATURDAY, June 3.

Royal School of Mines, at 8.—Geology: Dr. Cobbold.
Royal Institution, at 3.—On the Instruments Used in Modern Astronomy: J. N. Lockyer, F.R.S.

MONDAY, June 5.

Entomological Society, at 7.
Royal Institution, at 2.—General Monthly Meeting.

TUESDAY, June 6.

Zoological Society, at 9.—On Dinornis (Part XVII). Containing a description of the sternum and pelvis, with an attempted restoration of *Aptornis defossor*, Ow.: Prof. Owen, F.R.S.—On a Seal new to the British Fauna: Prof. Flower.—On Risso's Dolphin: Prof. Flower.
Society of Biblical Archæology.—On the Early History of Assyria and of Babylonia, from Contemporary Inscriptions (part 1): G. Smith.—On the Date of the Nativity: J. W. Bosanquet.
Royal Institution, at 3.—Least Action in Nature: Rev. Prof. Haughton.

WEDNESDAY, June 7.

Geological Society, at 8.—Notes on the Geology of part of the County of Donegal: A. H. Green, M.A., F.G.S.—On the Persistence in the Deep-seas of the present day of *Caryophyllia cylindracea*, Reuss, a cretaceous coral: Prof. P. Martin Duncan, F.R.S.—Note on an *Ichthyosaurus* (*I. enthikiodon*), from Kinmeridge Bay, Dorset: J. W. Hulke, F.R.S.—Note on a Fragment of a Teleosaurian Snout, from Kimmeridge Bay, Dorset: J. W. Hulke, Esq.
Royal Microscopical Society, at 8.
London Institution, at 2.—Distribution of Prizes and Certificates by Mr. T. Baring, M.P., President.

THURSDAY, June 8.

Society of Antiquaries, at 8.30.
Mathematical Society, at 8.
Royal Institution, at 3.—Sound: Prof. Tyndall.

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