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THURSDAY, JUNE 6, 1872

BAD GREEK OR GOOD GERMAN?

AN event occurred on Thursday last at Cambridge, not in itself, perhaps, of imposing magnitude, but yet fraught with very important consequences. For this long while back an agitation has been going on with the purpose of making Greek no longer absolutely essential to the Previous Examination (or "Little Go," as it is popularly called), but of allowing French or German, or both, to be substituted for it at the option of the candidate. As any long-headed man might have foreseen, the genuine scholarship and liberal intelligence of the University are in favour of such a change; but the opposition has been neither feeble nor silent. Discussion has abounded more and more, and "fly-sheets" have fallen like the latter rain. The advocates of the change seem to have been more or less governed by a dislike to many words, and to have had large faith in the merits of their cause; their opponents, on the other hand, appear to have believed in the efficacy of much speaking, and in the effects of arguments drawn from all quarters, and looking all ways; their papers and speeches, all put together, form as pretty a piece of incoherence as may be found in a literary day's march, and would have been a perfect godsend to the great Skepsius when he wrote his famous tract *An hominibus mens absit.* The reasons indeed for making the change were so clear and cogent that there seemed hardly any hope of its being accomplished. Yet by one of those freaks of fortune which are met with even in the Universities, wisdom prevailed; and by the vote of the Senate on Thursday last, which will, in all probability, be speedily ratified at a second meeting, the student who desires to go out in an "honours" examination henceforth need not at his Little Go scratch up a smattering of bad Greek, if he satisfies his examiner that he possesses a real knowledge of French or German.

We trust that the scientific workers at Cambridge will take heart at this happy issue of the struggle, and gird up their loins for the heavy task of introducing order and system into the chaos in which the natural science studies at Cambridge are now lost. Let them set to work at once, and no longer wait for that *Deus ex machina* of the Royal Commission, who at present sit aloft, like the gods in Tennyson's "Lotos Eaters," and of whom it might be said, "Though their wheels are grinding finely, yet they grind exceeding slow."

The graduates of the University of London too might do well to ponder over this result. It is one of the marks of good tone at Cambridge to be very imperfectly acquainted with the Metropolitan University, except so far as its scholarships and examinerships are concerned; and accordingly it was stated more than once in the course of the discussion, and used as an argument against the proposed change, that the University of London had recently refused to make Greek optional at its Matriculation Examination. Our better informed readers are probably aware that the Senate, the real governing body of the University, have the matter at this very moment under their consideration, and, without wishing to forestall the future, we may presume to say that beyond doubt a change will soon be made. It is perfectly true that Convocation, in spite of the Report of its Annual Committee, hesitated to recommend the change; and this seems to have led to the mistake of the Cambridge advocates of bad Greek; but it is well known that, as indeed a sound knowledge of human nature would lead one to expect would be the case, there is among the body of graduates of the radical University, a mass of partly rabid and partly stupid conservatism, which, if it had its own way, would soon bring the University to ruin. Happily the executive Senate, being for the most part selected by the Crown, is wise and liberal, and is especially animated by the feeling that the University, if it is to fulfil its function, must grow with the growth of time, and change with changing things.

It is not a little to the credit of the older University of Cambridge that she should have been actually the first to remove one more of the old-fashioned swaddling clothes, which have been checking the development of youthful science, and we trust it is an earnest of still greater changes which she means to take in hand. Science has been too long at that old University a sort of blind Samson, bound with many cords, and serving chiefly to make sport for mocking Philistines of the classical and mathematical tribes. It is time his cords were loosed, and his strength made use of for the general advancement of the University.

OUR NATIONAL INDUSTRIES

IT is believed by many scientific men that research is all but dead in England. Whether we confess it or not, England, so far as the advancement of knowledge goes, is but a third or fourth-rate power. It is not our present purpose to inquire into the causes of all this; whether, as some say, it is because our professors are so rich, or whether, as others affirm, because all arrangements for the increase of knowledge are so poor, but rather to call attention to the certain influence of this on the wealth—let us put it in the most sordid manner—of the nation in the future.

In this inquiry we find to our hand, in a recent number of the *Birmingham Morning News*, an article on the future extension of Birmingham industries, by Mr. George Gore, whose important researches are well known. We know no one better qualified than Mr. Gore to discuss the subject, and no town where it is more important that the subject should be ventilated, for Birmingham has received much from and has given nothing to original scientific research; but the conclusions to be drawn from the article are in no way limited to Birmingham.

In this article Mr. Gore first considers by what general means the chief trades of Birmingham were first originated and improved; and then discusses whether we can by similar means, applied in a more effectual manner, lay the foundation of other new trades and improvements. Mr. Gore writes:—

"Let us consider German-silver and its manufacture. That substance is an alloy of copper, zinc, and nickel; it owes its peculiar whiteness or 'silver-like' appearance to the latter metal, and cannot be made without it; it is certain, therefore, that by whatever means that metal or the alloy was discovered, the discovery was the origin of the German-silver manufacture, and was essential to all manufactures, processes, or appliances, in which German-silver, nickel, or any of its compounds are used. Nickel was discovered by Cronstedt during the year 1751, and its compounds were chiefly investigated by English and foreign chemists. Cronstedt found it as a peculiar metal in the mineral called kupfernickel, whilst chemically examining the properties of that substance. The general method by which he discovered it was careful experiment, observation, and study of the properties of matter. I believe it is a fact that the Chinese and other nations made alloys of nickel long before nickel itself was known to be a separate metal; they had found, by experiment, that when ores of copper and zinc were mixed with a particular kind of mineral and smelted, a white alloy was obtained; but this also proves the general statement already made, that the German-silver manufacture was originated by means of experiment and observation. It was by a more skilful, but similar mode of procedure, that Cronstedt discovered the metal itself, and thus laid the basis of improvements in the manufacture of its alloys. I need not here enlarge upon the multitude of uses to which nickel has already been applied in Birmingham manufactures, nor speak of the large sums of money which have been and still are made by means of it and its compounds. . . .

"The manufactures of iron-wire and copper-wire for telegraphs are two other modern trades of great magnitude in this town, and were originated in the following manner:—In 1799, Volta, an Italian philosopher, was experimenting, observing, and studying the electric properties of metals in liquids, and discovered the Voltaic battery. In 1815, Prof. Oersted, of Copenhagen, was experimenting on the relation of electric currents to magnets, and observed that when a magnet was suspended near and parallel to a horizontal copper-wire, through which an electric current was passing, the magnet moved spontaneously, and placed itself at right angles to the wire. From these two small experiments, made by putting matter and its forces under new conditions, observing and studying the results, all our telegraphs and the immense manufactures of iron and copper telegraph wire have arisen.

"There is a saying, that 'all great things have had small beginnings,' and this is true, not only of electric telegraphs, but also of the great trade of electro-plating, and of the magneto-electric machine, which is now largely used instead of the Voltaic battery. After Volta had made his small and apparently unimportant experiments on the electricity produced by metals and liquids, various persons tried the effect of that electricity upon metallic solutions. Brugnatelli, in 1805, found that two silver medals became gilded in a solution of gold by passing the electricity through them. Mr. Henry Bessemer, in 1834, coated various lead ornaments with copper by using a solution of copper in a similar manner. And in 1836 Mr. De la Rue found that copies might be taken in copper of engraved copper plates by the electro-depositing process. Faraday discovered magneto-electricity in the year 1831, by rotating a disc of copper between the poles of a magnet, and he has stated that the first successful result he obtained was so small that he could hardly detect it. This simple experiment was the origin of the magneto-electric machine, and many of those machines are now used by Messrs. Elkington for depositing copper, silver, and gold, instead of the Voltaic battery.

"Another large manufacture of this district is that of phosphorus. The origin of it is due to the man, whoever he was, who first isolated that element. Histories of chemistry tell us that it was discovered by Brandt, a merchant of Hamburgh, in 1669; but evidence exists that it had been obtained in the separate state very many years before by the early Arabian chemists. Brandt obtained it by distilling a mixture of dried residue of urine and charcoal. His discovery was also made by careful experiments, and observation of the properties of matter, and had it not been made there would have been no manufactures of phosphorus or phosphorus matches in this district.

"Priestley made many experiments on the absorption of gases by water, and proposed such liquids as beverages, and those apparently trifling experiments have since expanded into the large manufactures of aërated waters."

After having given these instances out of many, the manner in which these practical results have been obtained is stated:—

"Persons inexperienced in scientific matters are apt to think that discoveries are generally made by accident. The reverse is, however, the case; nearly all our great modern discoveries were effected by men who were constantly making careful experiments upon the properties of matter and its forces, by subjecting them to new and definite conditions. Nearly all persons look upon such discoveries as fortunate ideas, which, when once found, are quickly developed, instead of which they are in most cases, slowly developed results of most difficult mental labour. Discoveries in science are occasionally made, not by original scientific investigators, but by practical men engaged in manufacturing or technical employments. The hydro-electric machine originated in this way: a man at Newcastle was attending to a steam-boiler, and found that he received electric shocks when he touched the boiler. This circumstance was investigated by his employer, Mr., now Sir William, Armstrong, and led him to construct the hydro-electric machine. The accumulation of electricity in submarine telegraph cables was also first observed at the Gutta-Percha Company's works, London. It was noticed on testing the cable by means of a voltaic battery (the cable being submerged in water) that discharges of electricity flowed from the cable after the battery was removed; this circumstance was investigated by Faraday, and led to improvements in submarine telegraphy. In these instances also the same general method was employed, viz., new experiments were made (though not intentionally) by putting matter and its forces under new conditions, and new results were observed. . .

"Scientific discovery, therefore, by developing new facts and laws relating to matter and its forces, constitutes not only the basis of new manufactures, but largely, also, of the improvements in trades made by inventors and practical men; and if discoveries are not made, the means by which improvements are effected by such men will become exhausted. The great value of new scientific knowledge to such men is proved by the fact that when new scientific discoveries are published there are numerous inventors and practical men who immediately endeavour to apply them to useful purposes. Since the first application of coal-tar to the production of dyes, every discovery in that branch of chemistry has been closely watched for a similar purpose.

"According to all our experience, scientific discovery provides the knowledge necessary for making inventions, and practical inventions lead to increase of trade. It might easily be shown that in this way scientific research has already resulted in the employment of whole armies of workmen, and in the expenditure and investment of a fabulous amount of money in railways, telegraphs, machinery, gasworks, chemical works, electro-plating, photography, &c., &c., in this country; and Birmingham has received a large share of the benefit."

We now come to the point that we are anxious to enforce:—

"The future success of this town and district is dependent upon original scientific research to a degree of which persons in general can form but little conception. Hundreds of millions of pounds are being expended in covering the earth with telegraphs, and thousands of millions in covering it with railways, gasworks, waterworks, &c., and Birmingham and its district has its share in supplying the rails, the wire, and the machinery. In this country alone more than 550,000,000 of pounds have been already expended upon railways only. *Original scientific research is the great fountain-head of industry*, and its capability of developing increased trade is practically unlimited: it is at present quite in its infancy, and we are only on the very threshold of a knowledge of the forces of nature, and of the constitution of material substances; and if such enormous results are being produced by the beginnings of unaided science, what may be expected from its future developments, especially if scientific research is assisted in an effectual manner?

"Numerous important subjects of investigation, capable of yielding valuable results bearing upon the trades of this town, exist in all directions. Researches in electricity and in inorganic chemistry, particularly the metals and their compounds, would probably lead, as they have done before, to the establishment of new trades, and to improvements in local manufactures, and thus lay the foundation of future commercial prosperity. Discoveries in science, however, are best made, not by trying to obtain some valuable commercial or technical result (that object belongs to an inventor), but by making new, reliable, and systematic investigations. By investigating the chemical action of electricity upon saline bodies, Sir Humphry Davy isolated sodium and magesium, which has led to the recent establishment in Manchester of the manufactures of those metals. By the abstract researches of Hofmann and others upon coal-tar, the immensely profitable manufacture of the splendid coal-tar dyes was originated.

"Scientific discovery is the most valuable in its ultimate practical results when it is pursued from a love of truth as the ruling motive, and any attempt to make it more directly and quickly remunerative, by trying to direct it into practical channels, will decrease the importance of its results, diminish the spirit of inquiry, and sooner or later reduce it to the character of invention. The greatest practical realities of this age had their origin, not in invention or a search for utilities, but in a search after important new truths, entirely irrespective of what utilities they might lead to.

"I do not intend by these remarks to imply that any new trades or improvements in manufactures have been or can be effected without the labours of inventors and practical men; but that there should be a more judicious division of labour, one man to discover new truths, another to put them into the form of practical inventions, and the practical business man to work them; because it is proved by experience that in nearly all cases these different kinds of labour require men of widely different habits of mind, and that the faculties of discovery, invention, and practical manufacture, are very rarely united in one man.

"Our large manufacturers and men of business have accepted and employed the advantages of science in an endless number of ways in their occupations, and have thereby acquired great wealth; but, notwithstanding this, and that the greatest trades of this district were originated and improved largely by means of scientific investigation, scarcely any of the wealthy manufacturers or landholders of the locality, who have derived such great benefits from the increase of trades, give the least assistance to scientific research; that which is the duty of all has been attended to by none. The probable explanation is, original scientific research is a subject quite outside the experience and knowledge of persons in general. It may be objected that such research is not aided, because it sometimes takes a long time to acquire a practical shape and make it pay. We do not omit to plant an acorn because it requires many years to become an oak; we do not neglect to rear a child because he may not live to become a man; but we leave scientific discovery to take care of itself."

England's present and special weakness is then referred to:—

"Our practice with regard to science is very different from the plan carried out in Germany. Within the last few years great laboratories have been erected in Berlin, Leipzig, Aix la Chapelle, Bonn, Carlsruhe, Stuttgardt, Griefswald, and other places, at the expense of the State, and special provision has been made in them for original scientific research. A glance at the frequently published list of scientific investigations made in different countries will show us that the Germans are making a far greater number of discoveries in science than ourselves. If we are to maintain our position as a manufacturing nation, we also must adopt special means to promote scientific research; for how can we expect to obtain new arts and manufactures, or improvements in old ones, if we do not make new discoveries in the properties of matter and its forces? I need not multiply instances of the essential dependence of our present commercial success upon abstract scientific research, but may safely affirm that nearly all our great manufactures have been originated by means of experiment, observation, and study of matter and its forces; and that the great bulk of the improvements made in manufactures by practical men could not have been effected had not scientific investigators discovered, and made known in books, the properties of bodies. The inference from these conclusions is obvious: by adopting similar means, but in a more effectual way, we shall obtain similar but more successful results."

And this being so, what is the actual condition of things? According to Mr. Gore, "at present, original scientific researches are generally made by teachers of science, who spend a portion of their scanty incomes in making experiments, and lead lives of great self-denial in the labour. There is absolutely no provision in this country for the support of scientific investigators, and thus the great source of new trades and improvements in manufactures remains undeveloped."

Surely if scientific men are convinced, first, that the future of our national industries depends upon research, and secondly, that there is no research, the time has arrived when action of some sort is incumbent upon them if they are ever to take action in any subject whatever; for it is perfectly obvious that any bettering of such a state of things can only proceed from the action of the scientific men themselves.

We have let Mr. Gore speak for himself thus at length, as in his article there is ample endorsement of much that has already appeared in this journal, but he is by no means the only witness that we can appeal to. Even the President of the Chemical Society is compelled to acknowledge that the original researches brought before that Society have fallen nearly to zero. Commenting on this, the *British Medical Journal* points out that the institutions which were formerly considered the homes of research, are now silent. If these things be true, then if those who hold that research is a national necessity are right, our future position is not far to seek.

THE HIGHLANDS OF CENTRAL INDIA

The Highlands of Central India. By Captain J. Forsyth, Bengal Staff Corps. (London: Chapman and Hall.)

THIS is a book descriptive of that great tract of hill and forest country which is situated in the very centre of the Indian Peninsula, and whose drainage forms the headwaters of the Narbadá, the Tapti, the Sône, and affluents of the Màhànadi.

The author, who, in the course of his professional duties, acquired a close and intimate acquaintance with the country and its inhabitants, commences by giving a sketch of the probable history of the various races found in this wild and, until lately, almost unexplored country—races whose ethnological and religious history possess peculiar interest; because here the Aboriginal tribes, the races of mixed Aryan and Aboriginal blood, and the Hindoos proper, can be found living side by side; and the effects of the colonisation of the district by the Aryans of Hindustan can be traced out with some degree of clearness.

A considerable portion of the book is devoted to the author's sporting experiences. These he relates in a lively and spirited style; but as the reminiscences of Indian sportsmen have a strong family likeness, this part of the book does not call for much notice, except that the author appears to make rather too light of the danger of bison-shooting, and makes some statements concerning this game which are scarcely correct, one of these statements being that the animal is always known under the name of *bhìnsa*, or more correctly *bhainsa* (a word resembling in sound the English word bison), and never as *gaur*. Now in the very district of Northern Belàspùr which the author describes in the latter portion of the book, there is a high and prominent hill much frequented by these animals, and called in consequence Gaur-duàri, or the pass of the bison. The author also appears to confound the bison of Central India with the similar but not identical animal, known as the Mithan or Mithna, found on the hill ranges of Eastern and North-Eastern Bengal.

From the description given of some of the shrines and pilgrimages met with in these hill districts, an idea may be formed of the way in which the Hinduism of Hindustan proper has been localised; and how the scenes of some of the principal events of Hindu mythology have been transferred from the Ganges and Jumna to the Narbada and its adjacent heights.

The latter portion of the volume is devoted in a great measure to the country lying under and east of the Mykul range; that is the range which, according to the author's definition, forms the eastern boundary of the Highlands of Central India. This tract of country is remarkable, because it presents, perhaps, the greatest sweep of unbroken jungle and forest to be found in India south of the Ganges and Brahmaputra. It is neither plain nor hill, but rocky with gentle undulations, and covered with forest. Only a few miserable villages are found throughout the whole country; just enough to make its wildness and desolation more striking. Such a country, unfitted by nature for cultivation, and exhaling a deadly malaria, must long remain the home of the elephant, the tiger, and of the animals on which the latter preys; nor are there found any remains or traditions which would indicate that the place had ever been more thickly populated than it is at present. Here man is the inferior animal, and the wild elephant is the lord of the country. He roams at will from place to place, and, just when the unhappy villager looks to gather in his scanty rice crop, wrung with difficulty out of an unwilling soil, he invades the fields and browses at leisure on the ripening grain, utterly scorning the feeble efforts of the hapless owner to drive him away. Probably no European has braved the deadly malaria of the country at the time of the ripening of the rice crop, or that of the drying up of the rains, but the deep and enormous foot-prints imprinted in almost every field by this jungle ravager remain visible throughout the dry season, and tell the tale of spoliation only too plainly.

Throughout this district, and indeed with most of the tribes who inhabit the wilder and more remote parts of Central India, such religion as the people possess is "devil" worship and fetishism, with just so much gloss of Hinduism as may be imparted by a rare visit to a Hindu shrine, or the presence of a brahmin whenever anything is to be got—which is seldom.

A Gond legend, taken down from the lips of one of their most celebrated bards, and translated by the author into blank verse, in imitation of Longfellow's "Song of Hiawatha," is given at length; it is interesting because it brings in here and there some curious little bits of savage life, and also from the very strong resemblance of the whole legend to that on which Longfellow's poem is based. Moreover, as the author points out, a still more curious analogy will suggest itself to the careful reader.

The whole work is written in a very lively and readable style. It will be found amusing by those fond of sporting anecdotes; and though the subjects may be thought to have an interest too local for the general reader, yet the spectacle here presented of races of men just emerging from utter barbarism, and acquiring the rudiments of civilisation, gives to the country and its inhabitants an interest which it would not otherwise possess.

M. T. SALE

OUR BOOK SHELF

Lecture Notes for Chemical Students.—Vol. II. Organic Chemistry. By E. Frankland, D.C.L., F.R.S. (London: John Van Voorst.)

IN this, the second part of "Lecture Notes for Chemical Students," Dr. Frankland develops very fully his own peculiar notation. The use of thick letters, as **C H**$_4$; of contracted symbols for many organic radicles, as Ayo for $C_5H_{11}O$; of a peculiar way of writing the formulæ of well-known substances, so as at first sight to make them appear as if they were new compounds, as **N**PhO_2, representing nitrobenzol; these and a few other remarkable characteristics will make this book, we should think, seem rather startling to the ordinary student. By combining attendance on lectures on organic chemistry with careful reading and a good deal of patient work, the student will find these "Lecture Notes" very useful, containing as they do, in small space, the results of the latest investigations as these bear upon the molecular constitution of organic compounds. Very large use is made of graphic formulæ, and also of the theory of types. The types used are somewhat different from those with which we are familiar in the text-books, and certainly the names applied to them are derived more from an anatomical than a chemical vocabulary; thus, the marsh gas type is termed the "Monadelphic," the methyl type the "Diadelphic," and so on. The very useful and scientific method of writing the formulæ of all organic acids, as containing the group COHO; all alcohols, as containing CH_2HO; and all aldehydes, as containing COH, is adopted throughout. The relations of alcohols, aldehydes, and acids; the passage from one class of alcohols, &c., to another; and very many other points of great scientific value, too commonly overlooked in the text-books, are here all carefully noted. As we said, the book requires attentive study, but this it will certainly repay.

M.M.P.M.

Thermal Paths to the Pole. An Address delivered before the St. Louis Mercantile Library Association in January 1872. By Silas Bent.

IN this pamphlet the author repeats the substance of a lecture delivered in 1868, the object of which was to show that the continuations of the warm Gulf Stream of the Atlantic, and of the Japan current in the Pacific, afford the only practicable avenues by which ships can enter an assumed open sea round the North Pole; and points out how the more recent Arctic explorations have confirmed the views then advanced.

The author's opinions should derive weight from the fact that he was one of the leading scientific observers in the American expedition of 1852, during which the Japan current was mapped out, and from his twenty-five years of observations at sea.

So far as the warm drift continuing the Gulf Stream into the Arctic region between Spitzbergen and Nova Zemlia is concerned, the theory of its influence (which, however, can hardly be called "original," since it has been current among Arctic authorities for many years) has indeed been remarkably confirmed in the past year by Payer and Weyprecht's voyage in open water to 79° N. lat. But it remains to be shown that the summer current from the Pacific through the narrow and shallow passage of Behring Straits has any considerable influence on the condition of the Arctic basin. In the circumpolar chart which accompanies the pamphlet, Behring Straits has been carefully widened to admit the Kuro-Siwo in a breadth quite equivalent to that of the Gulf Stream drift.

A considerable portion of the address is devoted to the description of a method by which "were it not for the inhumanity of exercising such a power," the whole of Europe might be placed at the mercy of America. Europe derives its mild climate from the Gulf Stream, and to divert this stream from its present direction would be to make "Europe a frozen wilderness." This grand result, the author believes, could be accomplished "by the possession of the Isthmus of Panama and the expenditure of half the cost of the recent war between France and Germany, in the excavation of a sufficient width and depth of the rock only that intervenes between the Caribbean Sea and the Pacific." Mr. Bent has himself, however, thrown some doubt on the entire practicability of his design by quoting, in a previous paragraph, the belief expressed by Professor Maury that the great mass of the Gulf Stream is formed by that part of the equatorial current of the Atlantic which passes to northward of the Antilles, and which "must be a hundredfold greater than that which returns to the east from the Gulf of Mexico;" he has also omitted to notice that the force of the drift in the Caribbean Sea is not directed in any degree against the narrower portion of the isthmus, and we presume that even Mr. Bent would not attack the plateaus of Guatemala or Mexico.

K. J.

LETTERS TO THE EDITOR

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

Spectroscopic Nomenclature

THE letter of Captain Herschel upon "Spectroscopic Nomenclature," which appeared in NATURE of April 25 contains many criticisms that are eminently just and timely, but there are one or two points in respect to which I should like to express dissent.

Thus as regards the name D_3 (to which he objects) for the yellow line of the chromosphere spectrum, it is not easy to see what other designation would better convey to the mind an idea of its position in the spectrum and its importance, without involving any assumption, or hypothesis even, as to the material causing it.

To say nothing of the fact that the whole Greek alphabet would not suffice to name one in three of the bright lines which have been observed in the chromosphere spectrum, there is this further difficulty, that if the letters are to be applied to lines in the order of discovery, ω is as likely to fall between α and β as anywhere else, but if according to position in the spectrum, then every discovery of new lines involves a revision of the nomenclature.

It seems doubtful therefore whether any better system is possible than to designate lines by reference to some standard map of the spectrum, as stars are catalogued by their right ascension and declination.

An accurate chart of the solar spectrum on which the lines should be mapped according to "inverse wave-length," proposed by Captain Herschel himself I believe, as well as by Mr. Stoney and others, would sufficiently resemble the spectrum seen in a spectroscope to be equally convenient in the observatory with that of Kirchhoff, and would be free from the reproach of arbitrariness and irregularity in its scale. Such a chart would be most gladly welcomed by all spectroscopists, and would immediately supersede those of Kirchhoff and Angström.

With reference to the green corona line, he writes "and now we have '1474.' No one knows what the true position of that line is. The line 1474 K is an iron line, and it is to the last degree improbable that the corona line is identical with it." I am not quite sure what is meant by the second clause. If only, that the position of this line may *possibly* (not by any means *probably*) be doubtful to the extent of ½ of one of Kirchhoff's scale divisions, that is about ¼ the distance between the two E lines, I have nothing to object.

But if the sentence is intended, as one would naturally suppose, to convey the idea that the position of the line is not very accurately determined, and may be considered uncertain to the extent of several scale divisions, it is certainly wrong. I *know* of what I affirm, and perhaps may be allowed to refer to an article in this journal for Feb. 2, 1871, in which the evidence is stated as it was at that time, and it has received confirmation since.

Indeed as this bright line is almost always visible in the chromosphere to an instrument of sufficient power, I think I may confidently appeal to Mr. Lockyer or Dr. Huggins to bear me out in the statement that the bright scarlet line of the chromosphere appears to coincide no more perfectly with the dark C, than does this green corona line with the dark line at 1474 K.

I confess I am almost sorry that the spectrum of iron shows a bright line coincident with 1474, for all things considered, I cannot think that iron vapour has anything to do with this line in the spectrum of the corona, and the coincidence has probably only served to mislead.

But there are in the spectrum many cases of lines belonging to the spectra of different metals coinciding, if not absolutely, yet so closely that no existing spectroscope can separate them, and I am disposed to believe that this close coincidence is not accidental, but probably points to some physical relationship, some similarity of molecular constitution perhaps, between the metals concerned.

So in the case of the green coronal matter, is it not likely that, though not iron, it may turn out to bear some important relation to that metal? And yet I for one should be very glad if the application of higher dispersive power should show the apparent coincidence to be merely a very close juxtaposition.

C. A. YOUNG

Dartmouth College, U.S.A., May 16

Historical Note on the Method of Least Squares

THIS excellent method for the discussion of observations was published and first practically applied by Gauss in his Theoria Motûs, 1808. In the *American Journal of Science* for June 1871, Mr. Cleveland Abbe has shown that Prof. Robert Adrain, of New Brunswick, New Jersey, U.S., independently discovered the same method in 1808. I wish to call attention to what seems to me a singular oversight in the history of this subject, viz., to the fact that in 1770—1773, Lagrange published an elaborate memoir at Turin under the title "Mémoire sur l'utilité de la Méthode de prendre le Milieu entre les resultats de plusieurs Observations," &c. *Vide* "Œuvres de Lagrange," edited by J. A. Serret, vol. 2.

I have never seen any notice of this memoir except a translation of a part of it into German by Encke, published in the *Berliner Jahrbuch* for 1853. Thus in the abstract of a memoir by Mr. J. W. L. Glaisher, given in the notices of the Royal Astronomical Society for April 1872, the name of Lagrange does not occur.

I think that the English mathematician, Thomas Simpson, busied himself with this problem about 1750, but I am not able to refer to his works. ASAPH HALL

Washington, May 22

The Volcanoes of Central France

AN unlucky error, perhaps mine, in the letter on the "Volcanoes of Central France," p. 80, will quite prevent any reader finding the paper I mentioned of May 1865, which, instead of being in the *Gentleman's Magazine*, was in the *Englishman's Magazine*, a short-lived periodical, begun and ended, I think, with that year. As your two correspondents, Prof. Corfield and the Rev. Mr. Webb, like the writer of that paper, repeat the late Dr. Daubeny's most marvellous "conclusion" that there might have been nothing more eruptive in the phenomena than "bursting out of flames" from earthquake fissures, and even that the fires mentioned by Sidonius and Avitus might be "domestic conflagrations," may I briefly indicate the grounds that make these suppositions to me incredible? These fires, as named in the portions of each document that I have translated—quite distinct from the conflagration of some public building on the Easter festival of a previous year, which both writers afterwards relate at greater length as an earlier and less known case of successful prayer by Mamertus, the memory of which had encouraged him under these "prodigies" and "portents," the *ignes* (not *incendia*) that both writers make a chief or the chief part of the "terrors"—(Sidonius, indeed, names the earthquakes before them, but Avitus twice over puts the fires first)—these were *crebri* and *assidui*, continual for two or three years, yet not a word of what they fed on or what valuables they destroyed, and they were only *sæpe flammati*. Their being so sometimes is plainly named by Sidonius as an unusual and greater portent. Now, I never heard of any "domestic" fires that were *not* "*flammati*," whereas volcanic eruptions, even severe, seldom if ever involve flame truly so called, though their strongly illuminated smoke may often by night be mistaken for flames, and has led them to be called in extreme cases, as Sidonius here said, *sæpe flammati*. He adds that when thus "*flammati*" they did, or rather threatened to do, the only mischief named as even apprehended from them at the capital, the endangering frail roofs by a load of ashes thrown over, *superjecto favillarum monte*. Now, surely this is not an effect of any ædile conflagrations however often repeated (a repetition that would anywhere have been regarded rather as suspicious of incendiarism than as "prodigious" and preternatural). Nor would any such accidents lead Avitus to ask in his sermon to those who remembered all, "Who would not dread the Sodomitic showers?" Again, Mr. Webb conceives that earthquakes might not only drive the wealthier part of the population out of the city, "but, as it would seem, the beasts into it!" I never heard of shocks producing so singular an effect as driving any living thing into cities or buildings, and cannot conceive what natural event could so drive them, unless what is here by both witnesses implied, "Sodomitic showers" of hot or cumbering *favillæ*. Such showers, which we know to be often carried, from eruptions involving no lava, scores or even hundreds of miles, in the direction of the prevailing winds, would be carried from any of the well-known cones of the Forèz or Vivarais, towards, or even far beyond, Vienne; and wild animals, fleeing north-eastward, would have no refuge but under roofs; and if private house doors were habitually shut (as now in England) might crowd into the colonnades (*fori latera*) of that capital city. This incursion of the wild deer, bears, and wolves into towns was so well remembered as to become, in the later chroniclers, Gregory of Tours, &c., dwelt upon among the main "prodig es" of the time, along with the earthquakes and burnings of buildings, though any other fires cease to be implied; and the reason of this is obvious on comparing their accounts. They all copy one another, and the earliest, whose sole authorities were those two pompous and involved writers, mis-read them exactly as our moderns (except Sir F. Palgrave) appear to have done, confusing together the fires of the "prodigies," that led to the Rogation fasts with the earlier ædile conflagration at some Easter, said to have been prayed out by Mamertus, which occupies both the writer and preacher immediately after, and at greater length than these well-known "terrors" remembered by those they addressed personally.

The whole strikingly shows, as Sir F. Palgrave said, the fallacy of geological inferences from the "silence of history" (or what may be deemed silence) in times and places practically prehistoric, or at least preter-historic. He showed that, but for Pliny and a mere accident, we should probably have been as ignorant of even the Pompeii and Herculaneum eruption as of these equally attested ones. Again, the Spaniards would have preserved us no memory of the rise of Jorullo, in the very last century; and yet probably no part of Gaul in the generation when the Romans lost it was really more settled and populous than Mexico in its third century of Spanish rule. The only important colonies within moderate shower-range of the eastern volcanoes were Vienne and Lyons, the latter farther off, and not at that time a capital, indeed but little heard of in those early middle ages. And fires, not called damaging, only "prodigious" and terrifying to Vienne, and causing "Sodomitic showers" there, need not have been within a few miles, but far in the wilds, then hardly trodden, of its mountainous south-western horizon.

E. L. GARBETT

7, Mornington Road, N.W., June 1

Temperature of the Deep Sea

WILL you allow me to ask, through your pages, if there be any rule for ascertaining the temperature of the sea at given depths below the surface? To practical electricians such a rule would be very valuable.

I will state a case. There is a submarine cable connecting two stations, A and B, 150 miles distant. The temperature at A is 75° Fah.; that at B, 68° Fah.; and the average depth at which the cable lies 120 fathoms: what is the average temperature of the cable?

If you could refer me to any work in which this point is treated I shall be obliged. F.

ENDOWMENT OF PROFESSORSHIPS

THE following correspondence between Professors H. E. Roscoe and B. Stewart, of Owens College, Manchester, and the Chancellor of the Exchequer, is published in the *Times* of Monday last :—

"TO THE RIGHT HON. ROBERT LOWE, CHANCELLOR OF THE EXCHEQUER.

"Owens College, Manchester, May 21, 1862.

"SIR,—In the *Times* of May 17 you are reported, at the presentation for Degrees at the University of London, to have pointed out 'how the endowment of Professorships naturally tended to make teaching inefficient (seeing that the revenues come in independently of the results of teaching), suggesting that those who had any money to spare for the advancement of education should rather make it available in the forms of Scholarships and Exhibitions.'

"While we gratefully acknowledge the many services which, as Chancellor of the Exchequer, you have rendered to the cause of knowledge, we yet feel most strongly that the above expressions are calculated to mislead, and that were your suggestions to be carried out, the result would be fatal to the higher education of this country.

"We therefore request permission to lay before you our own views on this most important subject. Writing from the very house once inhabited by Cobden, we feel proud to be connected with a city which was the birth-place of Free Trade; yet we feel equally privileged to form part of a very useful institution which never could have existed except in apparent contradiction to the principles of Free Trade.

"That the foundation by the late John Owens of Professorships of Arts and Sciences in the midst of this great city was not thought by Cobden to be subversive of his principles is proved by the fact that he himself was one of the original trustees, yet this conclusion does not appear equally clear to all of his disciples.

"We are, in sober truth, utterly at a loss to conceive how the higher education of the country can be efficiently carried on without a moderate endowment of its Professorships. The necessity for such an education you yourself admit.

"A single example from our own staff, which, more or less, applies to other places and subjects, will render our argument clear. It is evidently of very great importance that in a place like Manchester the citizens should be taught by a master mind the principles of political economy, and they have been fortunate in being able to avail themselves of the services of such a man as our colleague, Professor Jevons. But, although here both elements of pecuniary success might appear to be present in an intelligent public and a first-rate teacher, the fact remains that without the (misguided !) endowment of our founder the few who attend his lectures could not have benefited from the teaching of Professor Jevons unless the fees of attendance had been enormously increased. Indeed, we question whether the great apostle of Free Trade himself would have ultimately met with success had he not first of all received some sort of protection and support.

"We are naturally led by the instance we have quoted to remark that endowments really tend to diminish the expenses of education, and, looking around us, we see that in University College and King's College (London), where there are no endowments, they cannot afford to give their education at so low a figure as is possible at Owens College and in the Scotch Colleges, where endowments exist.

"In the German Universities, again, where all the important Chairs are well endowed, the expenses of education are almost nominal. In Scotland the education is in some branches of a very high standard, and in others great improvements have recently taken place, chiefly in the direction of relieving the head Professors from the duty of teaching junior classes which pay, and of enabling them to devote their energies to senior classes which do not pay. Such, in Scotland, have been the effects of endowments. Again, with regard to Germany, we have never heard any complaints made of the inefficiency of the German Professors.

"We must candidly own that we were much surprised by your statement as to the advisability of simply founding Scholarships and Exhibitions, coming, as it does, from a distinguished Oxford man well acquainted with the present state of feeling in the older Universities. Is it not true that this feeling is strongly against the extension of the already too numerous Scholarships, Fellowships, and other incitements to study, and in favour of the application of these funds to increase the paltry salaries of the Professors ?

"The excessive endowment of Scholarships appears to us to be objectionable, as an instance of unnecessary protection, where, by means of a hotbed regimen, young men are induced to enter a profession for which there is no subsequent career.

"While we admit that in a perfect state of society (unhappily still far distant) the laws of supply and demand may perhaps be applicable to all knowledge, yet we must point out that the teachers of the higher branches have too often now to create a taste for the commodity which they supply, and hence we believe that the moderate endowment of Professorships, such as exists in our own case, is essential to the progress of civilisation in this country.

"In conclusion, Sir, we cannot understand why endowment naturally tends to make teaching inefficient in the case of a Professor of science or arts more than it does in that of a minister of religion or a statesman.

"Are they not all servants of the nation administering to its higher needs ? The teacher of science or of the arts will, we venture to say, be no less conscientious and faithful to the true interests of a noble cause in teaching his class than the minister of religion in addressing his congregation, or the Minister of State in addressing his constituents.

"We are, Sir, your obedient servants,
"HENRY E. ROSCOE, B.A. (Lond.) F.R.S.
"BALFOUR STEWART, LL.D., F.R.S."

"11, Downing Street, Whitehall, May 23, 1872

"Gentlemen,—The speech which I made at the annual meeting of the London University occupied three-quarters of an hour, and was reported in a few lines. I never alluded to Professors, but spoke only of teachers, meaning those who do the drudgery or hard work of teaching, not those who are devoted to the investigation and inculcation of higher and more refined knowledge. I have the greatest respect for Mr. Jevons, and do not doubt that the endowment of his Chair is money well laid out.

"I also agree—indeed, I said—that the endowment both of Fellowships and Scholarships at Oxford and Cambridge is excessive ; but I pointed out how hard the competition was for the London University, with strict examinations and hardly any endowment, against Oxford and Cambridge, with rich endowments and easy examinations. I added that in my judgment money was better spent in giving Exhibitions to young men, leaving them free to choose the place of their education, than in paying persons to teach them ; since in the one case the inducement to the teacher to work was diminished, while in the other the student with money in his hand was sure to find the best teacher for himself.

"I am, Gentlemen, your obedient servant,
"ROBERT LOWE.

"I am an older Freetrader than Mr. Cobden, and am by no means prepared to assent to his views in all respects."

*GLAISHER'S (HALL'S IMPROVED) RAIN GAUGE**

IN the first paragraph of my "Notes on the Rainfall of 1871," which recently appeared in NATURE (vol. v., p. 481) your readers will probably have noticed certain reference to the above.

The improvement to which I refer consists of an *inverted rim* (similar to the rim or flange in which the receiver stands) fixed to the outside cylinder of the receiver, and made sufficiently large to admit of its dropping over the rim or flange, sometimes called "channel," fixed to the lower cylinder, *id est*, the one just mentioned in parenthesis.

The *inverted rim* is shown by a thick line on the right of the accompanying half-sectional diagram.

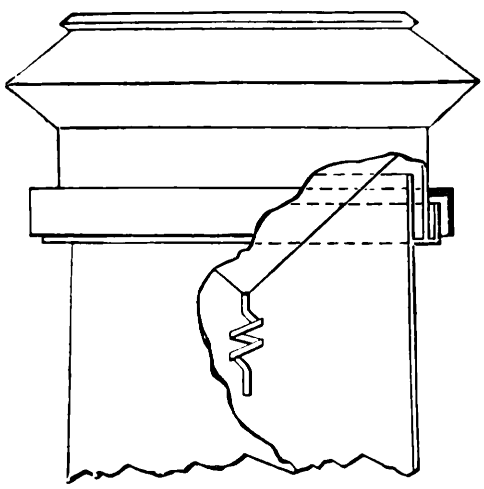
The reason that I suggested this addition was, that on one occasion, while registering the daily rainfall at Twickenham, during the winter of 1869-70, I was unable to take the receiver off, owing to the already existing channel being partly filled with water, which had frozen hard during the night.†

It is intended that water should be collected in the flange (Glaisher's) I have spoken of, and thus close the gauge against evaporation, scarcely a good idea theoretically, *certainly not practically*, inasmuch as the water

* Vide *Scientific Opinion*, vol. iii. pp. 449, 450.

† In order to avoid the interference of houses and trees, my gauge was supported at this time on a bracket carriage, running in vertical slides from a staircase window to a point a few feet above the roof of my residence.

collected therein so soon evaporates, especially in hot weather; *vide* "Symon's British Rainfall, 1868;" "Rain Gauge Experiments at Strathfield Turgiss, Reading," by the Rev. C. H. Griffith, F.M.S., &c., p. 23, which further establishes my remarks. The absence of outlet for confined air here spoken of, might be remedied, if indeed needed, by drilling small air-holes in the bottom of the receiving cylinder and upright flange, *but not facing each other*. I have two Glaisher's gauges fitted with the inverted flange arrangement, both of which answer remarkably well. I believe the improvement which I have adopted is more effectual against loss by evaporation (during all weathers) than the present (Glaisher's) system.



One of my gauges has been further improved at my suggestion by being fitted with a spiral or *helical* pipe in the place of the J-shaped pipe, thereby presenting no direct opening for evaporation, at the same time offering little or no hindrance to the speedy descent of the rain-water; but this is a matter I hope to enlarge upon in a paper (as yet unpublished) which I hope to communicate to you shortly, "On a Proposed New Form of Rain Gauge (the Atmospileometer)," in which a similar, but more extensive, idea, is shown.

The particulars mentioned in paragraphs 1 and 3 of this letter were long since communicated to and approved by the Secretary of the Rainfall Committee of the British Association (G. J. Symons, Esq., F.M.S., &c.) one of the highest authorities in matters relating to rainfall.

JOHN JAMES HALL

WATER ANALYSIS

I.

IT is now upwards of twenty years since the inhabitants of this country, and especially of the metropolis, were awakened, by a succession of virulent attacks of epidemic cholera, from the profound indifference with which they had regarded all matters connected with the public health for the hundred and eighty years which had succeeded the Great Plague. During that interval builders had been allowed to cover land with hundreds of acres of dwellings built without regard to ventilation, drainage, or water supply. In all the towns and villages of the country the ground was honeycombed with cesspools and wells, the latter deriving their supply of water at least in part from the former, and in all riverside towns the river either received the town sewage at once or after it had passed through the cesspools. Attention once being drawn to the matter, it became the duty of the chemist to detect the various polluting matters introduced by the sewage into the different sources of water-supply, and to discover, if he could, waters that were free from this pollution; and a still greater field was opened up, for the two first inquiries naturally led to the allied questions—How can sewage be rendered harmless? and Can slightly polluted water be rendered safely drinkable by the removal from it of the contaminating matter introduced by sewage? We propose in this article to look only at the first of these questions, the one on the successful solution of which depends all the others—Can organic contamination be detected and estimated with accuracy? As soon as the question was approached it was found to be one of extraordinary difficulty, and in 1856 Hofmann and Blyth drew attention to the inaccuracy of the then existing processes, especially of the one known as "loss on ignition" obtained by igniting the solid residue on the evaporation of the water. This loss, then generally looked on as affording a measure of the organic matter present in the water, they proved to consist of a loss of carbonic anhydride, nitric acid, ammonia, and moisture, &c., and they proposed to render the determination more accurate by the addition of a known weight of sodic carbonate, which, while it drove off the ammonia (usually a very small fraction of the loss), retained the acids and prevented the aqueous magnesic chloride from losing hydrochloric acid. The same chemists pointed out the necessity of determining the amount of nitrogen present, but were unable to recommend any process for its estimation. The methods for estimating the ammonia were also very unsatisfactory, for we find Dr. Dundas Thomson in 1855 distilling as much as fifty gallons of the metropolitan water-supplies in order to estimate the ammonia, which was done by titration with standard acid; and this when some of the metropolitan supply was taken from the Thames at Vauxhall, and "Fibrin from Fæces" could be distinctly recognised in the Southwark Company's water. Another process, devised by Forchhammer, was also in use for the determination of the organic matter, which consisted in adding a standard solution of potassic permanganate until no further loss of colour occurred. This process had been improved from time to time, and was and is largely used. The only other test was that for hardness, invented by Dr. Clark, and which is still in use, with but slight modification from the original method.

If these processes are considered but shortly, the defects they possess are at once apparent. Take, for instance, Hofmann and Blyth's improved solid residue process. On ignition there was great danger of decrepitation and consequent loss, notwithstanding the high temperature (120° to 130° C.) to which the residue had been exposed. Frankland and Armstrong have shown that portions of the nitrogenous matter were liable to remain fixed in the ignited residue as cyanogen compounds. Again, in the case of some artificial residues prepared by treating dilute solutions of urea as in the above process, from 44 to 59 per cent. of the urea used was found to have been lost during the preliminary evaporation, the sodic carbonate having expelled it as ammonic carbonate. And on the treatment of similar residues by ignition, from 58 to 85 per cent. of the organic matter was left in the residue. It was usual also to restore the lost carbonic anhydride to the ignited residue by evaporating a solution of that gas on it and weighing until a fresh treatment did not increase the weight; but to still further increase the difficulty of this unhappy process, it was shown that some residues seemed to have the power of taking up such quantities of carbonic anhydride, that they weighed more after this treatment than they did before ignition. The estimation of ammonia by titration with acid needs no argument against it; the enormous quantities of water necessary for the determination sufficiently condemn it; and it has been long superseded by the admirable quantitative form of the Nessler process invented by the late Mr. Hadow, of King's College. The permanganate process, however, being an easy one to perform, still survives in the laboratories of many analysts. Indeed, not content with giving the results of this determination as "oxygen required to oxidise the organic matter present," the lively imagination of some led them to the remarkable conclusion that every grain of oxygen oxidised eight grains of "organic matter."

Whether this test is to be trusted may be judged from the following facts. Potassic permanganate is deoxidised by ferrous salts, nitrites, sulphites, &c., much more rapidly than by organic substances, so that a water absolutely free from any organic matter, but containing one of these compounds, would be set down as requiring so much "oxygen to oxidise organic matter." Secondly, in the case of water to which known weights of various organic compounds were added, Frankland and Armstrong found that in no instance was the oxidation complete, even after the lapse of six hours. In fact, even after that time the amount of oxygen actually absorbed was in every case a mere fraction of the quantity actually required to completely oxidise the organic substance. The test, though thus shown to be valueless as quantitative, is of some value qualitatively, as it can be easily and quickly applied; and it may be said that, though it might induce a person to abandon a good water, it would not often lead him to use a bad one.

All the above processes were in use up to 1868, when Messrs. Chapman, Wanklyn, and Smith proposed to determine the organic matter in water from the amount of ammonia evolved when the water was treated with a strongly alkaline solution of potassic permanganate and then distilled, the ammonia being determined in the distillate by Hadow's modification of the Nessler test.

That albumin is decomposed and the nitrogen thus evolved, they had shown in a paper presented to the Chemical Society in the preceding year. The way in which this process was applied to the water may be briefly stated as follows:—A measured quantity of the water was rendered alkaline with freshly-ignited sodic carbonate, and the ammonia distilled off and estimated by Hadow's modified Nessler process. As soon as all the ammonia thus obtainable had been expelled, the alkaline permanganate solution (50 cubic centimetres of a solution containing 200 grammes of potassic hydrate and 8 grammes potassic permanganate per litre) was run in. The distillation was then resumed, and the ammonia estimated as before. This last was set down as "albuminoid ammonia," and as the average evolution of ammonia from the following substances, gelatine, caseine, dry albumin, uric acid, creatine, theine, dried fish flesh, amounted to 10 per cent., it was suggested the albuminoid ammonia multiplied by 10 gave a fair estimation of amount of organic matter.

It had at first been stated that albumin gave up the whole of its nitrogen when treated with alkaline permanganate, but the statement was subsequently modified to "It appears to be two-thirds, being at any rate a constant quantity." Now this process would indeed be a valuable one if the 10 per cent. average could be depended on, or if the albumin evolved a certain quantity, and the above substances were alone found in water. Unfortunately, none of these suppositions are true. With regard to the last, the authors themselves recognised the difficulty, and accordingly examined a number of other nitrogenous organic bodies; which examination led to the publication of two lists of bodies that evolve the whole of their nitrogen as ammonia, and bodies that yield various fractions. Frankland and Armstrong also made some experiments on this subject. With regard to the list of bodies yielding half their ammonia, the numbers given by the authors vary from 44 per cent. in the case of papaverine to 58 per cent. in the case of sulphate of cinchonine; and whilst narcotine appears in Wanklyn, Chapman, and Smith's list as evolving all its nitrogen, Frankland and Armstrong give it as evolving about 46 per cent. Strychnine, given by the former authors in their list as evolving 53 per cent., is given by the latter as evolving 31½ per cent., and sulphate of quinine also in the list with 50 per cent., appears again with Frankland to have evolved nearly 57 per cent. No other examples will be necessary to show the extreme uncertainty of the process. If the authors had enabled us to ascertain the absolute error on the quantity taken instead of the per-centage error, by giving us the quantities from which the results were taken, it would no doubt be much more apparent; the results given above in the case of those from Frankland and Armstrong's paper are absolute errors.

But it may be urged in defence of the method that none of these bodies are found, or are likely to be found, in natural waters. Let this be granted, and the process must be defended on the albumin and other bodies mentioned, and on the list of bodies giving up all their ammonia. The average from this is 11·82 per cent.; from the albumin list, 9·92; or, taking the two, 10·87. So far the lists hold good. But it must be borne in mind that we have three different statements about the ammonia evolved from albumin—first, that all is evolved; secondly, "two-thirds, or, at any rate, a constant quantity;" thirdly,* that 100 parts of albumin give 10 parts of ammonia.† The inconsistency of these statements needs but this comment, that they can only be caused by the extreme uncertainty of the process; in fact, the amount of nitrogen converted into ammonia will be influenced by the nature of its previous combination, the degree of concentration of the solution, and the amount of heat applied to the retort, and consequent rate of distillation and time to which the solution is exposed to the action of the alkaline permanganate. That this is the case is proved by the fact that water which has been distilled from alkaline permanganate, and gives no trace of reaction with Nessler's test, will evolve ammonia if again boiled with the permanganate. Lastly, let any one take a water which has been largely contaminated with sewage and then filtered, such as the effluent water from a sewage farm. Such water, as a rule, contains much nitric and nitrous acid, and comparatively small quantities of organic nitrogen. A water of this character continues to evolve albuminoid ammonia till boiled nearly to dryness, and not unfrequently the retort requires to be filled up with pure water, and the operation carried on. The process is thus not only rendered tedious, but the necessity of repeatedly taking samples of the distillate and estimating the ammonia in them introduces an amount of experimental error which becomes serious when calculated out in milligrams per litre, though its actual amount on each cylinder of distillate may be very small.

When it is added that Mr. E. T. Chapman has included in the second edition of Wanklyn's "Water Analysis" a process for the estimation of volatile organic matter, founded on the fact that water largely contaminated with sewage evolves volatile bases when boiled with potassic hydrate, the question is still further complicated; for it cannot be doubted that some portion of these bases would be driven off by the action of the alkaline permanganate before it had time to act on and destroy them.‡ Whether this is the case ought at once to be determined by those who use the process.

In a second article we shall consider Frankland and Armstrong's process for the analysis of potable water, and also those determinations, such as nitrous and nitric acids and chlorine, which are of great value as enabling us to trace back the history of a water, and to tell from whence it is derived and what it has received in the way of animal contamination before it came into our hands.

* "Water Analysis," 2nd edition, p. 66.

† If Lieberkühn's formula for albumin $H\,Na\,C_{72}\,H_{110}\,N_{18}\,S\,O_{22},\,H_2O$ be taken as the true one, 10 parts of ammonia (NH_3) from 100 parts of albumin will be equal to 8·23 parts, or little more than half the total nitrogen, which is for that formula 15·25 per cent. If the sodium in the above formula be replaced by hydrogen, the discrepancy is still greater.

‡ An action of this nature apparently occurs when sewage is treated with alkaline permanganate, as nearly the whole of the ammonia comes off in the first 100 cub. cent. of the distillate, and after that the evolution soon stops. Waters that evolve small quantities of albuminoid ammonia seem almost always to require a long time for its liberation. Is this because the organic bases are so diluted that they cannot be driven out, and so slowly decompose

RADIATION AT DIFFERENT TEMPERATURES

BALFOUR STEWART states in his "Elementary Treatise on Heat" that "Newton was the first to enunciate his views on the cooling of bodies. He supposed that a heated body exposed to a certain cooling cause would lose at each instant a quantity of heat proportionate to the excess of its temperature above that of the surrounding air." In order to prove the fallacy of Newton's supposition, Prof. Stewart presents the following extract from the work of MM. Dulong and Petit:—

Excess of temperature of the thermometer. °C	Velocity of cooling. °C
240	10·69
220	8·81
200	7·40
180	6·10
160	4·89
140	3·88
120	3·02
100	2·30
80	1·74

"We see at once from this table," says Prof. Stewart, "that the law of Newton does not hold, for according to it the velocity of cooling for an excess of 200° should be precisely double of that for an excess of 100°; now we find that it is more than three times as much." The author of the Elementary Treatise on Heat thus assumes that the velocity of cooling established by Dulong and Petit represents the radiant energy or quantity of heat transmitted by the radiator. Consequently, the amount of energy at 200° is assumed to be $\frac{10 \cdot 69}{1 \cdot 74} = 6 \cdot 14$ times greater than at 80°; while, agreeably to Newton's law, the increase of radiant energy should be proportional to the differential temperature, viz., $\frac{240}{80} = 3$ times that of the tabulated temperature of 80°. Modern research having established that radiant heat is energy amenable to the laws of dynamics, it may be demonstrated that the deviation from the Newtonian doctrine assumed by Dulong and Petit is groundless; but, before considering the theory, let us examine the practical result of recent elaborate experiments conducted with an apparatus containing the spherical radiator adverted to in the preceding article on Solar Temperature (vol. v. pp. 505-507). The accompanying illustration (Fig. 1) represents a vertical section of the said apparatus, *a* being a spherical vessel 5 inches in diameter, suspended within an exterior casing *b*, filled with water. A spherical radiator, *c*, 2·75 inches in diameter, composed of very thin copper, charged with water and coated with lamp-black, is sustained in the centre of the sphere *a* by means of tubes applied above and below. The upper tube is large enough to admit the bulb of a thermometer, the lower one being only sufficiently large to accommodate a small axle, to which is attached a paddle-wheel, provided with curved paddles, arranged in such a manner that the bulb of the thermometer may be inserted considerably beyond the centre of the sphere, as shown in the illustration. The external casing *b* is provided with nozzles, *g* and *d*, to which tubes are attached for circulating cold water through the annular space during experiments. The air is exhausted from the spherical enclosure through the tube *k*, which passes across the annular space. It will be evident that the centrifugal action of the paddles of the wheel applied within the radiating sphere will produce a continuous current from the centre towards the circumference, the fluid successively passing over and coming in contact with the inside of the thin shell, then returning to the centre to be again thrown off by the centrifugal action. The rotary motion of the water, kept up without intermission round the cylindrical bulb of the thermometer, will evidently render its indication prompt and reliable. It is hardly necessary to observe that the rapid presentation of fresh particles of water promoted by the action of the paddles, will effectually prevent the reduction of temperature to proceed faster at the circumference than at the centre, the radiation at the surface, in virtue of the continuous interchange of particles, affecting almost simultaneously every molecule within the sphere. Consequently the total energy of radiation will be rendered available in reducing the temperature of the contents of the radiator, while the central thermometer will indicate at every instant the precise degree of temperature of the entire mass.*

The mode of conducting the experiment will be seen by the following statement:—A wooden cistern containing 16 gallons, charged with water and crushed ice, is connected by flexible tubes to the nozzles *g* and *d* on opposite sides of the annular space, a pump being applied between the cistern and the said nozzles, by means of which the cold water is forced through the apparatus and then returned to the cistern.

In view of the great importance of the question at issue, the investigation has been conducted with the utmost care, four operators having invariably been employed during the experiments, the labour being thus divided: 1st operator regulates the temperature of the water in the cistern by continual agitation and supply of crushed ice from time to time; 2nd operator works the pump at a uniform rate; 3rd operator turns the paddle-wheel, and reads the thermometer under a magnifying glass, calling time for each degree at the instant when the top of the mercurial column is covered by half the thickness of the line on the scale. Lastly, the 4th operator, provided with a Casella chronograph, records the time called. It will be seen presently that, notwithstanding this procedure, there is a slight discrepancy in the ratio of temperature and time, viz., the increment of time for each degree is not regular. Obviously the most practised eye cannot determine exactly at what moment the top of the falling column is half covered by the line on the thermometric scale. Again a perfectly graduated thermometer cannot be obtained. But the discrepancy referred to in reality only disfigures the record, since the computations are based on *mean* time. Referring to the accompanying table, it will be seen that the rate at which the spherical radiator cools has been recorded separately for each degree of differential temperature from 100° to 10°, the enclosure being maintained at a constant temperature of 33°. Regarding the construction of the table, it will suffice to state that the time entered in the fourth column is that shown by the chronograph. It will be evident on reflection that the increment of time for each successive degree of differential temperature expresses very nearly the rate of cooling; but, the recorded times being irregular, from causes already pointed out, the true increment cannot be determined without ascertaining the mean time recorded by the chronograph. This mean time will be found in the fifth column, the true increment, viz., the number of seconds during which the temperature of the radiator falls one degree, being entered in the last column.

Let us now examine the accompanying diagram Fig. 2, in which the ordinates of the curve *a b* represent the observed time for each degree of differential temperature, while the ordinates of the curve *a c* represent the corrected time. The diagram having been constructed with the utmost exactness, in accordance with the temperature and time in the table, mere inspection will show that the observed and corrected times have produced curves nearly identical. Agreeably to Newton's law the rate of cooling is proportional to the excess of temperature of a body above that of the surrounding medium. Hence the increment of time for each degree, in other words, the number of seconds occupied in reducing the temperature of the radiator 1° (inserted in the last column of the table) should be proportional to the differential temperature inserted in the third column. For instance the rate of cooling at a differential temperature of 49° being 39·80 seconds for 1°, it should be $\frac{49 \times 39 \cdot 80}{31} = 62 \cdot 90$ seconds for an equal thermometric interval at a differential temperature of 31°. Referring to the table, it will be found that the rate thus computed agrees exactly with the increment of time inserted in the last column opposite the differential temperature 31°.

Applying a similar test to the other differential temperatures and rates of cooling contained in the table, the same exact agreement will be found to exist. Consequently, our table and diagram prove that the rate of cooling is proportional to the differential temperature, thus establishing the correctness of the Newtonian law. Regarding the discrepancy indicated by the slight irregularity of the curve *ab*, the writer attributes the same to the

* It might be supposed that the motion of the water within the radiating sphere, produced by the action of the paddle wheel, will occasion an elevation of temperature tending to render the indication of the central thermometer inaccurate. The requisite speed of the wheel being 30 turns per minute, experiments have been made to ascertain if at that rate heat is produced; but no elevation of temperature has been observed. The diameter of the wheel being 2·37in., the maximum speed of the particles of water produced by the rotation is scarcely 3·8ins. per second, a velocity too small to generate appreciable heat.

difference of emissive power of the radiator at different temperatures. It was stated in the preceding article, (vol. v. pp. 505-507), that the radiant power of one square foot of cast-iron develops 0·080 thermal unit per minute for each degree of differential temperature at 65°, and 0·337 unit at 3,000°; hence that the emissive power is increased $\frac{0·337}{0·080} = 4·21$ times for an increment of 3,000 − 65 = 2,935°. Experiments conducted in the

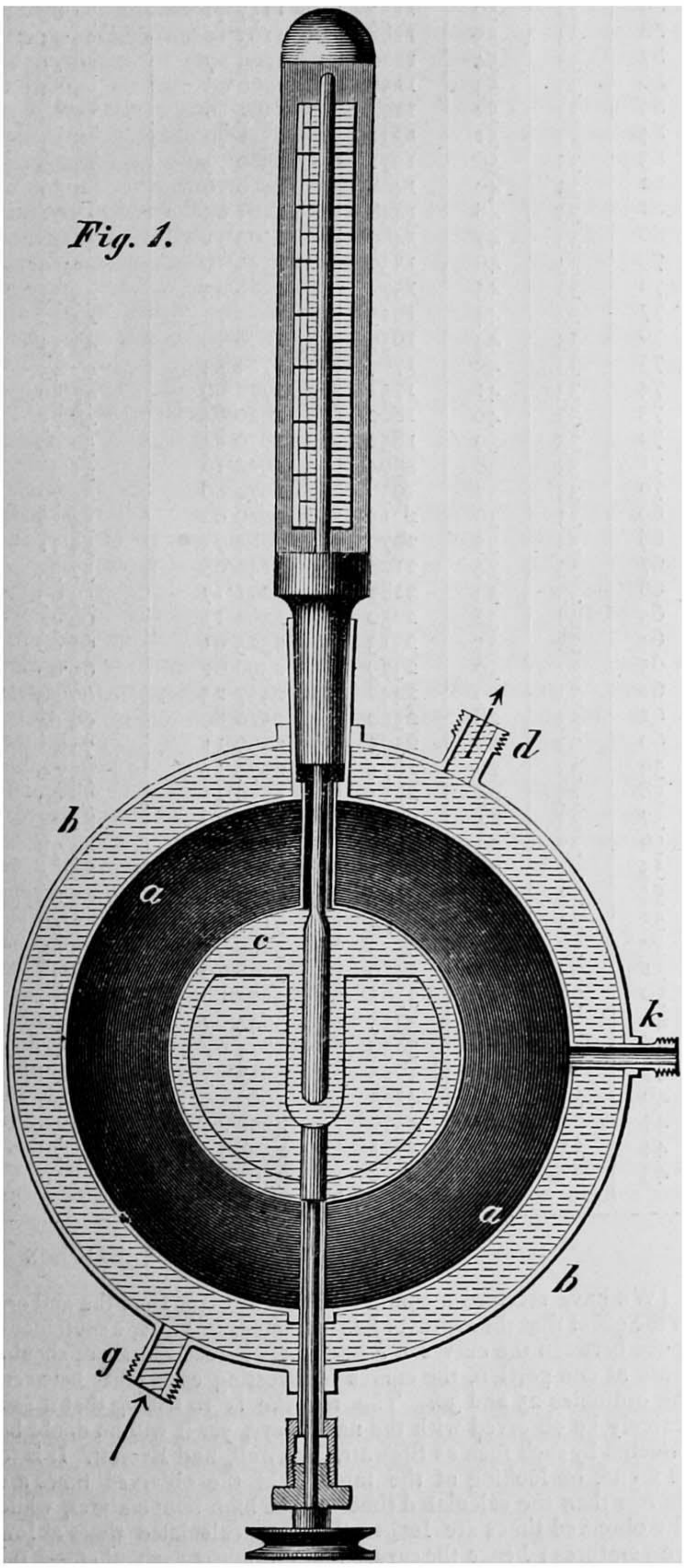


Fig. 1.

mean time show that the radiant power of one square foot of cast iron maintained at a differential temperature of 1,800° is 335 units per minute, hence that the emissive power at this stage of incandescence amounts to $\frac{335}{1,800} = 0·186$ unit for each degree of differential temperature. Our investigations have thus proved that at 65° the emissive power is 0·080 thermal unit, at 1,800° 0·186 unit, and at 3,000° 0·337 for each degree of differential temperature. We have accordingly established the fact that the

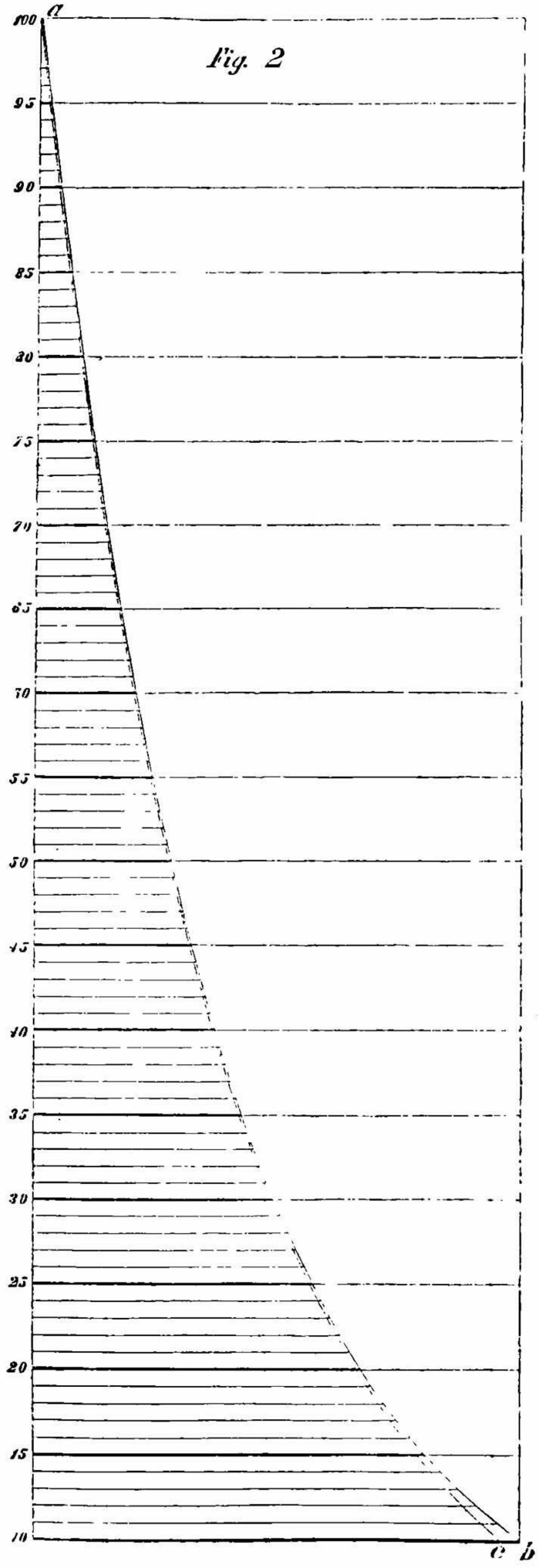


Fig. 2

emissive power increases nearly in the same ratio as the intensities, being fully quadrupled between the differential temperature of 65° and 3,000°. Let us be careful not to confound this increase of emissive power with the increase of radiant energy resulting from mere augmentation of temperature. It is, no doubt, owing to the change of the molecular constitution of the body during heating that the dynamic energy developed at a differential temperature of 3,000° is 4·21 times greater than it should be in accordance with the Newtonian law—a trifling increase, however, compared with that resulting from adopting the computations of Dulong and Petit, whose formula shows that for the stated range of temperature the ratio of radiant energy will be increased more than 4,000 times. It would be premature to attempt to explain the cause of the change of the radiant properties of metals at different temperatures disclosed by our experiments, until further investigations shall have established the exact relation between the actual and theoretical energy developed. Considering the difference of molecular motion within metallic bodies at white heat in a state of fusion, and at the freezing point of water, we need not be surprised at the variation of emissive power observed during our experimental investigation. Nor are we justified, in view of this variation of emissive power, in questioning the correctness of Sir Isaac Newton's assumption that heated bodies of definite radiant properties develop mechanical energies proportional to their excess of temperature over the surrounding media.

Temperature of the radiating sphere.	Temperature of enclosure.	Differential Temperature.	Observed Time.	Corrected Time.	Increment of time for each degree.
Fah. °	Fah. °	Fah. °	Seconds.	Seconds.	Seconds.
133	33	100	19·5	19·50	19·50
132	33	99	39	39·19	19·69
131	33	98	58	59·08	19·89
130	33	97	77	79·18	20·10
129	33	96	97	99·49	20·31
128	33	95	117	120·01	20·52
127	33	94	138	140·75	20·74
126	33	93	158	161·71	20·96
125	33	92	179	182·90	21·19
124	33	91	200	204·32	21·42
123	33	90	222	225·98	21·66
122	33	89	244	247·89	21·91
121	33	88	266	270·04	22·16
120	33	87	288	292·45	22·41
119	33	86	311	315·12	22·67
118	33	85	333	338·06	22·94
117	33	84	356	361·27	23·21
116	33	83	379	384·76	23·49
115	33	82	402	408·54	23·78
114	33	81	425	432·61	24·07
113	33	80	448	456·98	24·37
112	33	79	471	481·66	24·68
111	33	78	495	506·66	25·00
110	33	77	520	531·99	25·33
109	33	76	545	557·65	25·66
108	33	75	571	583·65	26·00
107	33	74	597	610·00	26·35
106	33	73	624	636·71	26·71
105	33	72	651	663·79	27·08
104	33	71	679	691·25	27·46
103	33	70	708	719·10	27·85
102	33	69	737	747·36	28·26
101	33	68	766	776·03	28·67
100	33	67	796	805·13	29·10
99	33	66	827	834·67	29·54
98	33	65	858	864·67	30·00
97	33	64	889	895·14	30·47
96	33	63	920	926·09	30·95
95	33	62	953	957·54	31·45
94	33	61	985	989·51	31·97
93	33	60	1017	1022·01	32·50
92	33	59	1050	1055·66	33·05
91	33	58	1084	1088·68	33·62
90	33	57	1118	1122·89	34·21

Temperature of the radiating sphere.	Temperature of enclosure.	Differential Temperature.	Observed Time.	Corrected Time.	Increment of time for each degree.
Fah. °	Fah. °	Fah. °	Seconds.	Seconds.	Seconds.
89	33	56	1152	1157·71	34·82
88	33	55	1188	1193·16	35·45
87	33	54	1220	1229·27	36·11
86	33	53	1257	1266·06	36·79
85	33	52	1294	1303·56	37·50
84	33	51	1331	1341·79	38·23
83	33	50	1371	1380·79	39·00
82	33	49	1408	1420·59	39·80
81	33	48	1448	1461·22	40·63
80	33	47	1489	1502·71	41·49
79	33	46	1530	1545·10	42·39
78	33	45	1572	1588·43	43·33
77	33	44	1615	1632·75	44·32
76	33	43	1659	1678·10	45·35
75	33	42	1704	1724·53	46·43
74	33	41	1751	1772·09	47·56
73	33	40	1802	1820·84	48·75
72	33	39	1852	1870·84	50·00
71	33	38	1904	1922·16	51·32
70	33	37	1958	1974·86	52·70
69	33	36	2015	2029·02	54·16
68	33	35	2070	2084·73	55·71
67	33	34	2128	2142·08	57·35
66	33	33	2188	2201·17	59·09
65	33	32	2250	2262·11	60·94
64	33	31	2313	2325·01	62·90
63	33	30	2379	2390·01	65·00
62	33	29	2448	2457·25	67·24
61	33	28	2520	2526·89	69·64
60	33	27	2595	2599·11	72·22
59	33	26	2674	2674·11	75·00
58	33	25	2754	2752·11	78·00
57	33	24	2839	2833·36	81·25
56	33	23	2929	2918·14	84·78
55	33	22	3025	3006·78	88·64
54	33	21	3126	3099·64	92·86
53	33	20	3232	3197·14	97·50
52	33	19	3343	3299·77	102·63
51	33	18	3459	3408·10	108·33
50	33	17	3581	3522·80	114·70
49	33	16	3715	3644·67	121·87
48	33	15	3859	3774·67	130·00
47	33	14	4015	3913·95	139·28
46	33	13	4185	4063·95	150·00
45	33	12	4370	4226·45	162·50
44	33	11	4571	4398·73	177·28
43	33	10	4792	4593·73	195·00

J. ERICSSON

[WE have received a communication by cable from the author, to the effect that the engraver has indicated, in Fig. 2, a continuous space between the curves *a b* and *a c*, whereas the space should cease at one portion, the curves intersecting each other between the ordinates 25 and 30. The mistake is so trifling that it can scarcely be observed with the naked eye, yet it will no doubt be detected by such men as Stewart, Maxwell, and Everett. It is to be found, on looking at the table, that the observed times are shorter than the calculated times at the high temperatures, while the observed times are longer than the calculated times at low temperatures; hence the curves must intersect each other.—ED.]

NOTES

THE *Pall Mall Gazette* states that the Earl of Portsmouth, who is the collateral representative of Sir Isaac Newton, has offered to the University of Cambridge, through the Duke of Devonshire (Chancellor of the University), all the papers of Sir

Isaac relating to scientific subjects which his lordship has inherited. Lord Portsmouth's gift is prompted by the feeling that these papers will be more fitly deposited in the library of the University of which Sir Isaac was so distinguished an ornament than in his own muniment-room.

THE visitation of the Royal Observatory took place on Saturday last. We have not yet received the Astronomer Royal's report; but there is a rumour that it will be proposed to the Government by the Board of Visitors that photographic and spectroscopic observations of the sun be added to the routine work of the Observatory.

IT has been decided that the first Meeting of the French Association for the Advancement of Science shall be held at Bordeaux in the autumn of the present year.

THE Prince of Wales will open the Bethnal Green Museum on Monday, the 24th inst. The Princess of Wales will accompany him. We should be glad to hear of free Scientific Lectures in connection with this Museum.

WE are glad to learn that the Royal Geographical Society are taking steps to press upon Government the importance of an expedition to the North Pole by way of Smith's Sound.

IN another column will be found a correspondence between the Chancellor of the Exchequer and Profs. Roscoe and Balfour Stewart with reference to the remarks made by the former at the presentation to degrees at the University of London, to which we drew attention a fortnight ago. It is with great satisfaction that we note Mr. Lowe's own version of his speech, on which we may say a word, although the accuracy of Mr. Lowe's defence has been called in question by Prof. Sylvester. If he and the Government which he represents remain firm to their avowed declaration in favour of the endowment of the higher professorships, the cause of scientific education will have reason to be grateful, the Professor being distinguished from the teacher by the presence of *investigation*, as well as of inculcation. The *Pall Mall Gazette* called attention in a forcible manner last week to the fallacies of the line of argument which Mr. Lowe was represented to have employed. To encourage a plethora of scholarships at the expense of the endowment of professorships would be a more perfect illustration of the maxim "How not to do it," than we have witnessed this long time.

THE proposed alterations in the examinations held during the first two years of a student's course in the University of Cambridge were submitted on May 30 to the Senate with varying success. The vote was taken on the principle, and not on the details, of the various propositions. The most important of those accepted were—to receive a knowledge of French and German as a substitute for Greek in the first or "Previous" Examination; and to add Heat as a subject in the "General" or Second Examination for the Ordinary Degree. The effect of these changes, when finally ratified, will be that a degree in Honours, in either Mathematics, Law, Natural or Moral Science may be obtained by a student who does not know Greek; but that an ordinary "Poll" or Pass Degree cannot be obtained without a knowledge of Greek. Also, a candidate for a degree in Honours will not be required to study (necessarily) any branch of Physical Science; but a candidate for a "Poll" Degree must have an elementary knowledge of Heat. As, however, several persons, in the debate which preceded the voting upon these questions, expressed themselves favourable to making some branch of Natural or Physical Science a part of the Previous Examination, it is, we conceive, not improbable that the Syndicate will embody some proposition of this kind in their amended scheme.

THE following arrangements are now made for the forty-second meeting of the British Association for the Advancement of Science, to be held at Brighton, and to commence Wednesday, August 14, under the direction of the following officers:—President—Dr. William B. Carpenter, F.R.S. Vice-Presidents—The Earl of Chichester, the Duke of Norfolk, the Duke of Richmond, K.G., the Duke of Devonshire, K.G., F.R.S., Sir John Lubbock, Bart., M.P., F.R.S., Dr. Sharpey, LL.D., Sec. R.S., Mr. Joseph Prestwich, F.R.S. General Secretaries—Dr. Thomas Thomson, F.R.S., Capt. Douglas Galton, C.B., F.R.S. Assistant General Secretary—Mr. George Griffith. Local Secretaries for the Meeting at Brighton—Mr. Charles Carpenter, Rev. Dr. Griffith, Mr. Henry Willett, the Pavilion, Brighton. Local Treasurer for the Meeting at Brighton—Mr. William H. Hallett, F.L.S. General Treasurer—Mr. William Spottiswoode, F.R.S. The General Committee will meet on Wednesday, August 14, at 1 P.M., for the election of Sectional Officers, and the despatch of business usually brought before that body. On this occasion there will be presented the Report of the Council, embodying their proceedings during the past year. The General Committee will meet again on Monday, August 19, at 3 P.M., for the purpose of appointing Officers for 1873, and of deciding on the place of meeting in 1874. The concluding meeting of this Committee will be held on Wednesday, August 21, at 1 P.M., when the Report of the Committee of Recommendations will be received. The first general meeting will be held on Wednesday, August 14, at 8 P.M., when the President will deliver an address; the concluding meeting on Wednesday, August 21, at 2·30 P.M., when the Association will be adjourned to its next place of meeting. The different Sections will assemble in the rooms appointed for them, for the reading and discussion of Reports and other communications, on Thursday, August 15, Friday, August 16, Saturday, August 17, Monday, August 19, and Tuesday, August 20, at 11 A.M. precisely. Authors are reminded that, under an arrangement dated from 1871, the acceptance of Memoirs, and the days on which they are to be read, are now, as far as possible, determined by Organising Committees for the several Sections before the beginning of the meeting.

PROFESSOR RUTHERFORD, of King's College, London, has been appointed Fullerian Professor of Physiology to the Royal Institution, in the place of Dr. M. Foster.

THE *British Medical Journal* gives currency to a report that a baronetcy is about to be conferred on Dr. William Stokes, Regius Professor of Physic in Trinity College, Dublin, and Physician to the Queen in Ireland.

WE have to record the death of one of our veteran botanists, Dr. Robert Wight, F.R.S., who died May 26, at his residence, Grazeley Lodge, near Reading, at the age of 76. Dr. Wight was a native of East Lothian, and very early in life entered the medical service of the East India Company, andwhile in this employment devoted his energies to the investigation of the then almost unknown flora of the British possessions in India. In 1834 he published, in conjunction with the late Prof. Arnott, the first volume of the "Prodromus Floræ Indiæ Orientalis," a work which was never continued. Further contributions to Indian botany were contained in his "Illustrations of Indian Botany," "Icones Plantarum Indiæ Orientalis," and "Spicilegium Neilgherrense," and in innumerable contributions to magazines and to the proceedings of societies. His name will also always be associated with his exertions towards the introduction of the cultivation of cotton into India. Dr. Wight was one of a band of botanists, to which Sir W. Hooker, Lindley, and Arnott belonged, who have now almost entirely passed away.

MR. JOHN B. LAWES, of Rothamsted, Herts, has announced his intention of placing in trust his laboratory and experimental fields with the sum of 100,000*l.*, the interest of which, after his death, is to be applied to the continuation of the investigations which have been carried on for so many years at Rothamsted. It is seldom that we have to record an act of so great munificence directed in a channel calculated to bring about such important results to the scientific department of agriculture.

PROF. WATSON, the indefatigable planet hunter of the Ann Arbor Observatory, reports the discovery, on May 12, 1872, of a new asteroid (No. 121) of the eleventh magnitude. The following observations are communicated by him to *Harper's Weekly* :—

Ann Arbor.	Mean Time.	Right Ascension.	Declination.
May 12	$14^h\ 13^m\ 42^s$	$16^h\ 20^m\ 37{\cdot}58^s$	$-18°\ 53'\ 9{\cdot}4''$
,, 13	$11^h\ 13^m\ 22^s$	$16^h\ 19^m\ 59{\cdot}35^s$	$-18°\ 52'\ 46{\cdot}2''$

Daily motion in right ascension, -43^s; in declination, $+26''$.

PROF. FLOWER'S lectures on the Comparative Anatomy of the Organs of Digestion of the Mammalia lately delivered at the Royal College of Surgeons of England are being published with illustrations in the *Medical Times and Gazette*.

THE last Swiney lecture of the present year will be delivered on Saturday next, when the general subject of Science in relation to Education will be considered. This will terminate Dr. Cobbold's tenure of the Chair, which is only open to medical graduates of the Edinburgh University. The collective attendances for this series of sixty discourses will, we understand, have registered a total of upwards of 15,000, a result which is gratifying to the friends of popular scientific instruction.

MR. CARRUTHERS has printed his Official Report for 1871 of the Department of Botany in the British Museum. In consequence of the extent of the recent additions to the general herbarium, additional cabinets have had to be incorporated, and the old ones rearranged. The exhibition rooms have also been rearranged, with a view of making them more instructive and attractive to visitors. Several natural orders in the General and British Herbaria have been rearranged. Among the more important additions to the General Herbarium are 17,000 species, chiefly from Central Europe, Alsace, the Jura, the Lower Rhine, Spain, Mexico, and Labrador, being the herbarium of Auerswald, of Leipzig; 1,000 from Yucatan, collected by Dr. A. Schott; upwards of 1,000 from Russia; and upwards of 1,500 from Scandinavia, collected by Ahlberg.

IN a letter from General Otto Struve, director of the Palkowa Observatory, and Astronomer Royal of Russia, to Prof. Newcomb, of the Washington Observatory, detailing the Russian preparations for observing the forthcoming transit of Venus, and printed in *Harper's Weekly*, he remarks that the inquiries into the meteorological conditions of the stations selected have given, on the whole, very satisfactory results, particularly for the station on the coast of the Pacific Ocean and in Eastern Siberia (85 per cent. of clear sky for December). In two only of the stations chosen, Taschkent and Astrabad, these conditions are not sufficiently satisfactory. For this reason the observers designed for Taschkent will probably go to a place about 100 miles west of that town; and instead of Astrabad it is proposed to take either the island of Aschuradeh, in the Caspian Sea, or, if possible, to cross the Elburz Mountains, and establish observers at Schahrech, in Persia (with nearly absolute certainty of clear sky). The total number of Russian stations will be twenty-four, each of them provided with only one instrument for the transit observation. These instruments are—three 4-inch heliometers, three photo-heliographs, four 6-inch equatorials, and four 4-inch equatorials, provided with filar micrometers and spectroscopic apparatus, and ten 4-inch telescopes, designed merely for contact observations. Each station will also be furnished with clocks, chronometers, and the instruments necessary for exact determination of time. The principal instruments have already been ordered. Most of them will be ready for use in the course of the present or beginning of next year. For these instruments the observers are also in a great part already selected. They will all visit Palkowa for a certain time in 1873 to exercise themselves in the observations. The geographical positions of the stations will not be determined by the transit observers; but all stations on which the transit has been successfully observed will be carefully determined afterwards by special expeditions of the general staff or the navy. For this purpose a principal line of telegraphic longitudes will probably be laid next year through all Siberia to Nicolajevsk, with which line the other stations of that part of Russia can easily be joined, either by telegraphic or chronometric operations. With regard to photographic observations, Prof. Struve states that two observers, one at Vilna, and Dr. Vogel at Bothkamp, in Holstein, have been perfectly successful in taking instantaneous observations with dry plates.

THE Cambridge Natural Science Club has just issued its first terminal report. The club was founded March 11, 1872, by some of the undergraduates and B.A. members of the university for (Rule 1) "The promotion of natural science by means of friendly intercourse and mutual instruction." The number of members was at first limited to nine, but at the third meeting the number increased to twelve. The number is limited in order that the meetings may be held in the rooms of the members. Meetings are held every Saturday evening during term, and during eight weeks of the long vacation, in the rooms of the members in rotation. The member in whose rooms the meeting is held is president for the evening, and (Rule 7) "brings some subject of scientific interest under the consideration of the members in the form of a short paper, with such practical illustrations as the subject admits of." The papers read during this term have been the following:—By Mr. J. C. Saunders, B.A. (Downing College), "On Conspicuous Movements in Plants;" Mr. C. T. Whitwell, B.A., B.Sc. (London), Trinity, "On Isothermals;" Mr. C. J. F. Yule (St. John's), "On the Anatomy of Pyrosoma;" Mr. H. M. Martin, M.B., B.Sc. (London) Christ's, "On the Modes of Reproduction of Animals and Plants;" Mr. A. Liversedge (Christ's) "On Supersaturated Saline Solutions;" Mr. J. E. H. Gordon (Caius), "On Submarine Telegraphy;" Mr. P. H. Carpenter (Trinity), "On the History of the Abbeville Jaw."

A BOTANICAL and geological excursion class has been commenced in connection with the St. Thomas's Church Schools Birmingham. President, Rev. T. D. Halstead, rector; secretary, Mr. Miller (brother of the late celebrated chemist); conductor, Mr. John Turner, F.M.S. The class consists of twenty-five members, most of whom have attended science classes during the winter, and recently competed in the examinations of the Science and Art Department. The class meets every Saturday afternoon.

WE learn from the *British Medical Journal* that a committee, consisting of Profs. Bamberger, Billroth, Brücke, Duchek, and Schroff, has been appointed to select candidates to be invited to fill the chair of Pathological Chemistry at the University of Vienna. The names of Hoppe-Seyler (lately appointed to the University of Strasburg), Kühne (of Heidelberg), and Liebreich (of Berlin), are mentioned in connection with the post. The establishment of a professorship of Hygiene in the University is under discussion; and there appears to be a general agreement among the professors in the University as to the necessity of having such a chair instituted with as little delay as possible.

A FAIRLY perfect human skeleton has been discovered by Dr. Revière in the caverns of Baoussé-Roussé, near Menton, on March 26, and a short paper on this interesting find was read to the Academy of Sciences at a recent sitting.

MR. BENTHAM'S ANNIVERSARY ADDRESS TO THE LINNEAN SOCIETY*

As a general summary of the current zoological literature the "Zoological Record" maintains its high value. The volume for 1870 has lately appeared under the new editorship of Mr. Newton, and the arrangements now made for its further prosecution are very hopeful; yet I must again urge upon all our Fellows, who, as amateur zoologists or patrons of the science, have joined our ranks, to give their further support to the "Zoological Record" Association in order to secure the continuance of this annual summary for the sake of the working members, to whom it is so essential. I would also call attention to the sketch of the ornithological works recently published or in progress contained in the last number of the *Ibis*, an example which it were to be wished were regularly followed in all periodicals specially devoted to any branch of our sciences. The Reports on the contributions to the various branches of zoology inserted in Wiegmann's "Archiv" under the editorship of, and some of them compiled by, Troschel, replace in some measure the "Zoological Record" for the German public, and are kept up nearly to the same period, some of the reports for 1870 having already appeared; they are also much to be commended, although they may not have quite the method and completeness of the "Zoological Record." I have further to congratulate science in general on the near completion of the Royal Society's great Catalogue of Scientific Papers, the sixth and last volume of which is far advanced, and likely to be in our hands by the commencement of the next session of the Society.

In Botany, Pritzel's excellent aud much-improved second edition of his "Thesaurus" is rapidly going through the press, and brings the repertory of separate botanical works down to the year 1871. Current botanical publications are also generally noticed in various botanical publications, especially the "Giornale Botanico Italiano," edited by Prof. Caruel; the "Flora" of Ratisbon; the "Botanische Zeitung," continued since the death of v. Mohl by A. de Bary; the "Bulletin de la Société Botanique de France," which comprises perhaps the fullest bibliographical review; and the *Journal of Botany*, which promises well under the new and active editorship of Dr. Trimen. But, with the exception of lichenography, the bibliography of which is brought down to the year 1870 in Kreppelhuber's detailed "History and Literature of Lichenology," we have no comprehensive references to Memoirs and Papers published since 1863, the term of the Royal Society's Catalogue, and we feel much the want of an annual summary corresponding to the "Zoological Record."

A work has recently appeared which has naturally attracted much of my attention as being intimately connected with a branch of the science which I have on several occasions taken as the subject of my annual Addresses, and as being the result of long and careful study of the great and varied mass of data collected by its laborious and distinguished author. I speak of Grisebach's "Vegetation of the Earth according to its climatological distribution." The general scope and plan of the work has been recently noticed in an article in NATURE,† and I shall on the present occasion confine myself to a few observations on his views with reference to some of those regions or districts to which I had intended to call your attention in my last year's Address.

One of the most interesting of these regions is the Japanese, or the greater part of Grisebach's Chino-Japanese regions, that is, the Japanese islands and opposite coasts of the Asiatic continent. The peculiarities of its flora have been accounted for, upon considerations depending chiefly on origin, in a well-known paper by Asa Gray (Mem. Amer. Acad. new ser. vol. vi. p. 424), whose views are fully coincided in by Maximowicz and others, but strongly objected to formerly by Miquel and now by Grisebach, who relies upon climatological and other physical considerations. It appears to me that this is a strong instance of the combined effects of the two agents as explained in my abovementioned Address of 1869 (p. 15; Proc. Linn. Soc. 1868-69, p. lxxvii.) The main features of this flora are the mutual intergrafting of northern and tropical types, and the number of highly differentiated endemic or widely dissevered monotypic or almost monotypic races;-the former due to physical, the latter to derivative causes.

With regard to the endemic or widely dissevered highly differentiated races (monotypic genera, sections, or very distinct species), Grisebach's views differ widely from those of Asa Gray and other modern naturalists who adopt more or less the theory of evolution. Grisebach, as already observed, entirely ignores community of origin of closely allied or representative species, and is but little disposed to take into consideration ancient dispersion under geological conditions different from the present ones. Each species he believes has arisen—he had formerly said been created, an expression he now abandons in order not to be supposed to prejudge a question which admits of no positive solution—each species has arisen in a particular spot (from what materials he thinks it vain to inquire), under the influence of physical and other external conditions, and has spread more or less in every direction from this birthplace or centre as far as those external conditions have prevailed, and in so far as their progress has been unopposed by insurmountable physical or climatological barriers. In conformity with these views he explains closely allied and representative species in a passage which I give at length for fear of misrepresenting him by an abstract. "The birthplace (*Entstehungsort*) of a plant species," he says, vol. i. p. 515, "may be taken as the most perfect expression of the concordance between the physical life-conditions of the place and the organisation of the plant; for this suitability to given influences of inorganic nature gives the highest measure of the capability of preservation which life strives to attain. Upon these propositions is founded the conclusion, that the nearer the centres of different plants are placed geographically, and the less different are therefore their climatological conditions, the more similar must be their organisation, or, what amounts to the same thing, the more species will have arisen in the same genus. This phenomenon is exhibited in all places where we can compare endemic species whose dispersion is limited, but in islands which have a peculiar vegetation it is less pronounced than in continents. From any one point climate is gradually altered, like the radii of a circle which gradually diverge more and more from each other from the centre to the circumference. In a continent the whole area of the circle may be supposed to be suited to the production of changes in organisation; in an archipelago it is interrupted by the sea, and here, therefore, few similar species have arisen. Another consideration to be taken into account is that genera when compared with each other are unequally susceptible of change (*veränderungsfähig*); their species, therefore, to keep to the same metaphor, will be found arranged at greater or less distances from each other in the radii of the circle. If the area of the continuous land is small, monotypes will have more readily arisen—genera which, on the one hand, are very little or not at all susceptible of change, and on the other hand, can no longer subsist with a certain degree of climatological change. If in a more remote geographical distance the more important climatological conditions which these genera require are repeated, we may, perhaps, find in another part of the globe a second species; and this generally explains the orign of the species which have been termed representative (*vikariirende Arten*). A precisely similar climate, however (exactly the same complication of the very varied phenomenon towards which organisms bear themselves receptively), is never repeated in two distant points of the earth's surface; and this may be taken as the foundation of the absolute unity of centres of vegetation, that is to say, of the proposition that every species in its wanderings has issued from a single birthplace, which does not exclude the possibility of solitary exceptions which might be imagined in plants of less receptivity."

In all this it appears to me that if the writer refuses to admit of a descent from a common parent, we have a right to ask of him what is the previous organisation upon which he imagines climate to have worked to produce allied species in one region and representative species in distant regions? what are the previous genera which have changed? for upon that seems to hinge the whole of his argument in refutation of Asa Gray's hypothesis explanatory of the original connection between the East Asiatic and East American floras. That every species had arisen in one spot, whether by differentiation or by creation, appears now to be tacitly admitted by all. Asa Gray, in accordance with Darwinian theories, supposes widely spread species to have been, under the different conditions of distant lands, gradually modified in different directions, so as to have produced distinct varieties or representative species; Grisebach supposes these different conditions to have independently produced distinct but similar species, by acting on organisms which had not been one and the same species; but what else they may have been he seems to think beyond the reach of plausible conjecture.

* Delivered Friday, May 24, and abridged.

† No. 128, April 11, 1872.

Leaving, however, these questions of origin aside, he strongly objects to the classing representative with identical species in considering geographical distribution; for the former appear in such absolutely dissevered distant regions that an interchange of species, even in early geological periods, seems impossible, as, for instance, in the case of several Ericas of the Cape and of Europe. It is on the contrary, he believes, almost always possible to deduce the actual progress of identical species from the form or physical accidents of their homes and from the means of dispersion at their command (p. 519). He therefore, in combating Asa Gray's conclusion, commences by eliminating from his calculations, after the example of Miquel ("Over de Verwantschap der Flora van Japan met Azië en Noord America, Versl. K. Akad." Amsterdam, ser. 2, ii.), all representative species, thus reducing Asa Gray's list of concordant races in Japan and Eastern North America from 226 to 81, from these Grisebach subtracts 41, which are also inhabitants of Western North America, and can still, he thinks, daily transmit their seeds across the Pacific Ocean; 17 more are, in his opinion, supported by that of other botanists, either certainly not identical or doubtful, and to be added to the already eliminated representative species. Of the remaining 23, he finds 21 which can bear a high northern climate and may yet be found in the Oregon or other imperfectly explored territories of North-West America; and the whole long list is thus reduced to two species only, whose problematical disseverance in Japan and Eastern North America remains unexplained—the one, *Elodea petiolata*, being a marsh plant, which as such possesses great migratory powers, the other, *Carex rostrata*, from the White Mountains, awaits further researches on its geographical distribution. Even admitting the possibility of the greater early dispersion of these species in former geological periods propounded by Asa Gray, Grisebach thinks that any such great antiquity of the Japanese flora is not established on so firm a ground as to supersede any attempts at finding other explanations limited to the results of forces still in activity in present times, and that accordingly the distribution of the species in question may be satisfactorily accounted for by the means of dispersion still available, if the data are viewed in the light he has placed them. I should doubt, however, whether his mode of cutting up a long array of ascertained facts further increased by subsequent researches, in order to make them agree with preconceived theories, will carry any stronger conviction into Asa Gray's mind than in my own, more especially as the presumed great antiquity of the Japanese flora is not deduced from these facts alone, but is derived also from other evidences, amongst which the peculiar character of the endemic monotypes bears a prominent part.

With regard to Grisebach's idea that representative and similar species are independently produced by similarity of climatological conditions, and that they afford no conclusive evidence of community of origin, for that they are to be found in widely dissevered localities between which it is impossible to conceive any continuity even in ancient geological periods, and with reference to the instance he adduces of the above-mentioned heaths of the Cape and of Western Europe, I would recall to your minds some remarks I made in my Address of 1869 (p. 25; "Proceedings," p. lxxxvii.) on the remarkable coincidence of several genera, and the near similarity of some species that exists between these two widely dissevered regions. I would now add that if it is difficult to imagine any ancient continuity which should readily explain this phenomenon, it seems equally difficult to account for it by any climatological similarity, if we consider how much Cape plants in general, accustomed to a prolonged summer's sun, suffer from its want in the dull damp seasons of Western Europe.

The Eastern Archipelago, the study of whose fauna, as connected with the history of the great changes it has undergone by successive submersions and upheavals, has been rendered so interesting by the well-known labours of Mr. A. R. Wallace, calls imperatively on the attention of botanists in the search of facts derived from its flora in confirmation or refutation of these views. Unfortunately we are in this respect very much in arrear. The botany of New Guinea is almost wholly unknown; and from Celebes we have but very little. Sumatra, Java, the Philippines, Timor, and a part of Borneo, have been more generally explored, and large collections of their plants have been deposited, chiefly in the Leyden Herbarium, but also in considerable numbers in those of Kew and in some others; but even these materials have been but little worked up in a manner to be available for the geographical botanist. The two eminent Dutch botanists who had successively charge of the Leyden collections contributed much in various ways to the progress of the science and especially to our knowledge of the flora of the principal Dutch islands, but without leaving any satisfactory general view of all that was known on that of the whole archipelago. Blume's "Bijdragen tot de Flora van Nederlandsch Indië," drawn and published at Batavia when he was still very young, was a wonderful work considering the means at his disposal; and after his return to Europe he commenced elucidating with equal ability and in greater detail several orders connected with that flora ("Flora Javæ," "Rumphia," "Museum Lugduno-Batavense"); but as general works all these remained incomplete. Miquel drew up a "Flora Indiæ Bataviæ," purposing to be complete as far as his materials allowed; but it was far too hastily compiled, without the necessary critical examination of genera and species. Since his lamented death I have seen no signs of any Dutch successor likely to take up the study of the botany of the archipelago in any scientific point of view. In the meantime the rich stores collected by P. Beccari in Sarawak are, I am informed, in the course of distribution; and that enterprising Italian naturalist has returned to the East with a view to the exploration of New Guinea and some others of the less known islands.

Grisebach, in his Indian Monsoon region, unites the archipelago with the East Indian peninsulas and continent to the foot of the Himalayas, the Island of Ceylon to the west, and the Society and the Marquesas and other coral islands to the east, embracing, as it were, the whole of Tropical Asia or Sclater's Indian, with a portion of his Australian Palæotropical regions; and certainly a cursory survey of the vegetation of this vast expanse of territory would appear to justify Grisebach's idea of its unity of character. It has also tolerably definite limits determined on the north-west by the drier rocky East Mediterranean or Persian region, on the north by the great Himalayan chain, and on the east and south by a wide extent of ocean; the exceptions being chiefly the above-mentioned innoculation, as it were, into the Japanese flora to the north-east, and more or less of an intrusion across the ocean to the westward into Tropical Africa, and over a narrower interval of sea to the south-east into north-east Australia. The principal cause of this uniformity of character, so far as it goes, is well deduced by Grisebach from climatological and physical conditions, his observations on the chief portion of the region, or East India proper, from Ceylon and the Peninsula to Malacca, being mainly derived from Hooker and Thomson's most instructive introduction to their "Flora Indica," which, from a variety of causes, was unfortunately put a stop to after the issue of the first volume. It is now being replaced by the "Flora of British India," under Dr. Hooker's editorship, of which the first part, just published in a more concise form, gives a confident hope that it may be steadily and rapidly brought to a conclusion. We shall then have ample means of instituting a comparison of the Indian vegetation with that of Boissier's "Flora Orientalis" to the north-west, of Ledebour's "Flora Rossica" to the north, of Miquel's almost as complete, though less methodical enumerations of Japanese plants to the north-east, of the "Flora Australiensis" to the south, and of Oliver's "Tropical African Flora" to the west.

The "Flora Indica" does not, however, extend to the eastern portion of Grisebach's Monsoon region, about which our information is so deficient; but where, as he observes, "the distribution of organisms involves one of the most remarkable problems in the darker regions of vegetation-centres." He further remarks that the flora of this eastern region, with the exception of the Timor group, is everywhere Indian, and regulated by climatological conditions, the vegetation of New Guinea being, as he rather hastily supposes, "thoroughly similar to that of Borneo," a result quite at variance with the distribution of animals as expounded by Wallace. As a possible explanation of this discrepancy, he proposes an hypothesis which, for fear of misrepresentation, I shall give at length:—"Thus the limits of particular forms of plants and of animals in the Indian Archipelago do not concur. Vegetation corresponds to climatological, the fauna to local (*räumliche*) analogies. This opens a wide field for speculation on the history of the globe. By a mere sinking of the land to an unimportant extent, Darwinism readily explains the origin of the fauna of these islands, but not the Indian character of the flora of New Guinea, which presupposes much greater upheavals than the origin of fauna, calculated to give rise to equatorial rainy seasons. This hypothesis would derive the endemic marsupials of New Guinea from the Australian ones after the establishment of the Torres Straits, but it gives no explanation of the way in which the peculiar palms of New Guinea could have arisen from allied Indian genera. With more plausibility, although with little more foundation on ascertained facts, may be put forward another conjecture derived from the respective relations of plants and animals to the outer world. From their organisation the former are much more dependent on climate, the latter on the vegetation which serves them for food. If an extent of sea is converted into land, its climate (independently of its geographical position) will depend on the form of its coasts and on the relief of its surface. If, now, creative forces are pronounced, the forms of vegetation will be suited to the climate. These forms correspond to the climate of the present day, as everywhere else, so also from the Malayan continent to the South-Sea Islands. If we assume that in an earlier geological period the eastern portion of the Archipelago did not yet possess its mountains, and was connected with Australia, so might the Australian climate have then extended to the Archipelago; but with the change in the climate the vegetation of the time must have disappeared. A new flora arose; but in the fauna, which was less dependent on climate, the earlier types may have longer persisted. Perhaps the present period may be regarded as one in which the Australian forms of animals are in an expiring state, because the jungle-forests do not sufficiently correspond to their demands for food. It would appear as if creative activity only wakes up at specific points of time on specific points of the earth's surface, and that during the long pauses Nature's struggles are directed only to the retaining that which exists. Vegetation, as well as the animals which it feeds, must ever be considered in relation to the geological developments. During the time which has elapsed since the mountains and the moist climate of New Guinea have been established no new creation of Mammalia has taken place. Only very few Marsupials, and scarcely any other Mammalia, have been found on this great island. But in other classes of animals forms have arisen corresponding to the present vegetation, such as the Birds of Paradise, which are unknown in Australia, but which in New Guinea hover over the forest tree-tops, whilst they can take shelter from the mid-day sun under the dense foliage. . . . The present type of organisation was already cast in New Holland in the tertiary period, whilst the endemic plants and animals of New Guinea appear to be of much later origin" (vol. ii. pp. 69, 70).

Without admitting to its fullest extent the main fact relied upon, that there is no marked line separating the vegetation of the western and the eastern portions of the Archipelago corresponding to that laid down by Wallace for animals, a premature conclusion in the present state of our knowledge,* and still less entering into speculations as to the intermittent action of creative forces which I do not quite comprehend, we must agree with Grisebach that, so far as shown by the scanty data at our command, the uniformity is much greater in the botany than in the zoology of the whole Archipelago. We may also admit with him that this comparative uniformity may be, in great measure, due to the uniformity of climate acting more upon plants than upon animals. But there are other circumstances which may probably have favoured the continued action of natural selection through countless ages in procuring this result. Dr. Hooker has very plausibly suggested a greater geological antiquity in the plant races than in those of animals, especially the higher animals, under which the former, or the ancestors from which they are descended, had become established over a wide extent of continuous land before its successive disruption, upheavals, and depressions producing the present isolation. We must next take into account that this continuity of land need not be so great in the case of plants as of animals. The dispersion of the former is passive, and takes place chiefly in a dormant state, in which minuteness and enormous multiplication afford them opportunities for crossing seas and other barriers denied to the higher animals. Plant races of accommodating (*accomodationsfähiger*) constitutions, as they successively arose and attained the full vigour of specific life, will have early spread over any continuous or but little broken area, enjoying comparatively similar physical and climatological conditions, the western and eastern forms intermingling, so as that the one should only gradually be replaced by the other, thus in early ages repeating under the tropics the phenomenon now observed in the northern temperate Europeo-Asiatic region. These vigorous or accommodating races, whether new differentiations or foreign invasions, will at the same time have gradually expelled and replaced races which in tertiary or other previous periods had occupied the land under different conditions, and which now could only maintain themselves in the struggle for life in localities affording them, in their reduced or weakened state, special protection against the effects of the altered climate and the attacks of their vigorous competitors. Such localities, suited to ancient or expiring races of few individuals, with varied but always special requirements, and generally slow of propagation, may be exemplified in the Mediterranean, the Japanese, and other regions abounding, as Grisebach terms it, in centres of vegetation; they may be faintly traced in the Nilgherries and in Ceylon; but are in general very few in Grisebach's Monsoon region, and those few are as yet but little known or wholly unvisited. Kini-Balu, in Borneo, has, however, as we learn from Dr. Hooker, supplied a place of refuge for a certain number of Australian types, and it may be conjectured that many more may have maintained themselves in those lofty mountains of New Guinea which have as yet been only seen from a distance. Continuity of vegetation probably existed in tertiary times between Australia and a vast extent of land including more or less of both of Wallace's divisions of the Archipelago. How far subsequent changes which have influenced the present distribution of animals may have affected that of the forest vegetation, can only be judged of when the floras of Borneo, Celebes, and New Guinea shall have been as well investigated and compared as have been those of Sumatra and Java.

* Dr. Hooker has, for instance, remarked that no Dipterocarpeæ have been found to the east of Borneo.

(*To be continued*)

SCIENTIFIC SERIALS

In the *Journal of Botany* for May the editor, Dr. H. Trimen, describes a species of *Luzula* new to the Flora of Europe, *L. purpurea* Link, of which a drawing is given.—Mr. J. G. Baker concludes his revision of the Cape species of *Anthericum*.—Mr. Archer Briggs notices some peculiarities of the botany of the neighbourhood of Plymouth, principally with reference to species common elsewhere which are absent from the south-western extremity of our island.—Mr. O'Meara continues his researches on Diatomaceæ.

The number for the present month opens with a note on Dimorphism in *Eranthemum* by Mr. John Scott.—The other original articles are Notes on British Gentianaceæ by Mr. James Britten, and Supplementary Notes on the *Erysiphei* of the United States by Messrs. Cooke and Peck. Several valuable reprints, as well as many interesting short notes and queries, also appear in both these numbers.

The *Canadian Naturalist*, vol. vi. No. 3, is almost wholly devoted to Geology, commencing with a continuation of Principal Dawson's series of papers on the Post-pliocene Geology of Canada, the portion of the subject specially treated of in this number being the local details.—Prof. Sterry Hunt's "History of the names Cambrian and Silurian in Geology" has already been reprinted in our columns.—Mr. E. Billings contributes some "Remarks on the Taconic Controversy," in which he defends the views of Dr. Emmons with regard to the position of these rocks, and a note on the genus *Obolellina*.—The only new geological article is Prof. Smallwood's "Meteorological Results for Montreal for the year 1871.

SOCIETIES AND ACADEMIES

London

Royal Society, May 30.—"On the Structure and Function of the Rods of the Cochlea in Man and other Mammals." By Urban Pritchard, M.D.

The aim of this paper is to describe the true construction and use of the cochlea, so far as its task of distinguishing the various sounds is concerned. This cochlea, it must be borne in mind, consists of a spiral canal, in form and shape very similar to the inside of a snail-shell. From the axis of this spiral there proceeds horizontally a plate of bone, the *lamina spiralis*, almost dividing this canal into two. From this plate again there extend two membranes, the membrane of Russner, and the Lamina t spiralis membranacea, as far as the walls of the canal, thus separating it into three minor canals.

Between the layers of the membranous spiral lamina are situated the so-called rods of Corti. These were first discovered and described by the Marquis de Corti; and although since then many observers have studied the subject, yet scarcely two investigators are agreed as to their exact form.

In a general view of the rods from above, they appear similar to two rows of pianoforte-hammers, rather than like the keys of that instrument, to which they have been likened. In a lateral view, these two rows of rods are seen sloping towards each other, like the rafters of a gabled roof. The rods consist of a shaft and two enlarged extremities, but the two rows differ considerably in form; the inner rods are attached by their lower extremities to the membrana basilaris at its junction with the lower lip of the limbus, and just external to the spot where the nerve-filaments emerge. They are directed outwards and upwards, with a slight undulation to meet the outer rods. The lower extremity is enlarged and rounded, gradually tapering to the shaft, which is cylindrical; the upper extremity is somewhat cuboid in form, but the outer surface is deeply concave, and the upper lip of the concavity is prolonged into a process.

The outer rods are attached to the membrana basilaris by a broad base, which also gradually tapers to a cylindrical shaft. Their upper extremity is less cuboid in form, and presents a convex internal surface, which articulates with the corresponding concavity in the inner rods just mentioned; from the outer and upper part there extends outwards a slender process.

One of the most important features with regard to these rods is their relative length. Most authors state that there is very little difference in the length of the two rods; in this, however, they are much mistaken; for not only do the two sets of rods differ in this respect, but the length of each varies according to its position on the cochlea. Thus, at the base, the outer rods are as nearly as possible equal in length to the inner, but proceeding upwards, both rows increase in length with great regularity, although not in the same ratio, the outer increasing with much greater rapidity, so that near the apex they are twice the length of the inner.

It was generally supposed, *a priori*, that these rods were graduated so as to distinguish the most minute variation of tone, but no one until now has been able to demonstrate this.

The rods, therefore, vary in length from about $\frac{1}{500}$ to $\frac{1}{200}$ of an inch. The number of rods in each row is not the same, there being about three of the inner to two of the outer, and, according to calculation, there are about 5,200 inner rods and 3,500 outer in the whole cochlea.

Corti and most other authors considered this system of rods to be the essential portion of the cochlea; they supposed the rods received the vibrations conducted to them, and being set in motion, so affected the nerves as to cause the brain to appreciate the various sounds. Later German writers have attributed the appreciation of the various vibrations to certain delicate cells, which are attached to the under surface of the membrana reticularis. From this circumstance alone it appears very evident that these investigators had not suspected, much less discovered, the fact that the rods are most exquisitely graduated, for otherwise they could surely never have doubted that so beautiful and suitable an apparatus could have any other ostensible purpose than that of appreciating the various sounds. I consider indeed that the cochlea represents a musical instrument, similar in nature to a harp or musical box, the strings of the one and the tooth of the other represented by the rods of Corti. The spiral bony lamina is simply a sounding-board; around the rods are placed the various nerve-cells and nerve-fibres, and from these cells the impressions are conveyed by the fibres to the brain itself.

It is possible, therefore, to trace very completely the course of sounds or vibrations from a musical instrument or any other source to the brain, through the medium of the ear. First the vibrations are caught and collected by the auricle, and transmitted through the external meatus to the drum of the ear, next across the middle to the internal ear. Here the sound is appreciated, merely as a sound, by the vestibule; the direction is discovered by means of the semicircular canals; but to distinguish the note of the sound, it must pass on to the cochlea. The vibration therefore passes through the fluid of the cochlea and strikes the lamina spiralis, which intensifies and transmits the vibration to the system of rods. There is doubtless a rod not only for each tone or semitone, but even for much more minute sub-divisions of the same; so that every sound causes its own particular rod to vibrate, and this rod vibrating, causes the nerve-cells in connection with it, to send a nerve-current to the brain.

"Examination of the Gases occluded in Meteoric Iron from Augusta Co., Virginia." By J. W. Mallet, Ph.D., M.D.

The author stated that, whether or not his analysis be considered as furnishing presumptive evidence of the Virginia iron having come to our earth from a different atmosphere to that of which the Lenarto meteorite brought us a sample, the result differs so far from that of our sole previously recorded determination of the kind as to make it a matter of much interest that a larger number of meteoric irons from various localities should be subjected to careful examination in the same direction, thus supplementing our knowledge of the fixed constituents of these curious bodies by a study of their gaseous contents.

Anthropological Institute, June 3.—Sir John Lubbock, Bart., president, in the chair. "The artificial enlargement of the Ear-lobe in the East," by J. Park Harrison; "On Tumuli at Sapolia, Ardaschevo, Russia," by Baron de Boguschefsky; "On Ogham Pillar Stones in Ireland," by Hodder M. Westropp; and "The Westerly Drifting of Nomades from the 5th to the 19th century, Part 9: the Fins and some of their allies," by H. H. Howorth. The object of the paper by Mr. Howorth was, in the first place, to discriminate between the Fins and the Lapps, whose history, physical features, customs, and other idiosyncracies are entirely different. In the second place, to show that the Esthonians belong to the Fin rather than the Lapp stock. Then to adduce the evidence for making both Fins and Esths very recent emigrants into their respective modern habitats, and to trace them to their former country beyond the Dwina, where they were known to the Norsemen as Biarmians, and to the early Russian chroniclers as Sarvalokian Ichudes. Having divested Scandinavia and Esthonia of their Fin inhabitants, and having thrust them back into an area which was of great renown in the times of the Norsemen, we can explain how the civilisation, which the Kalevala and other evidence proves, was once peculiar to the Fins, has been lost, and also explain whence the Norsemen derived a great portion of the culture which distinguished them. The main position that was new in the paper was the deriving the Esthonians from the same area as the old Fins, and making them also to be recent emigrants, and not Autochthones, as they have been so frequently described; and the clearing up of the ethnography of the old province of Biarmia, which has hitherto been much confused.

Victoria Institute, June 3.—The Rev. Prebendary Irons read a paper on Prof. Tyndall's "Fragments of Science for Unscientific People." He first dealt with physical science and its rivalries, Dr. Irons holding that there was a want of thoroughness in Prof. Tyndall's appeal to facts. He then analysed the statements made as to the action of matter on matter, and considered Dr. Tyndall to be inconsistent in stating that science could not solve the problem of the Universe, and yet adding that we ought not to see the evidences of Divine pleasure or displeasure in the phenomena of the material world. Finally, Dr. Irons urged that science and true religion could not be supposed as opposed to each other, as some men of science would have us believe.

CAMBRIDGE

Philosophical Society, May 27.—"On some properties of Bernoulli's numbers, and, in particular, on Clausen's Theorem respecting the fractional parts of those numbers," by Prof. J. C. Adams. The author gave a comparatively simple proof of Clausen's theorem. Thirty-one of Bernoulli's numbers are already known; the author has calculated twenty-two additional numbers. He also had proved that if n were a prime number other than 2 or 3, the numerator of the n^{th} Bernoulli's number was divisible by n.—"On some of the Symptoms produced by Uræmic Poisoning in Chronic Disease of the Kidney," by Dr. Latham. These symptoms were explained by:—1. The impeded passage of the blood through the minute arteries of the system, caused by excessive contraction and hypertrophy of the muscular walls of these vessels, as has been demonstrated by Dr. George Johnson. 2. The hypertrophy of the heart, developed by the resistance offered to the circulation from the contraction of these small arteries. 3. The impoverished state of the blood, which is the necessary accompaniment of the disease.

CANTERBURY

East Kent Natural History Society, May 2.—"Remarkable objects found in the Suffolk Crag, and simulating human workmanship." Some time since the president had received from the Rev. W. Bird some perforated shark's teeth; but it was only an hour or two before the meeting that a box arrived containing a further collection, including many remarkable fossils from the red clay diggings of that district, the whole being specimens of the admirable series of such objects in the possession of Mr. Edward Charlesworth, F.G.S. The teeth were described as belonging to the genera *Otodus* and *Carcharodon*, and each of these teeth had a hole near its base, about a sixth of an inch in diameter, like in form and position to the holes which the South Sea Islanders make in the teeth of sharks at the present day in order to the formation of necklace ornaments. Of course, should the perforations in the teeth from the Suffolk Crag prove to have been the work of man, it would suggest that he had existed on our planet an immense time before that at present fixed as his original appearance here. But though these holes are such as might have been, and most probably were, made by human agency; they might, on the other hand, have been the work of some boring sponge, worm, or mollusc, especially as there are in this last class many species with a curious file of lingual teeth composed of silex; and even at the present day there is a complete mystery as to the means by which some invertebrates bore into and through very refractory substances. However, the whole evidence as to these holes in the shark's teeth preponderates in favour of the view that they were made by man. But even fully admitting that they were so made, it would not necessarily follow that the perforations in the teeth were made by man coeval with the crag in which they were found.

PHILADELPHIA

American Philosophical Society, February 16.—A memorial to Congress was adopted, praying for an appropriation in aid of astronomical expeditions, especially for one to the Antarctic region, for the purpose of observing properly the approaching transit of Venus.—Prof. P. E. Chase read a paper "On Correlations of Cosmical and Molecular Force." From the hypothesis that the entire energies of opposing attractive and repulsive forces may be considered as concentrated in one of the foci of the resulting oscillations, he deduced various interesting approximations to the ratio between the respective amounts of heat required for equivalent work under constant volume and under constant pressure, to the change of specific gravity in the conversion of $H_2 + O$ into H_2O, to the period of terrestrial rotation, and to the solar and lunar masses. Some idea of his method may be formed from the following approximation to the sun's mass and distance. According to the mean result of experiments by Dulong, Hess, Andrews, and Favre and Silbermann, one pound of H burned with eight pounds of O liberates enough heat to lift the nine pounds H_2O vapour, *in vacuo*, $\frac{34533 + 77^2}{9}$ feet. Such a lift would establish an oscillation, which would be perpetually sustained, by terrestrial attraction, and elastic rebound, unless otherwise counteracted. If chemicals vary as gravitating energies, the mean height of the oscillating vapour : mean height of oscillating earth :: earth mass : sun's mass. Therefore, if m = mass of sun ÷ by mass of earth, d = distance of sun ÷ earth's radius, r = earth's equatorial radius in feet, h = mean height of oscillating vapour, T_1 = solar year in seconds, T_0 = time of satellite revolution at the surface of the earth.

$$m = \frac{dr}{h}$$

$$T_0 = T_1\left(\frac{d^3}{m}\right)^{\frac{1}{2}} = T\left(\frac{hd^2}{r}\right)^{\frac{1}{2}}$$

Hence we readily obtain the values

$$d = 233{,}772 = 92{,}639{,}500 \text{ miles}$$
$$m = 350{,}260.$$

—Benjamin Smith Lyman read a paper "On the Topography of the Punjaub Oil Region." It aimed at a somewhat detailed account of the topography of the Punjaub Oil Region: its situation, general features, special features, &c. The different places are mentioned where each kind of topography is to be seen, and its causes and simple laws pointed out, chiefly in order to show the great usefulness of careful topographical studies to geology. A short sketch of the geology of the region, apart from structure, is also added; as to the oil, from the writer's own "General Report on the Punjaub Oil Lands, Lahore, 1870," and as to other points from older works. The general section of the rocks of the region is as follows, below the new and old alluviums:—

Miocene (Sivalik), perhaps	3,000 feet.
Eocene (Nummulitic), with oil ...	1,950 ,,
Mesozoic, perhaps	700 ,,
Carboniferous, without oil, about ..	1,800 ,,
Devonian, with salt and plaster ...	2,850 ,,
	10,300 ,,

The oil or asphalt (dried oil), or rock tar (melted asphalt), is found at a dozen different places, and, in very small traces, at half a dozen more, all within a space of a hundred miles square. They are all in Nummulitic rocks, except one in Carboniferous. The deposits all seem of very small horizontal extent—sometimes only a few feet, seldom a hundred yards, once only as much as half a mile. In this case, too, the oil-bearing bed is a hundred feet thick, in one other forty, in two others twenty, and in the rest much less. The oil comes in some places from lime rock, in others from sand rock, or shales. The yield of one well was at first fifty gallons a day, but grew quickly less, like the ordinates of a parabola, and seems likely to reach 3,000 gallons in all within a year and a half. At a rough guess, a hundred such wells might be bored in the region, with a whole yield, then, of hardly 7,000 barrels. The natural springs (five) yield from a gill to three quarts a day. The oil is dark green and very heavy (25° B. or less). There is nothing whatever in the Punjaub oil deposits to bear out a belief in the distillation of oil from one bed to another, or in its emanation from below, or in its gradual passage from the lower parts of a bed to higher parts of the same bed, or in its origin from any source but the decomposition of organic matter in the rocks. Neither is there anything here (or anywhere else) to justify wild hopes of finding large quantities of oil by boring into cavities below the oil-bearing bed. The occurrence of salt, gypsum, and alum shales in large quantities is noticed, as well as that of sulphur, saltpetre, brown coal, and good in small quantities, and that of traces of copper, iron, and lead.—Prof. E. D. Cope read a paper on *Bathmodon*, a genus of extinct Ungulates. It was presented as Perissodactyl in general characters, but with peculiarities of dentition of a combined ruminant and suelline character. There was on the outer side of the molars but one crescent, and before this a tubercle. The inner portion of the crown a ledge. Besides the species *Bathmodon radians*, a second form, *Loxolophodon semicinctus* was referred to the group. The former animal was large as the rhinoceros, the second equal to the tapir.—A memoir on the "Geology of Western Virginia" was presented by Mr. J. J. Stevenson.

March 1.—Mr. B. Smith Lyman presented for publication a topographical map of West Virginia.—Prof. Cope read a paper on two new species of Ornithosaurians from the Kansas Cretaceous. They were described as *Ornithochirus umbrosus* and *O. harpyia*. The former was regarded as one of the most gigantic of the pterodactyles, extending probably 25ft. from tip to tip of the wings. The other was two-thirds the size.—Prof. Cope read a paper on *Protostega*, a genus of extinct Testudinata. A detailed account of the Osteology of *P. gigas* from the Cretaceous was given, by which it appeared that the genus had separate ribs as in *Sphargis*, and that the only carapace was formed by large radiating plates of bone in the skin. Two other species were described, *P. tuberosus* and *P. neptunius*—the last, the largest known marine turtle.—Mr. Eli K. Price read a paper on "Some other Phases of Modern Philosophy," in which he combatted the views of Huxley and others as to the physical basis of life.

PARIS

Academy of Sciences, May 20.—M. Becquerel read a ninth memoir on the means of increasing the effects of electro-capillary actions in inorganic bodies, and on the effects of the same kind produced in living organised bodies.—M. Sainte-Claire Deville presented a note by M. G. Guéroult on the relations existing between the numbers of vibrations of musical sounds and their intervals, and on a scale-rule for acoustic calculations invented by him.—M. Jamin communicated a note by M. J. M. Gaugain on the electro-motive forces developed by the contact of metals with inactive liquids, in continuation of a former paper by the same author; and M. T. Du Moncel presented a note on the induced currents resulting from the action of magnets upon induction coils normally to their axes.—A memoir was read by M. Le Verrier on the theories of the four superior planets, Jupiter, Saturn, Uranus, and Neptune, containing an investigation of the perturbations which each of these four planets undergo by the action of the other three.—A letter was read from Father Secchi, containing a summary of observations of solar protuberances from Jan. 1 to April 29, 1872, containing a tabular exposition of the results of observations, with a discussion of their bearing upon the general question.—M. Delaunay presented an extract of a letter from M. Förster on the magnetic disturbances observed during the occurrence of auroras.—M. Coste communicated a note by M. Z. Gerbe on the segmentation of the cicatricula in the ovum of plagiostomous fishes, in which the author describes the evolution of the Rays in confirmation of the opinion put forward by M. Coste that in the plagiostomous fishes, as in reptiles and birds, it is the cicatricula alone that undergoes segmentation. M. Coste also communicated a notes by M. G. Pouchet, on blue colorations in fishes, in which the author ascribes the blue colours presented by some parts of certain fishes to the presence beneath the skin of oval or roundish bodies which he calls "iridising bodies."—A note by M. E. Prillieux, on the influence of congelation upon the weight of vegetable tissues, was presented by M. Duchartre.—M. A. Rivière read a memoir on the oolitic or Jurassic formation of La Vendée, accompanied by a geological map of that locality.—M. H. Douvillé presented a note on the coal-bearing strata of the banks of the Rhine, with especial reference to the distribution of these deposits into an upper and lower formation, the deposition of which was separated by a great dislocation.—M. S. Meunier presented a mineralogical investigation of the grey serpentines, from which it results that these rocks consist essentially of a mixture of magnetite, pyroxene, peridot, and magnesite.—M. A. Leymerie communicated a note in reply to a recent communication by M. Garrigou on the unity of composition of the Pyrenees.

May 27.—M. Serret presented a memoir by M. V. J. Berton on the determination of the limits between which a primary number of a given form occurs.—M. Chasles presented a note by Mr. A. Cayley on a flattened quartic surface; M. Ribaucour a note on the developates of surfaces.—A note by M. C. Jordan on the infinitely small oscillations of material systems was presented by M. Yvon Villarceau.—M. Faye presented a reply by M. Respighi to a recent note by Father Secchi on some peculiarities of the constitution of the sun.—M. Le Verrier communicated a note by Father Denza on meteors observed in Piedmont on the evening of April 24, including the account of a second meteor in addition to that seen at Agde by M. Perris. The author also notices the occurrence of a fine aurora on May 9.—M. Becquerel read a note on the cultivation of the vine in clay soils, in which he indicated the conditions of temperature prevailing in clay, silicious, and calcareous soils, and showed that in the former the vine could only be successfully cultivated by training it to a considerable height.—M. W. de Fonvielle presented a note embodying some fresh examples of the danger arising from the vicinity of metallic masses during storms, and M. E. Nasse forwarded a note on an instance of globular lightning observed at Brives on May 17.—M. Delaunay presented a note by M. Frou on the laws of cyclones and tempests, and on their geometrical representations.—A considerable number of papers on chemical subjects was presented, namely, a memoir on the iron contained in the blood and in food by M. Boussingault, containing a great number of interesting details upon this important subject; a note by M. A. Wurtz on an aldehyde-alcohol, $C^4 H^8 O^2$, which he proposes to name *aldol*; a note by M. G. Bouchardat on a new organic base, dulcitamine, $C^{12} H^{15} NO^{10}$, derived from dulcite, presented by M. A. Wurtz; a note by M. T. Schlœsing on the influence of vegetable mould on the mobility of soils, communicated by M. Peligot; a memoir by Mr. F. Crace-Calvert on bleaching powder, presented by M. Balard; a note by M. B. Rénault on a new process for obtaining reproductions of drawings, also presented by M. Balard; and a note by M. Sidot on the production of a crystallised phosphuret of iron, presented with some remarks by M. Daubrée.—M. Cl. Bernard communicated a paper by M. Z. Pupier, containing an experimental demonstration of the action of spirituous beverages upon the liver; the author's experiments were made upon fowls and rabbits.—A note by MM. N. and E. Joly on the supposed Crustacean, on which Latreille founded the genus *Prosopistoma*, was presented by M. Milne-Edwards; the authors show that this animal is a true insect probably allied to the Ephemerina.—M. Blanchard communicated a note by M. S. Jourdain on the anurous Batrachia with large and small tadpoles, in which the author insists upon an analogy between the development of the former and that of insects.—M. de Vibraye communicated a note on the spontaneous appearance in France of exotic forage-plants consequent on the presence of the belligerent armies in 1870-71.—M. P. Gervais presented a note on the mammalia of which the bones are associated with the deposits of phosphate of lime in the departments of Tarn-et-Garonne and the Lot. These bones appear to belong to various epochs, the oldest belonging to the fauna of the Paris gypsum, others to the Miocene or more recent times. The genera represented are *Anoplotherium*, *Dichobune*, *Entelodon*, *Cainotherium*, *Amphitragulus*, *Palæotherium*, *Rhinoceros*, *Hyænodon*, *Canis?* (*palæolycos*, sp. n.) and *Viverra?* (*ambigua*, sp. n.) At Caylux there are Rodents allied to *Theridomys*, with remains of *Chalicotherium*, *Anthracotherium*, and *Antilope? boodon*.—M. Daubrée made some remarks on the deposits from which the fossils noticed by M. Gervais are derived.—M. de Verneuil read a note on the recent eruption of Vesuvius, which also formed the subject of a communication from M. Guiscardi.

BOOKS RECEIVED

ENGLISH.—Experimental Chemistry, founded on the work of Dr. J. A. Stöckhardt: C. W. Heaton (Bell and Daldy).—Knapsack Manual for Sportsmen in the Field: E. Ward (Bradbury and Evans).

DIARY

THURSDAY, JUNE 6.

ROYAL INSTITUTION, at 3.—On Heat and Light: Prof Tyndall, F.R.S.
SOCIETY OF ANTIQUARIES, at 8.30.—Excavations at Rome, 1871-2: J. H. Parker, C.B., F.S.A.
LINNEAN SOCIETY, at 8.—On some recent forms of *Lagenæ* from Deep-Sea Dredgings in the Japanese Seas: F. W. O. Rymer Jones.—On the Cutaneous Exudation of the Water Newt (*Triton cristatus*): Miss Eleanor A. Ormerod.
CHEMICAL SOCIETY, at 8.

FRIDAY, JUNE 7.

GEOLOGISTS' ASSOCIATION, at 8.—On the Classification of the Cambrian and Silurian Rocks: H. Hicks.—On the Silurian Rocks of the English Lake District: Prof. A. Nicholson, M.D.

SATURDAY, JUNE 8.

GOVERNMENT SCHOOL OF MINES, at 8.—On Geology: Dr. Cobbold, F.R.S.

MONDAY, JUNE 10.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

TUESDAY, JUNE 11.

PHOTOGRAPHIC SOCIETY, at 8.—On some early Glass Pictures produced by the late Sir John Herschel, Bart.: Prof. A. S. Herschel, B.A., F.R.A.S.—On the Photographic Manipulations undertaken at the last Eclipse, practically described: Capt. Waterhouse, Assistant Surveyor General of India.—Spectroscopic Observations in connection with the Carbon Process: Lieut. Abney, R.E.—On the Use of Uranium in Dry Plate Photography: Colonel Stuart Wortley.

THURSDAY, JUNE 13.

ROYAL SOCIETY, at 8.30.
SOCIETY OF ANTIQUARIES, at 8.30.
MATHEMATICAL SOCIETY, at 8.—On the Surfaces divisible into Squares by Curves of Curvature: Prof. Cayley.—On Prof. Cremona's Transformation between Two Planes and Tables relating thereto: Mr. S. Roberts.—On a Manifold Correspondence of Two Planes: Dr. Hirst.

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