THURSDAY, OCTOBER 26, 1882

SCIENTIFIC WORTHIES

XX.—JAMES PRESCOTT JOULE

JAMES PRESCOTT JOULE was born at Salford on Christmas-Eve of the year 1818. His father and his grandfather before him were brewers, and the business, in due course, descended to Mr. Joule and his elder brother, and by them was carried on with success till it was sold in 1854. Mr. Joule's grandfather came from Elton, in Derbyshire, settled near Manchester, where he founded the business, and died at the age of fifty-four in 1799. His father, one of a numerous family, married a daughter of John Prescott of Wigan. They had five children, of whom James Prescott Joule was the second, and of whom three were sons—Benjamin, the eldest, James, and John, and two daughters—Alice and Mary. Mr. Joule's mother died in 1836 at the age of forty-eight; and his father, who was an invalid for many years before his death, died at the age of seventy-four in the year 1858.

Young Joule was a delicate child, and was not sent to school. His early education was commenced by his mother's half-sister, and was carried on at his father's house, Broomhill, Pendlebury, by tutors, till he was about fifteen years of age. At fifteen he commenced working in the brewery, which, as his father's health declined, fell entirely into the hands of his brother Benjamin and himself.

Mr. Joule obtained his first instruction in physical science from Dalton, to whom his father sent the two brothers to learn chemistry. Dalton, one of the most distinguished chemists of any age or country, was then president of the Manchester Literary and Philosophical Society, and lived and received pupils in the rooms of the Society's House. Many of his most important memoirs were communicated to the Society, whose *Transactions* are likewise enriched by a large number of communications from his distinguished pupil. Dalton's instruction to the two young men commenced with arithmetic, algebra, and geometry. He then taught them natural philosophy out of Cavallo's text-book, and afterwards, but only for a short time before his health gave way in 1837, chemistry from his own "New System of Chemical Philosophy." "Profound, patient, intuitive," his teaching must have had great influence on his pupils. We find Mr. Joule early at work on the molecular constitution of gases, following in the footsteps of his illustrious master, whose own investigations on the constitution of mixed gases, and on the behaviour of vapours and gases under heat, were among the most important of his day, and whose brilliant discovery of the Atomic Theory revolutionised the science of Chemistry and placed him at the head of the philosophical chemists of Europe.

Under Dalton, Mr. Joule first became acquainted with physical apparatus; and the interest excited in his mind very soon began to produce fruit. Almost immediately he commenced experimenting on his own account. Obtaining a room in his father's house for the purpose, he began by constructing a cylinder electric machine in a very primitive way. A glass tube served for the cylinder; a poker hung up by silk threads, as in the very oldest forms of electric machine, was the prime conductor; and for a Leyden jar he went back to the old historical jar of Cunæus, and used a bottle half filled with water, standing in an outer vessel, which contained water also.

Enlarging his stock of apparatus, chiefly by the work of his own hands, he soon entered the ranks as an investigator, and original papers followed each other in quick succession. The Royal Society List now contains the titles of ninety-seven papers due to Joule, exclusive of over twenty very important papers detailing researches undertaken by him, conjointly with Thomson, with Lyon Playfair, and with Scoresby.

Mr. Joule's first investigations were in the field of magnetism. In 1838, at the age of nineteen, he constructed an electro-magnetic engine, which he described in Sturgeon's "Annals of Electricity" for January of that year. In the same year and in the three years following he constructed other electro-magnetic machines and electro-magnets of novel forms; and experimenting with the new apparatus, he obtained results of great importance in the theory of electro-magnetism. In 1840 he discovered, and determined the value of the limit to the magnetisation communicable to soft iron by the electric current; showing for the case of an electro-magnet supporting weight, that when the exciting current is made stronger and stronger, the sustaining power tends to a certain definite limit, which, according to his estimate, amounts to about 140 lbs. per square inch of either of the attracting surfaces. He investigated the relative values of solid iron cores for the electro-magnetic machine as compared with bundles of iron wire; and, applying the principles which he had discovered, he proceeded to the construction of electro-magnets of much greater lifting power than any previously made, while he studied also the methods of modifying the distribution of the force in the magnetic field.

In commencing these investigations he was met at the very outset, as he tells us, with "the difficulty, if not impossibility, of understanding experiments and comparing them with one another which arises in general from incomplete descriptions of apparatus, and from the arbitrary and vague numbers which are used to characterise electric currents. Such a practice," he says, "might be tolerated in the infancy of science; but in its present state of advancement greater precision and propriety are imperatively demanded. I have therefore determined," he continues, "for my own part to abandon my old quantity numbers, and to express my results on the basis of a unit which shall be at once scientific and convenient."

The discovery by Faraday of the law of electro-chemical equivalents had induced him to propose the voltameter as a measurer of electric currents; but the system proposed had not been used in the researches of any electrician, not excepting those of Faraday himself. Joule, realising for the first time the importance of having a system of electric measurement which would make experimental results obtained at different times and under various circumstances comparable among themselves, and perceiving at the same time the advantages of a system of electric measurement, dependent on, or at any rate comparable with the chemical action producing the electric current, adopted as unit quantity of electricity, the quantity required to decompose nine grains of water, 9 being the atomic weight of water, according to the chemical nomenclature then in use.

He had already made and described very important improvements in the construction of galvanometers, and he graduated his tangent galvanometer to correspond with the system of electric measurement he had adopted. The electric currents used in his experiments were thenceforth measured on the new system; and the numbers given in Joule's papers from 1840 downwards are easily reducible to the modern absolute system of electric measurements, in the construction and general introduction of which he himself took so prominent a part. It was in 1840, also, that after experimenting on improvements in voltaic apparatus, he turned his attention to "the heat evolved by metallic conductors of electricity, and in the cells of a battery during electrolysis." In this paper and those following it in 1841 and 1842, he laid the foundation of a new province in physical science—electric and chemical thermodynamics—then totally unknown, but now wonderfully familiar even to the roughest common-sense practical electrician. With regard to the heat evolved by a metallic conductor carrying an electric current, he established what was already supposed to be the law, namely, that "the quantity of heat evolved by it [in a given time] is always proportional to the resistance which it presents, whatever may be the length, thickness, shape, or kind of the metallic conductor," while he obtained the law, then unknown, that the heat evolved is proportional to the *square* of the quantity of electricity passing in a given time. Corresponding laws were established for the heat evolved by the current passing in the electrolytic cell, and likewise for the heat developed in the cells of the battery itself.

In the year 1840 he was already speculating on the transformation of chemical energy into heat. In the paper last referred to and in a short abstract in the *Proceedings* of the Royal Society, December, 1840, he points out that the heat generated in a wire conveying a current of electricity is a part of the heat of chemical combination of the materials used in the voltaic cell, and that the remainder, not the whole heat of combination, is evolved within the cell in which the chemical action takes place. In papers given in 1841 and 1842, he pushes his investigations farther, and shows that the sum of the heat produced in all parts of the circuit during voltaic action is proportional to the chemical action that goes on in the voltaic pile, and again, that the quantities of heat which are evolved by the combustion of equivalents of bodies are proportional to the intensities of their affinities for oxygen. Having proceeded thus far, he carried on the same train of reasoning and experiment till he was able to announce, in January, 1843, that the magneto-electric machine enables us to *convert mechanical power into heat*. Most of his spare time in the early part of the year 1843 was devoted to making experiments necessary for the discovery of the laws of the development of heat by magneto-electricity, and for the definite determination of the mechanical value of heat.

At the meeting of the British Association at Cork, on August 21, 1843, he read his paper "On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat." The paper gives an account of an admirable series of experiments, proving that *heat* is *generated* (not merely *transferred* from some source) by the magneto-electric machine. The investigation was pushed on for the purpose of finding whether a *constant ratio exists between the heat generated and the mechanical power* used in its production. As the result of one set of magneto-electric experiments he finds 838 foot lbs. to be the mechanical equivalent of the quantity of heat capable of increasing the temperature of one pound of water by one degree of Fahrenheit's scale. The paper is dated Broomhill, July, 1843, but a postscript dated August, 1843, contains the following sentences:—"We shall be obliged to admit that Count Rumford was right in attributing the heat evolved by boring cannon to friction, and not (in any considerable degree) to any change in the capacity of the metal. I have lately proved experimentally that *heat is evolved by the passage of water through narrow tubes*. My apparatus consisted of a piston perforated by a number of small holes, working in a cylindrical glass jar containing about 7 lbs. of water. I thus obtained one degree of heat per lb. of water from a mechanical force capable of raising about 770 lbs. to the height of one foot, a result which will be allowed to be very strongly confirmatory of our previous deductions. I shall lose no time in repeating and extending these experiments, being satisfied that the grand agents of nature are, by the Creator's fiat, *indestructible*, and that wherever mechanical force is expended, an exact equivalent of heat is *always* obtained."

This was the first determination of the dynamical equivalent of heat. Other naturalists and experimenters about the same time were attempting to compare the quantity of heat produced under certain circumstances with the quantity of work expended in producing it; and results and deductions (some of them very remarkable) were given by Séguin (1839), Mayer (1842), Colding (1843), founded partly on experiment, and partly on a kind of metaphysical reasoning. It was Joule, however, who first definitely proposed the problem of determining the relation between heat produced and work done in any mechanical action, and solved the problem directly.

It is not to be supposed that Joule's discovery and the results of his investigation met with immediate attention or with ready acquiescence. The problem occupied him almost continuously for many years; and in 1878 he gives in the *Philosophical Transactions* the results of a fresh determination according to which the quantity of work required to be expended in order to raise the temperature of one pound of water weighed in vacuum from 60° to 61° Fah., is 772·55 foot lbs. of work at the sea-level, and in the latitude of Greenwich. His results of 1849 and 1878 agree in a striking manner with those obtained by Hirn and with those derived from an elaborate series of experiments carried out by Prof. Rowland at the expense of the Government of the United States.

His experiments subsequent to 1843 on the dynamical equivalent of heat must be mentioned briefly. In that year his father removed from Pendlebury to Oak Field, Whalley Range on the south side of Manchester, and built for his son a convenient laboratory near to the house. It was at this time that he felt the pressing need of accurate thermometers; and whilst Regnault was doing the same thing in France Mr. Joule produced, with the assistance of Mr. Dancer, instrument maker, of Manchester, the first English thermometers possessing such accuracy as the mercury-in-glass thermometer is capable of. Some of them were forwarded to Prof. Graham and to Prof. Lyon Playfair; and the production of these instruments was in itself a most important contribution to scientific equipment.

As the direct experiment of friction of a fluid is dependent on no hypothesis, and appears to be wholly unexceptionable, it was used by Mr. Joule repeatedly in modified forms. The stirring of mercury, of oil, and of water with a paddle, which was turned by a falling weight, was compared, and solid friction, the friction of iron on iron under mercury, was tried; but the simple stirring of water seemed preferable to any, and was employed in all his later determinations.

In 1847 Mr. Joule was married to Amelia, daughter of Mr. John Grimes, Comptroller of Customs, Liverpool. His wife died early (1854), leaving him one son and one daughter.

The meeting of the British Association at Oxford in this year, proved an interesting and important one. Here Joule read a fresh paper "On the Mechanical Equivalent of Heat." Of this meeting Sir William Thomson writes as follows to the author of this notice:—

"I made Joule's acquaintance at the Oxford meeting, and it quickly ripened into a life-long friendship.

"I heard his paper read in the section, and felt strongly impelled at first to rise and say that it must be wrong because the true mechanical value of heat given, suppose in warm water, must, for small differences of temperature, be proportional to the square of its quantity. I knew from Carnot that this *must* be true (and it *is* true; only now I call it 'motivity,' to avoid clashing with Joule's 'mechanical value.') But as I listened on and on, I saw that (though Carnot had vitally important truth, not to be abandoned) Joule had certainly a great truth and a great discovery, and a most important measurement to bring forward. So instead of rising with my objection to the meeting I waited till it was over, and said my say to Joule himself, at the end of the meeting. This made my first introduction to him. After that I had a long talk over the whole matter at one of the *conversaziones* of the Association, and we became fast friends from thenceforward. However, he did not tell me he was to be married in a week or so; but about a fortnight later I was walking down from Chamounix to commence the tour of Mont Blanc, and whom should I meet walking up but Joule, with a long thermometer in his hand, and a carriage with a lady in it not far off. He told me he had been married since we had parted at Oxford! and he was going to try for elevation of temperature in waterfalls. We trysted to meet a few days later at Martigny, and look at the Cascade de Sallanches, to see if it might answer. We found it too much broken into spray. His young wife, as long as she lived, took complete interest in his scientific work, and both she and he showed me the greatest kindness during my visits to them in Manchester, for our experiments on the thermal effects of fluid in motion, which we commenced a few years later."

"Joule's paper at the Oxford meeting made a great sensation. Faraday was there and was much struck with it, but did not enter fully into the new views. It was many years after that before any of the scientific chiefs began to give their adhesion. It was not long after, when Stokes told me he was inclined to be a Joulite."

"Miller, or Graham, or both, were for years quite incredulous as to Joule's results, because they all depended on fractions of a degree of temperature—sometimes very small fractions—his boldness in making such large conclusions from such very small observational effects, is almost as noteworthy and admirable as his skill in extorting accuracy from them. I remember distinctly at the Royal Society, I think it was either Graham or Miller, saying simply he did not believe Joule, because he had nothing but hundredths of a degree to prove his case by."

The friendship formed between Joule and Thomson in 1847 grew rapidly. A voluminous correspondence was kept up between them, and several important researches were undertaken by the two friends in common. Their first joint research was on the thermal effects experienced by air rushing through small apertures. The results of this investigation give for the first time an experimental basis for the hypothesis assumed without proof by Mayer as the foundation for an estimate of the numerical relation between quantities of heat and mechanical work, and they show that for permanent gases the hypothesis is very approximately true. Subsequently Joule and Thomson undertook more comprehensive investigations on the thermal effects of fluids in motion, and on the heat acquired by bodies moving rapidly through the air. They found the heat generated by a body moving at one mile per second through the air sufficient to account for its ignition. The phenomena of "shooting stars" were explained by Mr. Joule in 1847 by the heat developed by bodies rushing into our atmosphere.

It is impossible within the limits to which this sketch is necessarily confined, to speak in detail of the many researches undertaken by Mr. Joule on various physical subjects. Even of the most interesting of these a very brief notice must suffice for the present.

Molecular physics, as I have already remarked, early claimed his attention. Various papers on electrolysis of liquids, and on the constitution of gases, have been the result. A very interesting paper on "Heat and the Constitution of Elastic Fluids" was read before the Manchester Literary and Philosophical Society in 1848. In it he developed Daniel Bernoulli's explanation of air pressure by the impact of the molecules of the gas on the sides of the vessel which contains it, and from very simple considerations he calculated the average velocity of the particles requisite to produce ordinary atmospheric pressure at different temperatures. The average velocity of the particles of hydrogen at 32° F. he found to be 6055 feet per second, the velocities at various temperatures being proportional to the square roots of the numbers which express those temperatures on the absolute thermodynamic scale.

His contribution to the theory of the velocity of sound in air was likewise of great importance, and is distinguished alike for the acuteness of his explanations of the existing causes of error in the work of previous experimenters, and for the accuracy, so far as was required for the purpose in hand, of his own experiments. His determination of the specific heat of air, pressure constant, and the specific heat of air, volume constant, furnished the data necessary for making Laplace's theoretical velocity agree with the velocity of sound experimentally determined. On the other hand, he was able to account for most puzzling discrepancies which appeared in attempted direct determinations of the differences between the two specific heats by careful experimenters. He pointed out that in experiments in which air was allowed to rush violently or *explode* into a vacuum, there was a source of loss of energy that no one had taken account of, namely, in the sound produced by the explosion. Hence in the most careful experiments where the vacuum was made as perfect as possible, and the explosion correspondingly the more violent, the results were actually the worst. With his explanations the theory of the subject was rendered quite complete.

Space fails, or I should mention in detail Mr. Joule's experiments on magnetism and electro-magnets, referred to at the commencement of this sketch. He discovered the now celebrated change of dimensions produced by the magnetisation of soft iron by the current. The peculiar noise which accompanies the magnetisation of an iron bar by the current, sometimes called the "magnetic tick," was thus explained.

Mr. Joule's improvements in galvanometers have already been incidentally mentioned, and the construction by him of accurate thermometers has been referred to. It should never be forgotten that *he first* used small enough needles in tangent galvanometers to practically annul error from want of uniformity of the magnetic field. Of other improvements and additions to philosophical instruments may be mentioned a thermometer, unaffected by radiation, for measuring the temperature of the atmosphere, an improved barometer, a mercurial vacuum pump, one of the very first of the species which is now doing such valuable work not only in scientific laboratories, but in the manufacture of incandescent electric lamps, and an apparatus for determining the earth's horizontal magnetic force in absolute measure.

Here this imperfect sketch must close. My limits are already passed. Mr. Joule has never been in any sense a public man; and, of those who know his name as that of the discoverer who has given the experimental basis for the grandest generalisation in the whole of physical science, very few have ever seen his face. Of his private character this is scarcely the place to speak. Mr. Joule is still amongst us. May he long be spared to work for that cause to which he has given his life with heart-whole devotion that has never been excelled.

In June, 1878, he received a letter from the Earl of Beaconsfield announcing to him that Her Majesty the Queen had been pleased to grant him a pension of 200*l*. per annum. This recognition of his labours by his country was a subject of much gratification to Mr. Joule.

Mr. Joule received the Gold Royal Medal of the Royal Society in 1852, the Copley Gold Medal of the Royal Society in 1870, and the Albert Medal of the Society of Arts from the hands of the Prince of Wales in 1880.

J. T. BOTTOMLEY

COAL-TAR

A Treatise on the Distillation of Coal-Tar and Ammoniacal Liquor, and the Separation from them of Valuable Products. By George Lunge, Ph.D., F.C.S., Professor of Technical Chemistry in the Federal Polytechnic School, Zurich. (London: Van Voorst, 1882.)

A couple of centuries have just elapsed since the first English patent was granted to Becker and Serle for "a new way of makeing pitch and tarre out of pit coale, never before found out or used by any other"; and in 1742 a second patent was obtained by M. and T. Betton for the manufacture of "an oyle extracted from a flinty rock for the cure of rheumatick, and scorbutick, and other cases." Whether we have here a foreshadowing of the antiseptic method of treatment is impossible to say, but that there was virtue of another sort in coal-tar was fully recognised by the Earl of Dundonald, the father of brave Lord Cochrane, who, towards the close of the last century, set up tar ovens on a pretty extensive scale in Ayrshire.

What we know as the coal-tar manufacture is however practically an industry of the present generation: it is not even contemporaneous with that of the making of coal-gas, for during the earlier years of that manufacture the tar was counted as the most noxious of bye products to be got rid of by being burnt under the retorts or by being turned into the nearest stream. We have changed all that however, and to-day the tar is among those substances which, as Dr. Siemens pointed out the other day at Southampton, make the products of the destructive distillation of coal so much more valuable than the coa itself.

England is the great tar-producing country of the world; at the present time about half a million tons o tar are produced annually throughout Europe, of which we make about three-fifths. The distillation of coal-tar as a starting-point in the manufacture of colouring matters has indeed become one of our most important chemical industries. We however do not make these colouring matters although we are the principal users of them. Although Faraday first discovered benzene, and Mansfield gave his life in showing us how to isolate that substance on the large scale, and although Perkin led the way by the discovery of aniline purple, the first coal-tar colour; nevertheless the manufacture of the so-called coal-tar dyes has mainly centred itself in Germany. We send to the Germans the crude material, and they return to us the finished products. At the same time we also supply many of the chemicals necessary to transmute the baser substances into the costly dyes. In fact in this matter we are as mere hewers of wood and drawers of water; a circumstance which doubtless has not escaped the attention of the Royal Commissioners who are to report on the technical education of this country. We have not far to seek in tracing the cause of this: it is simply owing to the extraordinary development of chemical research in Germany arising largely from the attitude of the German universities towards scientific inquiry.

We have to thank Prof. Lunge for what is in reality the only monograph on this subject of tar distilling either in our own or in any continental language. Probably no one more fitted, both from practical experience and scientific knowledge, could have been found to undertake the work. Already in 1867 Dr. Lunge had published a treatise in German on the subject; this has now been elaborated into the present excellent work, which describes the processes of manufacture as carried on in the largest and best arranged tar and ammonia works in England and the Continent. In the preparation of the newer work the author has received much assistance from Mr. Watson Smith, who has extensive knowledge of these processes as carried out in Lancashire.

Chapter I. is mainly concerned with the origin of coal-tar; with historical notes on its applications, and with the general characters of the tars obtained from various sources. Much in this chapter, as indeed in other parts of the work, is of direct interest to the gas-engineer. An iron smelter has been defined as one who makes slag, and the economical production of cast-iron is very much a question of the economical production of the proper sort of slag. So important indeed are, nowadays, the "residual" products in the manufacture of coal-gas that a gas-engineer may with even greater truth be described as a maker of coal-tar and ammonia-water, and we fully agree with Dr. Lunge that with the electric light looming in the near future, gas managers will have to consider the market prices of these "residuals," as influencing the mode of their manufacture, more carefully than they have hitherto done. They must in fact recognise that they are just as much makers of tar and ammonia as of coal-gas, and whether the one or the other is to be worked for must be governed by calculations depending upon the relative prices of gas and tar.

Chapter II. deals mainly with the properties of coal-tar and its constituents. A very complete list of these is given, and special attention is paid to their physical characters whenever these have been ascertained. Benzene, of course, is very fully described, even to an account of the rival theories of Kekulé, Claus, and Ladenburg as to its constitution. We entirely endorse Dr. Lunge's recognition of the enormous value of Kekulé's famous hypothesis in the development of the history of the aromatic derivatives; nevertheless the average tar distiller will, we are afraid, be lost in wonder and amazement at the idea of such fruitful consequences flowing from pictures of hexagons and prisms. In other words the description on p. 40 of the chemical constitution of the parent member of the aromatic group is far too bald to be of the slightest use to persons ignorant of the modern methods of representing constitution, and conveys no new information to those who know anything of such matters.

Chapter III. treats of the applications of coal-tar without distillation, such as its use for gas making, heating, and for the preservation of building materials and its use as an antiseptic, and in the manufacture of paints, varnishes, &c. Chapter IV. deals with the methods of distilling coal-tar, such as its distillation by steam and by fire. This and the next chapter (Chapter V.), on pitch, are extremely well illustrated by cuts and plates showing the best methods of constructing stills and condensing apparatus, mode of treating the gases and the different fractions, and a series of most valuable figures and tables are given of the results obtained in various works in England and on the Continent from different tars. Chapters VI. and VII. treat of anthracene and creosote oil, and considerable attention is given to the important question of the quantitative determination of anthracene and of the so-called coal-tar acids. Chapter VIII. is concerned with phenol or carbolic acid and naphthalene, and contains many valuable details as to the manufacture of carbolic acid hitherto unpublished: we would especially instance the careful description of the manufacture of pure phenol, as carried on in Lancashire which is furnished by Mr. Watson Smith. Chapters IX. and X. treat of what is technically known as "light oil" or "crude naphtha," and of its rectification by steam. The last chapter (Chap. XI.) is entirely devoted to the subject of gas-liquor, or the ammoniacal liquor obtained at the gas works by condensation in the hydraulic main and by washing the gas in the scrubbers. Ammonia is in fact one of the most important products of the destructive distillation of coal; indeed the supply falls very far short of the demand made by the employment of ammoniacal salts in artificial manures and in the manufacture of soda ash by the modern method. The price of sulphate of ammonia has been pratically doubled within the last twenty years. Whether ammonia will ever be produced commercially from the nitrogen of the air is a vexed question, but there is no doubt that if the coking of coals could only be carried out in a rational manner we might count upon an important addition to our stock of ammonia and of tar. It is indeed to this source that we must more immediately look for the increased supply so urgently needed.

Dr. Lunge has already enriched our literature by a most valuable treatise on another of our most important chemical industries, viz. the manufacture of alkali, and he has still further added to our debt by the publication of the present excellent manual. The work is extremely well got up, and deserves to be on the table of every gas manager and tar distiller in the kingdom.

T. E. THORPE

OUR BOOK SHELF

Tables for the Qualitative Analysis of "Simple Salts" and "Easy Mixtures." By Joseph Barnes. (Manchester: James Galt and Co.; London: Simpkin, Marshall, and Co., 1882.)

THESE tables are evidently compiled by one who has had considerable experience in teaching qualitative analysis; the directions are always clear and to the point; the student is not confused by too many alternative methods, neither is the art of analysis made altogether a matter to be learnt by rote. The short and simple solubilities table on p. 37 is especially to be commended. If we must have yet another set of tables for elementary qualitative analysis let us have these by Mr. Barnes; but have we not sufficient already?

LETTERS TO THE EDITOR

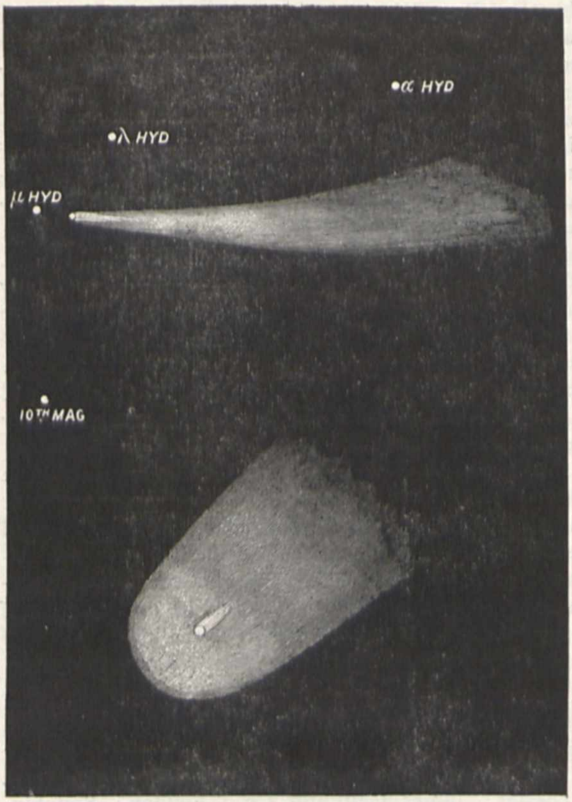
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

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

The Comet

AT 4.30 this morning this comet was a most conspicuous and lovely object in the clear sky, in the south-east. With the assistance of Mr. Hodges and Mr. Percy Smith, the following details were obtained. The tail extended for quite 15° in length, and about 5° in width at its widest part, being slightly curved with the convexity downwards. The lower edge of the tail was very sharp, but the upper edge was gradually shaded off. The nucleus was considerably lengthened out in the direction of the tail to an extent of quite three times its width. Its estimated length was 10″.

On examination with the spectroscope, with the nucleus across the slit, there appeared a narrow continuous spectrum crossed by three bands, which I at once recognised as the usual hydrocarbon lines; the central one was the brightest, and I could see no other lines but these three.



5 a.m. October 23, 1882.

At 5.2 a.m. Greenwich time, the position of the nucleus was determined with the equatorial to be R.A. 10h. 9m. 33s., Dec. 16° 18′7″, being a mean of two observations.

I send sketches of the comet, a small star, which I have not identified, appeared in the field of view about 2′ 40″ from the nucleus as drawn, and if identified may assist to check the position of the nucleus as given by the circles.

The morning was exceptionally clear, so much so, that, at dawn, when we could read small print out of doors, 4th magnitude stars were clearly visible. GEO. M. SEABROKE

Rugby, October 23

I INCLOSE a drawing made this morning after a prolonged examination (with a binocular) of the end of the comet's tail. Should you think the peculiar features which I have endeavoured to portray of sufficient interest to reproduce, the drawing is at your service. It is difficult to indicate truly features of this kind without exaggeration, if they are to catch the eye at all; but I am sure the exaggeration is very slight. The tail would seem to be about to end rather suddenly and with a broad end, when, from near the middle, shoots out, at a slight inclination to the general direction of the tail, a cleanly-shaded wisp. And as though this were due to a kind of cleft or parting, there is a corresponding broader sweeping-aside of the tail-end on the other side. One is at once reminded of the backward fraying of the broad side of a large feather. The effect is a decided enlargement of the end of the tail on one side, and a well-defined streamer shooting out at a slight inclination towards the other. The direction of the latter is such as to pass quite clear of the head, which is not a necessary consequence of its inclination, because of the curve which characterises the sharply-defined southern edge of the whole tail.

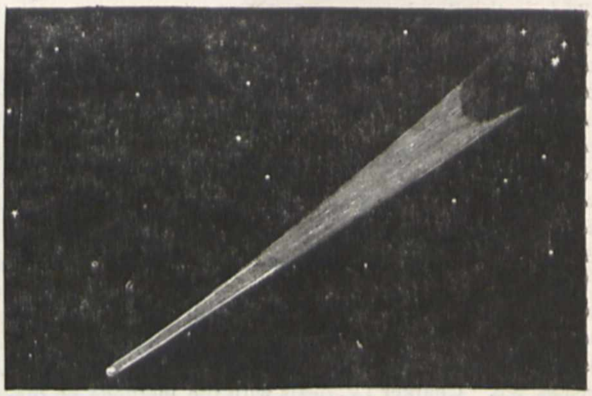
It is surely unusual for such decided features to present themselves at the very end of a comet's tail.

As a whole, the comet seems to have changed wonderfully little during the three weeks since I first saw it. Its change of place, also, is so moderate that, at this rate, there seems no reason why we should not see it for months yet. What if it should not vanish at all! J. HERSCHEL

Collingwood, October 23

FOR several mornings past we have had fine views of the comet, first seen in England by Mr. A. Common. I inclose a sketch taken this morning, as accurate as I could make it with materials at hand.

It is chiefly remarkable (1) for the crescentic end of the tail, the lower or eastern horn being longer than the other; (2) for the distinctness of the shadow in the space beyond the tail, shadow obviously projected by the comet. Such a shadow I have never seen in any of the comets which have been under my observation during the last fifty years, nor do I recollect to have



The Comet from Cannes, October 21, between 5 and 6 a.m.

seen it described. (Here I have no access to books on the subject.)

I presume that the propinquity of this comet to the sun is the reason why the shadow is unusually visible in contrast to the luminosity around it; but probably the peculiar clearness of our atmosphere renders the phenomenon clearer than it may be in England. In any case the appearance is interesting in relation both to the nature of cometary matter, and to that of light and shade in space. C. J. B. WILLIAMS

Villa du Rocher, Cannes, France, October 21

THE comet was well seen here on Monday, October 23, for some considerable time about 5 a.m., though clouds occasionally hid part of it. I noticed the following:—1. The length that was clearly visible was such, that if the head had been placed on Sirius, the tail would have reached to Orion's belt. 2. The lower edge of the tail was comparatively sharp and brightly defined, while there was no well-defined upper edge. 3. At first sight the tail ended, fairly abruptly, in a short fork. But on glancing to one side, so as to allow the image to fall on a more sensitive part of the retina, one became aware that these two forks were continued in a very faint and hazy manner as far again as the length of the comet first noticed (mentioned and measured in (1.)) Or, more strictly, one became aware of a black rift in the sky behind the comet, in its direction, above and below which the sky was faintly luminous. One may say that at first sight the comet ended like a house-martin's, on more careful observation like a swallow's, tail. The *total* length of the comet thus seen was enormous; and the appearance suggested an even greater extension. W. LARDEN

Cheltenham, October 24

ALTHOUGH the fact is mentioned in NATURE of the 5th inst., that the comet was observed by Mr. Finlay, the First Assistant to the Astronomer-Royal at the Cape of Good Hope, at 5 a.m. on September 8, perhaps the following graphic account of its appearance, which I extract from a letter received this morning from my friend Mr. G. B. Bennett, dated Water-Hof, Cape Town, September 26, may have some interest. Mr. Bennett believed himself the earliest observer, but he does not consider the comet more conspicuous on this occasion than it was in 1843.

"I take an especial interest in our present visitor, as I fancy that I am the very first person who saw it, and this was on the 8th inst. at 5 a.m. I was attracted into the garden by the marvellous brilliancy of the stars. On turning my eyes eastward I detected a stranger at once; it did not appear as a comet, but I knew that there ought not to be any large star in the spot occupied. It was about midway between *Alpherat* (Cor Hydræ) and Regulus; the latter, however, was not visible at the time I called to my daughter, and asked her to put her head out of the window, and she at once said, 'a comet.' I then wrote a note to the Editor of the *Cape Times* announcing it; this letter did not reach him, it would be long to explain why. . . . It is of such size and brilliancy as to be seen in the brightest sunshine. I saw it September 18 between noon and 1 p.m. Dr. Gill is reported to have said, 'the largest for 200 years.' I don't believe he said so; if so, he could not have seen the one of March, 1843."

My friend adds that he has ascertained most positively that it was not observed from the deck of either of the mail steamers *Athenian* or *Garth Castle*, then approaching the Cape. The latter carried Father Perry and the members of the Transit of Venus Expedition. "My belief is that it came within the ken of human vision on the morning of the 8th September, and not before." His station of observation, Waterhof, is about half-way up Table Mountain. J. H. LEFROY

October 19

REFERRING to my letter of the 16th, I beg to say that the R.A. of the "neighbouring object" should have been 10h. 11m., and that it was probably, not Schmidt's comet, as supposed, but the 7th or 8th mag. star 19980 Lalande, which does not appear in the B.A. Catalogue, or in the V.S. Catalogue, or in the large maps of the S.D.V.K., or on Mattly's Globe. It appeared to me of much greater magnitude than the above.

Bray, Co. Wicklow, October 21 WENTWORTH ERCK

THE magnificent comet now visible in our eastern sky shortly before sunrise is no doubt being observed in England. In case it should not I may add that its approximate position at 4h. 50m. a.m. (local mean time) this morning as determined by my equatorially mounted (4¼ inch Cooke) telescope was R.A. 10h. 55m., South declination 3° 29′. The tail by estimation is about 14°, and of unusual breadth. The borders of the tail appear much brighter than the central part. H. COLLETT

Lahore, The Punjab, India, September 25

The Proposed Bridge over the Forth

IT is no small evidence of the importance of this great undertaking, that the proposed scheme should have drawn from Sir George Biddell Airy such severe criticism as that which appeared in last week's NATURE. Coming from such a source, this criticism is sure, not only to receive the most careful consideration of those few who are sufficiently conversant with such matters to form their own opinion, but is sure to have great weight with the much larger class who accept the opinion of those they conceive best able to judge. It therefore behoves those who are responsible for this scheme, to make the best answer they can. Whether they will be able to remove altogether the impression adverse to the scheme, may well be doubted; but for my own part I do not anticipate that they will find much difficulty in meeting the objections raised, in so far as these are definite. It is not my present object to defend, or even to discuss the merits of the proposed bridge; what I wish to point out is that the knowledge of engineers as regards the theory of structures, is not so imperfect, or their methods of designing such guesswork as might be inferred from the tone of the criticism.

Sir George Biddell Airy expresses alarm lest in the design due consideration has not been paid to the "theory of buckling;" but whether this is so or not, does not appear from any circumstance to which he has referred.

To make a strut or "thrust-bar" 340 feet long to sustain a thrust of several hundred tons, is doubtless a stupendous undertaking, but so is a bridge to carry a railway over 1700 feet. There is, however, no theoretical reason against the possibility of such structures; that is to say, assuming the same strength and elastic properties of material as are experienced in existing structures, it appears by the application of the principles of mechanics that both such distributions and such quantities of material are possible as will assure the safety of these structures. Whether or not such distribution and quantities have been secured in the designs for these struts, could only be judged of after careful consideration of the proposed lateral sections in conjunction with the longitudinal section, and to these no reference whatever is made in the criticisms.

That the experienced engineers who have made themselves responsible for this design can have overlooked such an important consideration as buckling is very improbable. There is no possible accident to structures which has received more careful consideration than buckling, or of which the laws have been more definitely ascertained.

The very pretty method, given in the appendix to the communication under consideration, for obtaining the formula $W = C\frac{\pi^2}{a^2}$ is a well-known application of the theory of elasticity, and is given by Bresse.[1] But this formula is known only to apply to prismatic bars very thin, compared with their length, and is therefore of little practical use. The laws of stiffness and strength for struts of a solid section, were first deduced by Eaton Hodgkinson from his own experiments, and have since been extended to struts of any section by Lewis Gordon and Rankine.

For wrought iron, putting P for the load, S the area of section, L the length, and r the least radius of gyration of the section about any line in that section, the units being inches and lbs., the formula is—

$$\frac{P}{S} = 36{,}000 \div \left(1 + \frac{l^2}{36{,}000\, r^2}\right).$$

From this it will be seen that L must be very large compared with r before this formula assumes the same form as that which Sir George Biddell Airy has obtained.

Such general formulæ are not, however, the only or the chief guides in modern construction; sufficient actual experience has been obtained as regards such a great variety of forms for the elementary parts of structures as to furnish rules for the proportioning of every class. And although any novelty such as unprecedented size furnishes matter for grave consideration, both as regards proportions and the possibilities of art, still the theory and data for assuring reasonable safety are available, and engineers must be much behind the day if they neglect these.

Owens College, October 21 OSBORNE REYNOLDS

I HAVE read Sir George Airy's criticism of the design for the proposed Forth Bridge with interest. So far as engineers are concerned the letter calls for no reply; but as others pardonably ignorant of the present state of engineering science may feel the

[1] "Cours de Méchanique appliqué," p. 210 (1866).

same difficulties as Sir George Airy, I propose with your permission to offer a few explanations.

Sir G. Airy summarises his remarks under six heads, but I think two would have sufficed, viz. that the bridge was too big to please Sir George, and that the engineers were presumably incompetent. As to size, for example, Sir George considers the fact of the cantilever being "longer than the Cathedral by 175 feet is in itself enough to excite some fear," and even to "justify great alarm." But when I look for some justification for this bold statement I find that Sir George does not advance any reason whatever, nor make use in any way of his high mathematical attainments, but simply shifts the responsibility for this alarm on to the shoulders of the "citizens of London," asking, "would they feel themselves in perfect security? I think not; and I claim the same privilege of entertaining the sense of insecurity for the proposed Forth Bridge."

If Sir George had alleged that the stresses on the cantilever could not be calculated, or that the strength of the steel ties and struts could not be predicted, or that the cantilever could not be erected, I might have replied by publishing diagrams of stresses, results of experiments, and the names of the firms who have tendered for the work. I cannot, however, answer an argument based upon the supposed fears of the "citizens of London."

To prove that Sir George's criticisms imply a charge of incompetency on the part of the engineers, I need only point out that in one sentence he remarks that "experienced engineers must have known instances in which buildings have failed from want of consideration of buckling," and in another, that "there appears to be a fear of its occurrence in various parts of the bracket," when "the bridge will be ruined." Sir George's conclusions on this head are, however, as he fairly enough states, "made in the total absence of experiment or explanation," and in ignorance whether "a theory of buckling finds place in any of the books which treat of engineering." To assume, however, that an engineer is similarly ignorant, clearly amounts to a grave charge of incompetency. Again, how incompetent must the engineer be who required to be informed that the "horizontal action of the wind on the great projecting brackets depends not simply on the wind's pressure, but also on its leverage," or who neglected to provide for the consequent stresses. Yet Sir George does not hesitate to say, in reference to this, that "in the proposed Forth bridge there is a risk of danger of the most serious kind, which may perhaps surpass all other dangers."

As Sir George in the whole of his letter does not produce a single figure or fact in support of his very serious charges, I must, in justice to Mr. Fowler and myself, explain that it was from no want of data. At Sir George's request he was furnished with every necessary detail for ascertaining the maximum stress on each member, and the factor of safety. I stated in the paper referred to by Sir George at the commencement of his letter, that under the combined action of an impossible rolling load of 3400 tons upon one span, and a hurricane of 56 lbs. per square foot, the maximum stress upon the steel would in no case exceed 7½ tons per square inch. Any useful criticism must be directed to prove that such load is not enough or that such stress is too great. Nothing can be decided by appeals to the citizens of London.

Sir George's remarks about what he terms "buckling," and the "total absence of experiment," I can hardly reconcile with his having read my paper, because I have there devoted six pages to the question of long struts, and have given the results of the most recent experiments on flexure by myself and others. When he asks whether a tubular strut 340 feet long would be safe against buckling, he has evidently overlooked the twenty years' existence of the Saltash Bridge, which has a tubular arched iron strut 455 feet long, subject to higher stresses than are any of the steel struts in the proposed bridge. Reference is made to the fall of the roof of the Brunswick Theatre, which is attributed to buckling. This accident occurred about fifty-four years ago, and consequently considerably before my time; nevertheless I have heard of it often, and if I am not mistaken, the verdict of the jury was to the effect that the fall of the roof was due to a carpenter's shop weighing about twenty-five tons having been built on the tie-rod, which sagged under the weight, and so pulled the feet of the principals off the wall. However that may be matters little, as engineers are in possession of more recent and trustworthy data than the personal reminiscences of Sir George Airy. American bridges invariably have long struts, and consequently there is no lack of practical experience on the subject.

The late Astronomer Royal thinks that "the proposed construction is not a safe one," and hopes to see it withdrawn. When he wrote his letter it probably did not occur to him that rival railway companies might be only too glad to seize hold of anything which might prejudice the Forth Bridge project and alarm the contractors who were preparing their tenders for the work. I do not complain of Sir George's action, as it involves a matter of taste of which he is sole judge. I would only mention that when he penned the above sentence he had been furnished by the engineers with the Parliamentary evidence and other documents necessary to inform him of the following facts:—(1) That a wind pressure of 448 lbs. per square foot upon the front surface would, as stated in my paper on the Forth Bridge, be "required to upset the bridge, and under this ideal pressure, though the wind bracing would, it is true, be on the point of failing, none of the great tubes or tension members of the main girders would even be permanently deformed." (2) From the evidence given before the Tay Bridge Commissioners, Sir George, being a witness, would know that, even supposing the workmanship had been good, a wind pressure of about one-tenth of the preceding would have sufficed to destroy the Tay Bridge. (3) He would also remember, no doubt, his own report of 1873, wherein he says that "the greatest wind pressure to which a plain surface like that of the Forth Bridge will be subjected in its whole extent is 10 lbs. per square foot." (4) The Parliamentary evidence would have informed him that the proposed design was the outcome of many months' consideration by the engineers-in-chief of the companies interested, representing a joint capital of 225 millions sterling, and that it was referred to a Special Committee of the House of Commons and to a special Committee of the Board of Trade inspecting officers for examination and report, and that the reports of engineers and committees were alike unanimous in testifying to the exceptional strength and stability of the proposed bridge. As a sample of foreign opinion, I would quote that of Mr. Clarke, the eminent American engineer and contractor, who has built more big bridges himself than are to be found in the whole of this country, and who has just completed a viaduct 301 feet in height, by far the tallest in the world. Referring to the proposed bridge, he writes: "If my opinion is of any value I wish to say that a more thoroughly practical and well considered design I have never seen." I need hardly say that the opinion of such a man has far more weight than that of an army of amateurs.

Sir George Airy refers "unhesitatingly to the suspension bridge" as the construction which he should recommend. He has clearly learnt nothing on that head during the past ten years. In a report on the late Sir Thomas Bouch's design for the Forth Bridge on the suspension principle, dated April 9, 1873, he says: "I have no doubt of the perfect success of this bridge, and I should be proud to have my name associated with it." Chiefly on this recommendation, and in spite of numerous warnings from practical men, the bridge was commenced, but it had to be abandoned after spending many thousands, because having reference to the fate of the Tay Bridge, it was pronounced by the Board of Trade and every engineer of experience at home and abroad to be totally unfit to carry railway trains in safety across the Forth.

Sir George Airy stands alone in his advocacy of a suspension bridge for high speed traffic, and in his views as to the force and action of the wind on such a structure. That being so I may be permitted to say that I should have felt no little misgiving if he had approved of the substituted girder bridge, because it has been the aim of Mr. Fowler and myself to design a structure of exceptional strength and rigidity, differing in every essential respect from that with which Sir George evidently would still be proud to have his name associated.

B. BAKER

THE alarming observations in Sir George Airy's paper on the stability of the Forth Bridge as proposed by Mr. Fowler, which appeared in your last issue, seem to call for a reply, and I think I am in a position to make an unbiassed reply, as I had nothing whatever to do with the design, and moreover do not approve of it. I disapprove of the adopted system as one in which the distribution of the material can be economical only in a moderate degree, and I object to it from an æsthetic point of view, and also on account of some practical reasons of minor import, but I have no hesitation in asserting that the material may be so arranged in it—and very probably is so arranged—that the stability of the bridge when erected would equal that of the best existing structures of that class.

The paper referred to contains six points of objection, which are treated in a general way without attempting a scientific criticism. This is to be regretted considering the importance of the subject. I take each point in succession. With regard to

I. I cannot see an objection to the novelty of a system, if, as n this case, the conditions are unprecedented, and if the author of the paper himself is compelled to recommend a system of striking novelty.

II. What, may be asked, constitutes the enormity of magnitude of a structural part? Is it the excessive proportion of strain in it arising from its own weight to that arising from other weights and forces? If so, it will be found that this proportion may here be still very small, although it may not be ignored, as sometimes is done.

III. The experimental knowledge hitherto derived from structures with rising degrees of magnitude has not upset the theories used in the calculations of strength. It cannot be asserted that the top flange of a common rolled beam, being a strut, we assume twenty times as long as it is wide, would be under a test load in a safer position against buckling than the top flange of the Ohio girder bridge, which is 510 feet long and 20 feet wide, or the bottom flange of the Forth Bridge, which is 675 feet long and from 32 to 120 feet wide.

IV. We constantly rely on the strength of long struts; they exist in all girders, and many of them are of the same importance for the strength of the girders as the links for the strength of a chain. The theory of their strength, imperfect as it is, is applicable to all with a fair amount of truth, and there is no reason why it should not be applied equally to the struts in the Forth Bridge.

V. Assuming that the dangers from wind-pressure during the erection do not concern us here, it would be interesting to hear from the author which parts of the erected bridge would probably give way first, and whether this would take place by crushing, shearing, twisting, or pulling actions. The leverage offered to wind by the long brackets would come into question only when the pressure is different on the two sides of a pier. The difference would produce a twisting action, which would exist in the central pier, but which could be obviated in the two side piers. The resisting leverage of the central pier is 270 feet, or about two-thirds of the acting leverage. Approximately the same proportion obtains with regard to the stability against tilting under uniform wind-pressure, while in the case of the Tay Bridge the proportion was less than one-third.

VI. It is highly improbable that Mr. Baker should not have calculated his struts; in his book on the strength of beams, columns, and arches, he gives a very intelligible deduction of the theory of long struts, which, although elementary and not so elegant as that by the author, seems original. I have found deductions of that kind in most English text books, while in books of foreign origin generally the equation of the line of flexure is taken as the starting point. Its approximate form is—

$$-\frac{M}{EI} = \frac{1}{\rho} = \frac{d^2y}{dx^2}$$

M being the bending moment at any point, E the modulus of elasticity, I the moment of inertia of the section of the strut, and ρ the radius of curvature. The integration gives the limiting weight W acting endways upon a long strut, as already Navier stated it,

$$W = \frac{\pi^2}{a^2} EI$$

where $EI = C$ in the Paper. This formula is not applicable to short struts, since W might exceed the crushing strength of the material. The limiting weight W^1 for short columns is therefore calculated with $W^1 = fp$, where f is the sectional area and p the pressure on the sectional unit. Unfortunately there exists among theorists a difference of opinion as to the proper value of p; some put for it the crushing strength, and others the limit of elasticity, and now and then there are controversies going on about this matter. Meanwhile it is impossible to mark the limit between short and long struts which theoretically exists. Practically, however, the limit is indistinct, and Rankine, Gordon, and others, taking this into consideration, have put the two formulæ together into one empirical formula for W'', the limiting weight for struts of any given dimensions.

$$W'' = \frac{W^1}{1 + \frac{W^1}{W}}$$

This formula embellished with some empirical coefficients gives good results for struts of ordinary proportions, and as the struts in the Forth Bridge seem to have ordinary proportions, it is quite safe to use it for their calculation. M. AM ENDE

3, Westminster Chambers, Victoria Street, S.W., October 24

HAVING read with interest Sir G. Airy's article on this subject in the last number of NATURE, I am glad to see that it advocates a suspension-bridge in lieu of the proposed structure. It may perhaps interest your readers to give the particulars of the Great International Suspension Bridge over the Niagara River, which supports a carriage-way and a railway-track above.

The length of span between the towers is 800 feet. There are 4 cables, each composed of 3640 wires No. 9 = ·155" diam., without weld or joint; the cables are 10" in diameter. All the wires of each cable were separately brought into position, so that each one bears its full share of the tension. When a cable had been thus built up, it was tightly served with soft iron wire to bind the 3640 wires together, and to preserve them from rust.

Since this bridge was built, great improvements have been made in the manufacture of wire. Whereas the resistance to tensile stress at the moment of fracture of the best qualities of iron wire, such as that manufactured at Manchester for this bridge, does not much exceed 27 tons per square inch of section, *hardened and tempered steel wire* can now be made in large quantities and in long lengths with a minimum resistance at the moment of fracture of 90 tons per square inch.

Steel plates, rods, or bars cannot be made in quantity with a higher resistance than 34 tons, or less than half that of wire. Hardened and tempered steel wire similar to that used in pianos is thus clearly the most suitable material for suspension bridges, and has been recognised as such in America, where it is to be used in the construction of the New York and Brooklyn suspension bridge, the span of which is the same as the proposed Forth Bridge.

Our English railway engineers, however, have not yet recognised the great advantages wire possesses over any other form of material such as bars, chains, &c., for resisting tensile stress, and the further advantages that wire can be tested more easily and made of a more uniform quality.

Some ten years ago I called on Sir T. Bouch, the former Engineer to the Forth Bridge, to point out the advantages of a tempered steel wire suspension bridge over any other form of structure for the Forth Bridge. The idea was, however, nveer worked out on paper. WILLIAM H. JOHNSON

Manchester, October 23

On the Alterations in the Dimensions of the Magnetic Metals by the Act of Magnetisation

I HAVE read with interest Prof. Barrett's paper in NATURE, vol. xxvi. p. 585. Between his results as to the effect of magnetisation on the dimensions of bars of iron, of steel, and of nickel, and those of Sir William Thomson's experiments ("Electrodynamic Qualities of Metals," Part VII., *Phil. Trans.* R. S., Part I., 1879) on the effects of stress in the magnetisation of bars of the same metals, there exists a remarkable analogy, which, however, seems to break down in the case of cobalt. According to these experiments (which, I may mention, were carried out under Sir William Thomson's direction by my brother, Mr. Thomas Gray, and myself), the effect of the application of longitudinal pull to a bar of iron, while under the influence of inductive force tending to produce longitudinal magnetisation, is, for forces lower than a certain critical value, called from the Italian experimenter who first observed it, the Villari Critical Value, to increase, and of the removal of pull, to diminish, the inductive magnetisation. When the magnetising force exceeded the critical value, these effects changed sign, and tended to a constant value as the magnetising force was increased.

Again, the effect of transverse pull, produced by means of hydrostatic pressure in an iron tube, is, when applied, to *diminish* the longitudinal magnetisation, and when removed, to increase it. We see, then, from Joule's result, confirmed by

Prof. Barrett's, that the effect of longitudinally magnetising a bar of iron, or of increasing its magnetisation, is to increase its dimensions longitudinally and to diminish them laterally, so that the volume remains constant; and on the other hand, from Sir William Thomson's investigations, that the effect of increase of longitudinal dimensions in an iron bar is to increase, and of increase of transverse dimensions to diminish its longitudinal magnetisation.

This analogy holds also with reference to steel and nickel. In the case of bars of these metals, we found their longitudinal magnetisation to be diminished by the application of longitudinal pull, and Prof. Barrett has found that bars of the same metals undergo a shortening when their longitudinal magnetisation is increased.

In the case of cobalt, however, the results do not agree. The results for cobalt, given in Sir W. Thomson's paper, are somewhat anomalous, but they refer only to the effect of stress on magnetism in a bar which had been previously magnetised and then placed while being experimented on, under the influence of the earth's vertical force The results were therefore complicated by the effects of the stress on the residual magnetism. So far as these results go they bear out to some extent those found by Prof. Barrett, but further experiments, the results of which have not yet been published, prove that the effects of stress are the same as for nickel. This is the case at least for all but low magnetising forces.

The behaviour of cobalt and nickel throughout a wide range of magnetising forces, and under the influence of both transverse and longitudinal stress, will, it is hoped, be fully investigated in a continuation of Sir William Thomson's experiments, begun some time ago, and temporarily interrupted by other, and for the time being, more pressing work, but now about to be resumed.

I may mention that my brother and myself pointed out in NATURE, vol. xviii. p. 329, the applicability of a modification of Edison's Tasimeter to the measurement of the changes of dimensions produced in a body by magnetisation. We still think that this is perhaps the most simple method, and we have found it very sensitive for qualitative results. In our trials of it we have experienced some difficulty in obtaining a carbon button which would return after having been subjected to stress to the same resistance as before. The experiments of Prof. Mendenhall, however, show that the kind of carbon used by Edison in his Tasimeter possesses this property in perfection; and we hope soon by the use of this carbon to obtain quantitative results.

ANDREW GRAY

The Physical Laboratory, the University, Glasgow, October 19

Aurora

AN aurora was seen at Croydon at about 7 p.m. on Wednesday, the 18th inst. Three streamers of a whitish colour could be traced distinctly across the whole of the sky while the moon was still up.

A. E. EATON

The Victoria Hall Science Lectures

THE popular science lectures at the Victoria Hall have proved quite sufficiently successful, so far, to make the managers wish to continue them, provided that the kindness of competent lecturers makes it possible to do so. There have been audiences each night of ab ut 600—small compared with what the building will hold, but not amiss for a Friday night, in a neighbourhood where (except on Saturdays) people think twice before spending a penny. Those who have been present, agree in describing the audience as a peculiar one, for whom greater simplicity is needed than for the audiences of mechanics' institutes, and the frequenters of penny science lectures in general. They are quite ready to attend and to be interested, and do not think an hour too long, provided the ball is kept constantly moving, but as to this they are very exacting, and any breakdown of the apparatus, however temporary, places the success of the lecture in serious danger. There are stamps and whistles of impatience at any pause, such as must occur in adjusting experiments, but these cease the moment the lecture proper proceeds. This impatience perhaps makes the sustained interest of a lantern more suited to the audience than the more varied but intermittent experiments.

It is to be wished these lectures could be more widely known among the artisan class, who have not too many opportunities of hearing sound popular science.

ONE OF THE COMMITTEE

Royal Victoria Coffee Hall, Waterloo Road, S.E., October

THE TYPHOONS OF THE CHINESE SEAS [1]

THIS work by the learned director of the Zi-Ka-Wei Observatory, consisting of 171 pages quarto, and eight illustrative plates giving the tracks of the twenty typhoons of 1881, may be regarded as the outcome of the recent establishment of meteorological stations over the regions swept by the typhoon. The typhoons of 1880, amounting to fourteen, were described by Father Dechevrens in a previous paper. These two papers, from the greater fulness and accuracy of their details, form a contribution of considerable importance to the literature of cyclones.

An examination of the tracks of these thirty-four typhoons shows that they generally have their origin in the zone comprised between the parallels of 10° and 17°, some of them originating in the Archipelago of the Philippines, but the greater number to the eastward of these islands in the Pacific. The first part of their course is westerly and north-westerly; they then recurve about the latitude of Shanghai, and thence follow a north-easterly course over Japan. During the first half of their course the barometric gradients are steepest, and the destructive energy of typhoons is most fully developed; but after advancing on the continent, and, particularly after recurving to eastward, they rapidly increase in extent, form gradients less steep, and ultimately assume the ordinary form of the cyclones of North-Western Europe. In illustration of the steepness of the gradients sometimes formed, it is stated that on July 15 a gradient occurred of 2·760 inches per 100 miles, or one inch to 36 miles.

Typhoons do not occur during the prevalence of the north-east monsoon from November to May. In 1881 the typhoon season extended from May 22 to November 29. In Japan the true typhoon season is restricted to August and September, the storms there during the other months resembling rather the ordinary cyclones of temperate regions. The tracks of the typhoons during the months of moderate temperature, May, June, the latter half of September, October, and November, are the most southerly; they lie flattest on the parallels of latitude, and present a great concavity looking eastward; but those of the warmer months, July, August, and the beginning of September, exhibit, on the other hand, very open curves. This seasonal difference in the form of the tracks, taken in connection with the general form of the recurving tracks of the West Indian hurricanes, which are less open than those of the Chinese seas, suggests a possible connection between the forms of these curves and the different distributions of atmospheric pressure prevailing over the continents at the time.

Of the new facts brought forward in this report, the most important perhaps are those which show that the typhoon tracks have the feature of recurvation as distinctly as the hurricanes of the West Indies and the Indian Ocean. The degree of recurvation and the relative frequency with which it occurs in the tracks of the cyclones of the Chinese seas, the West Indies, the Indian Ocean near Madagascar, and the Bay of Bengal respectively, are important features in the history of these storms, which such reports will do much to elucidate. We shall look forward with the liveliest interest to Father Dechevrens' future reports, which from the lines of inquiry already indicated may be expected to throw considerable light on the influence of extensive regions of dry air and of moist air respectively, and of elevated

[1] "The Typhoons of the Chinese Seas in the Year 1881." By Marc Dechevrens, S.J., Director of the Zi-Ka-Wei Observatory, China. (Shanghai: Kelly and Walsh, 1882.)

table-lands, in determining the continuance and the direction of the course of cyclones; and the influence of isolated mountains and mountainous ridges in breaking up a cyclone into two distinct cyclones, which, from the difficulty necessarily experienced by seamen in interpreting the complex phenomena attending them, often prove so destructive in their effects.

SEISMOLOGY IN JAPAN

THE first earthquake that I ever felt took place about 2 a.m. on the night of April 10, 1876. On this night, which was soon after my arrival in Yedo, I had been installed in a new house. To be absolutely alone in a large partially furnished dwelling in a strange land, and then in the dead of night to be wakened by a swinging motion of the bedstead, a rattling of windows, creaking of timbers, and flapping of pictures was more than bewildering.

For some time after the motion had died away, which motion had several maxima and minima, some little rings upon the bedstead which had been caused to swing, kept up a gentle clicking, and a night light upon a basin of oil as it swayed from side to side cast long flickering shadows across the room. The general behaviour of things was ghostly, and it was some time before I could assure myself that what I had experienced was an earthquake.

Next morning, however, my doubts were dissipated by my neighbours making jocular inquiries about the nature of my experiences. Earthquake conversation, I may remark, is often used in Yedo to fill up the gaps in conversation, which in England are usually stopped by queries and truisms about the weather. This was my first earthquake.

Palmieri's instrument indicated that its direction was about E.S.E. to W.N.W., and its force was 6 degrees. By 6 degrees is meant that the shaking caused some mercury contained in a glass tube to wash up and down until a little string attached to an iron float on its surface had turned a pully and a pointer through 6°. By observing the tables of these indications it is seen that a very gentle shaking of long duration may get up a violent oscillation in the mercury and so indicate a shock of a great number of degrees, whilst a violent sharp shock, which might knock over a chimney, may possibly only indicate a few degrees.

Since my first earthquake I have had the opportunity during the last six years of studying rather more than 400 other shakings. One of these shook down chimneys, unroofed houses, twisted gravestones, and by its action generally entitled itself to be called destructive and alarming. The effect that this earthquake produced upon the nerves of many people was quite as great as that which might be produced upon children with an imaginary ghost. As residents in Japan are so often alarmed by earthquakes it is only natural that they should be led to study these phenomena. Amongst the first instruments which were employed for their investigation were, as might be anticipated, small columns, bowls of liquid, and other contrivances, which are found described in books and papers treating of observational seismology.

Columns which have been made of various shapes and various materials have been found unsatisfactory, because it is seldom (even when a house may be swaying violently), if they are on a stone platform *firmly* fixed to the ground that they are overturned. When it happens that they are overturned, if there were several columns side by side you would usually find them lying pointing like the arms of a star-fish in different directions. If an earthquake was a sharp blow, no doubt the columns would fall in the direction of the shock and also towards the point from which the shock came. Yedo earthquakes, however, commence gently, and the column is caused to rock before it falls, and as it rocks its plane of rocking may be gradually changed. Another explanation would be that some of the columns had fallen with direct shocks and some with reflected shocks, or that some were overturned with the normal and some with the succeeding transverse vibrations.

Bowls of liquid have been found impracticable; first, because it is seldom that in a bowl on a *firm foundation* a sufficiently measurable amount of washing up is obtained; and second, that any of the usual methods of registering the motion as well as many other methods, both chemical and mechanical which have been tried, are not satisfactory. Also there are the difficulties of freezing and evaporation to contend with.

Similarly the records of the old-fashioned ordinary pendulum with a pointer resting in sand, or, what is perhaps better, provided with a sliding pointer writing over a smoked glass plate, are also very unsatisfactory. All that many of the carefully drawn records produced by *swinging* pendulums appear to indicate, is that there has been an earthquake, and it has caused the pendulum to swing about. For reasons like these, after considerable experience the conclusion arrived at is that the records of most of the older forms of seismographs and seismometers, of which legions have been experimented with, can only be regarded as being *seismoscopic*.

When we look into the history of observational seismology, and take the following description of a seismometer invented nearly 1800 years ago as a standard of comparison between the old and better known forms of earthquake instruments for registering *ordinary* shocks, it is doubtful whether this branch of earthquake investigation has been much advanced. This description was given to me by Mr. J. Hattori, vice-director of the Imperial University. It was translated for me by my assistant, Mr. M. Kuwabara. It runs as follows:—

In a Chinese history called "Gokanjo," we find the following: "In the first year of Yōka (A.D. 136) a Chinese called Chioko invented a seismometer. This instrument consists of a spherically formed copper vessel (Fig. 1), its diameter being 8 'shaku.' It is covered at its top. Its form resembles a wine bottle. Its outer part is ornamented with the figures of different kinds of birds and animals and old peculiar looking letters. In the inner part of this instrument a pillar is so placed that it can move in eight directions. Also in the inside of this bottle there is an arrangement by which some record of an earthquake is made according to the movement of the pillar. On the outside of the bottle there are eight dragon heads, each of which contains a ball. Underneath these heads there are eight frogs, so placed that they appear to watch the dragon's face, so that they are ready to receive the ball if it should be dropped. All the arrangements which cause the pillars when it moves to knock the ball out of the dragon's mouth are well hidden in the bottle. When an earthquake occurs and the bottle is shaken, the dragon instantly drops the ball, and the frog which receives it vibrates vigorously. Any one watching this instrument can easily observe earthquakes. With this arrangement, although one dragon may drop a ball, it is not necessary for the other seven dragons to drop their balls unless the movement has been in all directions; thus one can easily tell the direction of an earthquake. Once upon a time a dragon dropped its ball without any earthquake, and the people therefore thought that this instrument was of no use, but after two or three days a notice came saying that an earthquake had taken place in Rōsei. Hearing of this, those who did not believe about the use of this instrument began to believe in it again. After this ingenious instrument had been invented by Chiokō, the Chinese Government wisely appointed a secretary to make observations on earthquakes."

We have here I think not only an account of an earthquake instrument which in principle is identical with many of our modern inventions, but the science has been conjoined with art. The record of the Chinese Government establishing a seismological bureau at a time when America was unknown, and half of Western Europe were living in the woods, is also interesting. Experience having taught us that the older instruments told us so little about the *actual* movements which were going on at the time of an earthquake, a large number of instruments to replace them have been gradually invented. Of these I may mention the three following types.

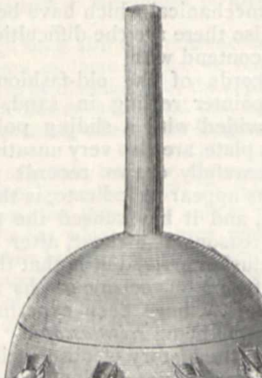


FIG. 1.

1. Pendulums, so far controlled by friction as to be "dead-beat," provided with an index which magnifies the earth's motion. The general construction of one of these instruments will be understood from Fig. 2. B B is a heavy lead ring, used as a pendulum; *p* a sliding pointer loaded with lead (so that it may give sufficient

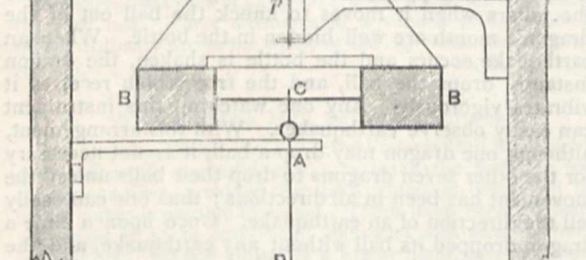


FIG. 2.

friction), resting on a glass plate on the pendulum. This pointer is carried by an arm attached to the side of the box containing the pendulum. Its object is to control the swinging of the pendulum. P the index, axled near the centre of gravity, C, of the pendulum, and again a short distance below at A in an arm attached to the side of the containing box. At the lower end of this index there is a sliding needle, N, to write on a moving or stationary glass plate. The magnification of the actual motion of the earth in this instrument is as C A : A N.

2. *Bracket Machines* (see Fig. 3). B is a heavy weight pivoted at the end of a small bracket, C A K, which bracket is free to turn on a knife edge K above, and a pivot A below, in the stand S. At the time of an earthquake B remains steady, and the index P forming a continuation of the bracket, magnifies the motion of the stand, which is to that of the earth in the ratio of A C : C N. These instruments are used in pairs, the brackets in each being placed at right angles to each other.

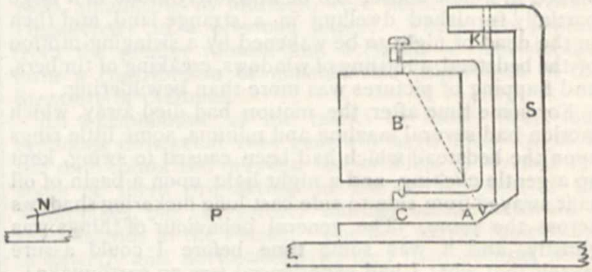


FIG. 3.

3. *Rolling Spheres* (Fig. 4). S is a segment of a large sphere with a centre near C. Slightly below this centre a heavy weight, B, which may be a lead ring, is pivoted. At the time of an earthquake C is steady, and the earth's motions are magnified by the pointer C A N in the proportion of C A : A N. The working of this pointer or index is similar to that of the pointer in the pendulum.

The indices of all these instruments, of which there are many modifications, are allowed to write on smoked glass plates, which at the time of an earthquake are either being moved by clockwork or else are stationary. For *vertical motion*, sunken buoys, the water in a can with a flexible bottom, and a weight suspended at the end of the long

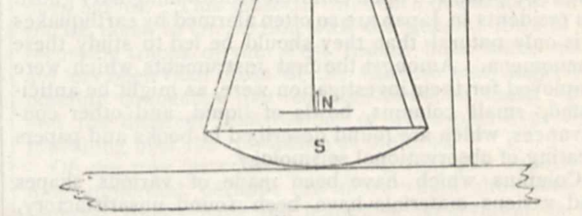


FIG. 4.

arm of a lever and stretching a stiff spring, have all been used with more or less success as steady points in the recording of vertical motion.

For the invention of the greater number of these instruments, which I may remark have already done very much in writing down actual earthquake motion, we are indebted to Mr. T. Gray. Messrs. Ewing, Chaplin, Wagner, and other members of the Seismological Society of Japan, have also made valuable contributions to this part of the subject.

Some of the more important results which have been arrived at by the use of these and other instruments are :—

1. That the earthquakes in Tokio usually commence gently, the motion is irregular, both as regards space and time, increasing and decreasing. Finally it dies away as it commenced.

2. There are usually from two to three vibrations per second. Occasionally there may be six or eight.

3. The maximum amplitude of an earth particle is seldom over one millimetre, although buildings may swing through several inches. When the amplitude is four or five millimetres, and the motion rapid, the shock may be dangerous.

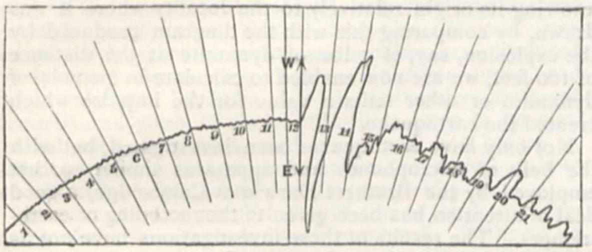


Fig. 5.

4. During a given shock the direction of motion may change, apparently showing the presence of normal and transverse vibrations.

5. The motion of the ground *inwards* towards the origin of the disturbance has in certain cases been much greater than the motion outwards. In this respect the diagram obtained from an actual earthquake closely resembles the diagram obtained when we explode a charge of dynamite in a bore hole.

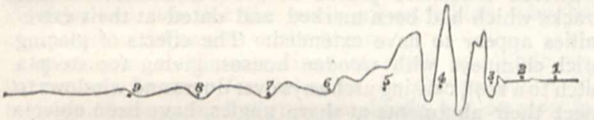


Fig. 6.

6. The velocity, and with it the acceleration for the inward motion, is usually much greater than it is for the outward motion.

In addition to these characters, earthquake motion has others which are more complex, and are now forming a subject of examination. Thus, for instance, experiment apparently indicates that two neighbouring points of ground (say at the distance of two feet from each other) do not synchronise in their motion. This would indicate that a building, although it may be small, may not be

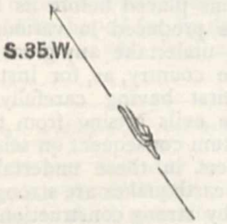


Fig. 7.

moved back and forth as a whole, but may suffer considerable racking.

The intervals in time between the actual earthquakes, which on the average occur from six to ten times per month, have been filled up with experiments upon artificially produced earthquakes, made by exploding charges of dynamite and gunpowder in bore holes. These experiments, in which the vibrations of the ground produced by the explosions have been simultaneously written down at a number of different stations, have perhaps been more instructive than the actual earthquakes. They have been to seismology what laboratory experiments on magnetism have been to the student of the magnetic phenomena of the earth. Not only have results similar to those which have been enumerated for actual earthquakes been obtained, but also many others. Thus it is found that in the alluvium of the Tokio plain a surface *wave* is produced, as might be inferred from the fact that the observation of the horizontal and vertical components of the motion of the ground, do *not* enable us to deduce angles of emergence for the shock and the depth of its origin. Normal and transversal vibrations have been clearly separated. The effect produced by inequalities in the surface of the ground in cutting off the propagation of vibrations have been studied. *Small* hills appear to produce but a slight

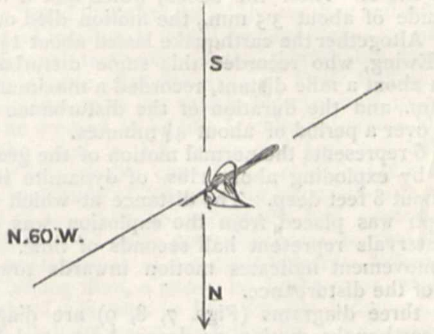


Fig. 8.

effect, whilst cuttings (like a deep pond) are more or less effective in interrupting a disturbance.

By the comparison of a number of diagrams of earth vibrations, taken simultaneously at different stations, it has been an easy matter to investigate the relative amplitudes of different vibrations. Near to the origin of a disturbance the amplitude of the normal vibrations was found to be greater than that of the transversal ones, but the former, as they progressed outwards, died out more quickly than the latter. The diminution in the period of these vibrations as they died out at a single station, or as they died out by propagation to distant stations, was also a matter of considerable importance. In connection with this subject it does not seem impossible that when we have a large earthquake, say like that of Lisbon in 1755,

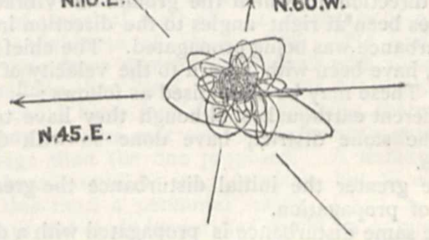


Fig. 9.

that the vibrational period of the disturbance may gradually be so far reduced, although its amplitude may be great, that inhabitants in distant countries may be moved backwards so slowly that the only indication of the motion would be a slow rising and falling in the waters of their lakes and ponds.

May not certain disturbances of this nature, like the *seiches* of Switzerland, usually attributed to variations in atmospheric pressure, be sometimes caused by slow oscillations of this description?

Sir William Thomson has suggested that Mr. George Darwin should employ the same reasoning to discuss these phenomena as that which he has so well employed to discuss the elastic yielding of the earth in connection with the rise and fall of tides. Sudden alterations in barometrical pressure may possibly produce earthquakes of large amplitude and slow period, similar to those here referred to, which hitherto have been passed by unnoticed. The following are examples of the various records which have been referred to.

The diagram (Fig. 5) is a tracing from a photograph of the east and west component of the earthquake of March 11, 1882, as recorded in Tokio. The regularly marked intervals represent seconds of time. For about 12 seconds before the shock there was a rapid tremulous motion. It will be observed that the westward motion of the shock is less in amplitude, and has been performed more slowly than the eastward motion. The origin of the shock was to the S.S.E. After the shock, which had a maximum amplitude of about 3·5 mm., the motion died out irregularly. Altogether the earthquake lasted about 1½ minutes. Prof. Ewing, who recorded this same disturbance at a station about a mile distant, recorded a maximum motion of 6 mm., and the duration of the disturbance could be traced over a period of about 4½ minutes.

Fig. 6 represents the normal motion of the ground produced by exploding about 2 lbs. of dynamite in a bore hole about 8 feet deep. The distance at which the seismograph was placed from the explosion was 100 feet. The intervals represent half seconds of time. The upward movement indicates motion inwards towards the origin of the disturbance.

The three diagrams (Figs. 7, 8, 9) are diagrams of actual earthquake motion, as drawn by a pendulum seismograph on stationary smoked glass plates.

Fig. 7 an earthquake at Chiba (16 miles E. of Tokio), 11.49.0 p.m., February 16, 1882. Here the motion has been simply in *one* direction, S. 35° W. Its extent is about ·9 mm.

Fig. 8 an earthquake at Chiba, December 23, 1881. Here the motion has been in at least *two* directions, N. and S., and N. 60° W. The maximum amplitude is about 1 mm.

Fig. 9 an earthquake at Tokio, 4.15.0 p.m., March 8, 1882. Here there has been motion in *several* directions. The maximum amplitude is about 2·2 mm.

Another class of seismic experiments which, although they are by no means sufficiently complete have yielded good results, are those in which time observations have formed the important features. One result of these experiments, in addition to telling us the side and locality from which earthquakes have come, has been to show that the direction in which the ground has vibrated has sometimes been at right angles to the direction in which the disturbance was being propagated. The chief results, however, have been with regard to the velocity of propagation. These may be epitomised as follows:—

1. Different earthquakes, although they have travelled across the same district, have done so with different velocities.

2. The greater the initial disturbance the greater the velocity of propagation.

3. The same disturbance is propagated with a decreasing velocity.

These results it may be remarked have received direct confirmation, both for normal and transverse motions, in experiments made by exploding charges of dynamite.

Another point which has received considerable attention has been the production of what are apparently earth currents at the time of an artificial disturbance.

A problem of local interest which has been worked at for some years has been the localisation of the origins of the shocks which from time to time disturb the eastern shores of Japan. The result of these labours has shown that the greater number of shocks have originated beneath the sea, off a coast which shows clear evidence of recent and rapid elevation.

A phenomenon which has clearly been illustrated in these investigations has been the very rapid manner in which heavy mountain ranges have completely prevented the spread of a disturbance.

By placing a large number of similar seismographs on the hills and in the valleys of a limited area, it has been quantitatively demonstrated that we may have two localities within a quarter of a mile of each other, one of which will experience at least double the amount of disturbance as the other. In some localities the hills appears to be the most affected, at others the plains are the troubled regions.

Having before us the diagram of an earthquake, and knowing its origin relatively to the locality where it was drawn, by comparing this with the diagram produced by the explosion, say, of 5 lbs. of dynamite at the distance of 100 feet, we are now enabled to calculate in pounds of dynamite or other units a value for the impulse which created the earthquake.

Not only have earthquakes been investigated, but with the help of microphones and apparatus similar to that employed by the Brothers Darwin at Cambridge, a good deal of attention has been given to the recording of earth-tremors. The results of these investigations have not as yet been sufficient to enable us to form any general laws such as those which have been formulated by Prof. Rossi and other workers in the Italian Peninsula.

A utilitarian branch of seismology has been a study of the effects which have been produced upon buildings. Walls with openings in them, which run parallel to the direction in which there is the greatest motion, appear to be more cracked than those at right angles to such directions. At the time of an earthquake existing cracks in a building have been found to open and shut. Records of these motions have been obtained by means of specially contrived indicators placed across the cracks. Other cracks which had been marked and dated at their extremities appear to have extended. The effects of placing brick chimneys with wooden houses, giving too steep a pitch to a roof, causing archways over doors and windows to meet their abutments at sharp angles, have been objects of observation. The difference in the effects produced upon buildings like those of the Japanese, which simply rest upon the surface of the ground, and those which by means of foundations are firmly attached to the soil, have yielded instructive lessons. In short it would seem that for earthquake countries the rules and formulæ used by engineers and architects require considerable modification. In England the principal elements which enter into consideration, are stresses and strains produced by gravity acting vertically. In an earthquake country we have in addition sudden stresses and strains arising from forces applied more or less horizontally.

After the lessons placed before us by the ruins which earthquakes have produced in various portions of the globe, should we undertake any great engineering work in an earthquake country, as, for instance, the Panama Canal, without first having carefully considered how best to avoid the evils arising from the sudden acquisition of momentum consequent on seismic disturbances, is for shareholders in these undertakings a financial suicide. Because earthquakes are strong the usual method to meet them is by strong construction. Still very much more than this may be done. And if we cannot prevent the destructive effects of earthquakes, observation in Japan has shown that we can at least mitigate them. This is testified by the modification in the style of buildings now adopted in Japan by all who suffered by the earthquake of February 22, 1880.

The observation of earthquakes in Japan has therefore led to results which are utilitarian as well as scientific. The description which has here been given of the work which is going on in that country is short and imperfect, many branches of seismological investigation which have been taken up has not even been referred to.

In conclusion I must draw attention to the excellent opportunities which many of those residing in Britain have for the observation of artificially produced earth tremors. By these I mean the vibrations which are produced by our railway trains, our carriages, explosions at mines and quarries, steam hammers, the falling of balls used in the breaking of castings, and other means. All of these vibrations I can state with confidence are capable of being graphically recorded, and the value of a series of such records to the practical seismologist it is hardly necessary to dilate upon.

Investigations of this description are the laboratory work of the practical seismologist, and often lead to more valuable results than those which are obtained from actual earthquakes. Actual earthquakes are produced by unknown causes, they come at unknown times, and from unknown localities. With artificially produced disturbances none of these difficulties have to be contended with, the cause and the result are before us simultaneously, and we are enabled to arrive at laws which actual earthquakes would never yield to us.

Another point to which I should like to draw attention is the study of earth movements in general. Hitherto we have only devoted our attention to the sudden and violent movements which we call earthquakes. In addition to these we have in nature movements of less amplitude called *earth tremors*. Inasmuch as we now know that these are probably a universal phenomenon, and at the same time are in every probability governed by laws simpler than those which govern earthquakes which are usually due to a complexity of causes, it certainly devolves upon us to establish the necessary means for their investigation. From the little we have learnt about earth tremors it is not unlikely that they may be to our continents what tides are to our oceans, phenomena which are regular and law abiding, and not like the earthquakes, which may be compared to the storms of the ocean.

In addition to these motions of small amplitude we have many reasons for believing in the existence of motions of our ground of great amplitude, but so slow in period that hitherto they have been overlooked.

In order to complete the study of earth motions we have to add to seismology the study of earth tremors and what might be called earth pulsations.

As we have done so much for our skies, for our atmosphere, and for our waters, we can surely do a little towards the investigation of the movements of the earth on which we live.

Although these latter remarks have no direct connection with the work which has been accomplished in Japan, they are nevertheless an outcome of such work, and if they tend in any way to draw attention to a much neglected study, an object will have been attained greater than any which could be hoped for by diffusing a knowledge of the labours of those who dwell at our Antipodes.

JOHN MILNE

THE LATE PROFESSOR BALFOUR

THE meeting held last Saturday to establish a memorial to the late Prof. Balfour was very largely attended by all grades in the University, and among non-residents by Professors Huxley, H. J. S. Smith, A. W. Williamson, W. K. Parker, Ray Lankester, H. N. Moseley, and A. M. Marshall and Mr. George Griffith, of Harrow. The president of the Royal Society would have been present but for his recent accident. The speakers, including most of those mentioned above, and Professors Paget, Humphry, Newton, and Westcott, bore unanimous testimony to the high regard and affection in which the lamented professor was held, to the original work he had accomplished, and the high promise of his life, and to the energy and success of his teaching. Dr. Paget referred to Balfour's having abandoned his favourite pastime of deer-stalking in order not to inflict unnecessary suffering upon harmless animals, and his having taken up instead that of Alpine climbing, in which he met his death. Any memorial to him would, he hoped, do something to perpetuate the spirit in which his scientific achievements had been accomplished, which placed him beside such men as Miller, Sedgwick, and Clerk Maxwell. Mr. A. Sedgwick, late demonstrator with Balfour, spoke of the growth of the class in seven years from ten to ninety students, and of the crushing nature of his loss to the school he had attracted around him, for his personal intercourse and counsel was of the extremest value.

Prof. Huxley, in proposing "That the memorial take the form of a fund, to be called the Balfour Fund, for the promotion of research in biology, especially animal morphology," said that after the addresses they had listened to with painful interest, it would be superfluous for him to add his personal testimony to the remarkable sagacity and the remarkable characteristics of Prof. Balfour. It was no exaggeration to say that in his eyes and to many of his age he seemed to be like Lycidas, of whom Milton wrote, "who died in his prime and hath not left his peer." The remarkable capacity he exhibited was developed by a multitude of surrounding circumstances. He was happy to say that he personally had contributed, amid a multitude of more powerful forces, to that which led to the development of his great powers. When Balfour was a young man, a paper he had written while at Harrow School was sent to him for his judgment, and again when Balfour was a candidate for a Fellowship at Trinity College, he was one of the examiners. "Amid many of my faults and failings during a long life," said Prof. Huxley, "I do not reproach myself for neglecting to recognise the capacity of the friend we have lost, both in the paper written while at Harrow and during the examination for the Fellowship at Trinity." He would draw attention to two words in the resolution he proposed—viz., research and morphology. The late Professor distinguished himself in both these directions. In former days men were content with being learned, but now we must not only know what is known, but help to extend the bounds of knowledge. This Balfour did. and his title as an eminent researcher was undoubted. With regard to morphology, it was a science until recently only known as a field of wide speculation of German philosophers. It was now a new learning, a great system of doctrine, established by an enormous mass of carefully co-ordinated facts. Three things were requisite to develop this new science:—1. Mastery of practical methods. 2. Accuracy of observation. 3. Vividness of imagination. He had never met any one more marvellously gifted with these three great qualities than Prof. Balfour. If his unshrinking modesty could have foretold this meeting, there would not be any form of memorial more entirely grateful to his feelings than the one proposed. A monument in stone or brass would be inappropriate; but to establish through this fund a perennial spring of activity in the direction of the study he pursued would be a more lasting and perfect memorial. And they might think of him in the concluding words of "Lycidas":—"Henceforth thou art the genius of the shore, In thy large recompense, and shalt be good To all that wander in that perilous flood"

Prof. Newton proposed "That the proceeds of the fund be applied (1) to establish a studentship, the holder of which shall devote himself to original research in biology, especially animal morphology; (2) to further, by occasional grants of money, original research in the same subject." He said that the room and the building in which they were assembled reminded him that he felt and entered for the first time into the full meaning of the Poet Laureate's words—"But O! for the touch of a vanish'd hand And the sound of a voice that is still." With reference to an allusion of Mr. Sedgwick, he said he should always consider it to be one of the brightest episodes of his career that, having found in Balfour a young man capable of giving instruction, he had afforded him facilities. The late Professor was not only an instructor but a student, and no one ever remained so much a student."

Prof. Williamson proposed "That a committee be appointed to collect subscriptions and to draw up conditions under which, with the sanction of the subscribers at a future meeting, the fund shall be offered to the University."

Dr. Michael Foster proposed the following resolution :—"That the Committee be instructed—(1) that the value of the studentship be not less than 200*l*. a year ; (2) that while it is desirable that the studentship should be in some way closely connected with this University, persons other than members of this University shall be eligible to it ; (3) that it be not given away by competitive examination ; (4) that in framing regulations both for the conduct of the student and the award of occasional grants, the primary object of the fund—namely, the furtherance of original research, be closely adhered to." He said he thought the above instructions to the Committee did not need any defence. The object of the memorial was not to keep Prof. Balfour's memory alive, for his works would do that, but to connect his name with some useful thing. The idea in fixing the value of the studentship at 200*l*. was that such a sum would be just sufficient to attract men led by enthusiasm to turn their attention to research, while it would be insufficient to induce persons to accept it as a competency. He thought it right not to restrict the studentship to members of the University, for they desired to attract talent from all parts of the country, while he considered that it was a proper condition not to throw it open to a competitive examination, for the studentship was not intended as a reward for past work or an acknowledgment of merit, but to encourage men of promise to undertake research.

An influential committee was appointed to collect subscriptions and draw up detailed conditions under which, after a future meeting of subscribers, the fund may be offered to the University. Mr. J. W. Clark, M.A., 1, Scroop Terrace, and Mr. A. Sedgwick, M.A., Trinity College, Cambridge, were appointed secretaries of the committee, the former to act as treasurer. The fund starts well, with the munificent contribution of 3000*l*. from the family of the late Prof. Balfour, and to 1000*l*. which had been left by the deceased to Dr. Michael Foster to be applied according to his discretion for the promotion of biology; nearly 1000*l*. was subscribed in the room or shortly afterwards.

DR. THWAITES

GEORGE HENRY KENDRICK THWAITES, whose death was recorded in a recent number of NATURE, was a well-known member of the older generation of British botanists. I do not know the exact date or place of his birth, but suppose it to have been in 1811. In his early life he followed the profession of Notary Public at Bristol, and apparently had a hard struggle to support and educate numerous younger brothers and sisters. He had a natural passion for botanical studies which he cultivated to such good purpose as to obtain the appointment of Lecturer on Botany and Vegetable Physiology at the School of Medicine at Bristol. He was no less ardent as an entomologist, and throughout his life collected assiduously; some of his earliest papers are on entomological subjects. His principal published work has, however, always been botanical. Till he left England he was mostly occupied with microscopical investigations, and what he published of these were like all that he did later—excellent specimens of careful and intelligent observation. His paper "On the Cell-membrane of plants" (1846) which established many interesting and at that time novel points, received a good deal of attention. Amongst other things it apparently gave the first accurate interpretation of the mucous investment of the cells of many *Palmelleæ*, *Nostochineæ*, and *Diatomaceæ;* Thwaites was able to show clearly that this was the product of the gelatinisation of the cell-walls. His capital discovery, however, was that of Conjugation in the *Diatomaceæ*. This he observed in *Eunotia turgida*, and the paper describing it bears the date May 11, 1847, and was published in the *Annals and Magazine of Natural History*. It was, as Thwaites himself remarked, "a discovery which is valuable as proving that a relationship of affinity as well as of analogy exists between the *Diatomaceæ* and the *Desmideæ* and *Conjugatæ*, and will help to settle the question as to whether the former are to be referred to the animal or the vegetable kingdom." I have been told nevertheless that when this important discovery was communicated to the British Association at Oxford, it was received with but little attention.

The present director of the Royal Gardens, Kew, then Dr. Hooker, was about this time attached to the Geological Survey. At the instance of Sir Henry de la Bêche he was engaged in the Bristol Coal Field, endeavouring to ascertain whether any definite relation could be traced between the superficial flora and the underlying rocks. This brought him in contact with Dr. Thwaites, who was, notwithstanding his professional pursuits, in the habit of spending the early hours of the morning in teaching himself the practical details of gardening in the Durdham Down nurseries. It was probably this circumstance which brought under his notice the curious instance of hybridity in a Fuchsia, which so much excited the interest of Mr. Darwin, and has often been referred to. A seed of *F. coccinea* fertilised by *F. fulgens* contained two embryos. These were extremely different in appearance and character, though both resembled other hybrids of the same parentage produced at the same time. What was still more remarkable, was that they were closely coherent below the two pairs of cotyledon-leaves into a single cylindrical stem.

In 1847 Thwaites was an unsuccessful candidate for a chair of botany in one of the newly founded Queen's Colleges in Ireland. His combined scientific and practical knowledge, however, indicated his fitness for a botanical post, and on the death of Dr. Gardner, he was appointed in June, 1849, Director of the Royal Botanic Garden, Peradeniya, Ceylon, on the recommendation of Sir William Hooker. He never returned to this country, and from the first threw himself into the duties of his post with great fervour; under his management Peradeniya became perhaps the most beautiful tropical garden in the world. He continued the labours of his predecessors in investigating the very peculiar flora of the island with great success, and, between the years 1858-64, issued, in parts, the "Enumeratio plantarum Zeylaniæ." This was at the time of its publication the first complete account on modern lines of any definitely-circumscribed tropical flora. The want of affinity which the flora thus worked out was seen to have to the general vegetation of the contiguous peninsula of Hindostan and its marked relationship to that of the Malayan region established facts of the greatest significance in the study of geographical distribution. A passage from the preface (1864) is worth quoting, as showing that Thwaites was one of the earliest English naturalists to give his adhesion to the Darwinian theory :—

"These forms or varieties would probably be viewed by some botanists in the light of distinct, though closely-allied species, and they occupy, in fact, that debatable ground the difficulties and perplexities of which the practical naturalist alone knows, and which in the opinion of many (and I may include myself amongst the number) are only to be got rid of by the adoption of the views enunciated by Mr. Darwin as regards the relationship of allied forms or species by descent from a common ancestor."

Besides the "Enumeratio," Thwaites published subsequently a few papers on detailed points in Ceylon botany.

His tenure of office was associated with some of the most important developments of the Ceylon planting industry. In 1861 and subsequent years he took an active part in the operations undertaken by the Government of India, in concert with the Royal Gardens, Kew, for the introduction of Cinchona into the East. From the first the enterprise succeeded in Ceylon beyond expectation, and in 1869 the first ton of bark grown in the island was sent to England for sale. In 1864 he began to urge the cultivation of tea, and in 1868 a sample, manufactured in Ceylon, was sent to this country. Cocoa was similarly brought forward in 1867, and it now bids fair to be one of the most important of Ceylon staples. Liberian coffee was introduced from Kew in 1873. In 1876 the plants of Para, Ceara, and Central American india-rubber plants, obtained for the Indian Government, were sent from Kew, where they had been propagated, to Dr. Thwaites' charge in Ceylon, which was made the depôt, for their subsequent distribution to India.

During the later years of his life Dr. Thwaites had been in weakly health, and lived latterly a retired and extremely abstemious life. But his singularly refined and cultivated mind always gave him a position of distinction in Ceylon society, and he enjoyed the esteem and personal friendship of successive governors. He became a Fellow of the Linnean Society in 1854, and of the Royal Society in 1865; and in 1878 the Crown conferred upon him the Companionship of St. Michael and St. George, in recognition of his long services. Two years afterwards he retired, and took up his abode near Kandy, being unable to persuade himself to leave the island where so much of his life had been continuously spent. He died on September 11, and was followed to the grave on the following day by a large assemblage and the members of the Peradeniya Garden Staff, including the coolie labourers.

W. T. T. D.

ELEVATION OF THE SIERRA MADRE MOUNTAINS

DURING the past summer, in travelling across the Sierra Madre Mountains from Parral in the southern part of the State of Chihuahua, Mexico, to the mining town of Guadalupe y Calvo, on the Pacific slope about one hundred and fifty miles from the Gulf of California, some observations were taken with a small pocket aneroid barometer with thermometer attached, which may be of interest to the readers of NATURE. Both barometer and thermometer had been carefully compared with the standard instruments in Vanderbilt University and the proper corrections made.

Starting from Parral, or Hidalgo as it is generally named on the maps, the road leads in a south-westwardly direction to the small mining town of Santa Barbara, at the foot of the Sierra Madre range. From this point there is no road, but merely a trail running westwardly through the small villages of Providentia, Cerro Prieta, and Piedra Larga—the two former in Durango—to the old mining town of Guadalupe y Calvo, a distance of about eighty Mexican leagues or two hundred English miles. The journey can only be made on mules, or horses accustomed to mountain travel, as there are no roads, and the trail passes over several precipitous mountains. The distances, as near as could be ascertained, are about as follows:—

	Leagues.
Parral to Santa Barbara	7
Santa Barbara to Providentia	7
Providentia to Cerro Prieta	18
Cerro Prieta to Piedra Larga	26
Piedra Larga to Guadalupe y Calvo	22

The heights going westward as determined by the barometer at the several stations mentioned, are as follows:—

	Feet.
Parral	5,880
Santa Barbara	6,490
1st Mountain	8,670
Providentia	6,850
2nd Mountain	10,220
Cerro Prieta	6,720
3rd Mountain	8,760
Cave	9,270
Valley of Rio Verde	9,110
4th Mountain	9,440
5th „	9,350
Piedra Larga	8,010
6th Mountain	9,470
7th „	9,260
Guadalupe y Calvo	7,500

The temperature in the mountains—July 10 to 31—ranged from 58° to 85°. During five days in Guadalupe y Calvo—July 20 to 25—the temperature was taken at 6 a.m., 12 a.m., and 6 p.m., and found to range from 59° to 68°. On two days—July 21 and 22—it was 65° at the time of each observation. The rainy season begins about the middle of June and extends to the 1st or middle of September. The amount of rain that falls increases towards the west. The mountains run generally S.S.E. and N.N.W., and are covered with fine timber, consisting mainly of yellow pine.

Outside of the villages mentioned there are no inhabitants except a few Indians, descendants of the Aztecs, who live chiefly in caves and cultivate small patches of corn, beans, and pepper, and have small herds of cattle. These Indians are peaceable. The Apaches once roamed through these mountains, but of late years their depredations are confined to Middle and Northern Chihuahua and Sonova.

N. T. LUPTON

Vanderbilt University, Nashville, Tenn., October 3

NOTES

MR. M. A. LAWSON, M.A., F.L.S., having been appointed Superintendent of the Government Cinchona Plantations (Madras), the Professorship of Botany at Oxford will shortly be vacant.

ALTHOUGH they have M. Cochery as their common president, the two Electrical Congresses now sitting in Paris have separate sittings, as well as separate ends. The greater number of Governments have appointed separate delegates for each. The programme for the Congress on Electric Units was already published at the end of the session of the Congress of Electricians, and adopted by them. The consequence is that the committees were established beforehand, and that some Governments, as Belgium and Italy, appointed special delegates for each committee. The total number of delegates is sixty-two. The German Empire, having the exclusive right of representing the central Government in foreign parts, no delegate has been appointed either by Bavaria or Saxony; but amongst the five German delegates we find the name of Dr. Kohlrausch, Professor at the Bavarian University of Wurzburg. After having appointed M. Cochery as president, the Congress appointed a secretariat composed of two French officials; four others, belonging to the French Administration, have been appointed as *secrétaires rédacteurs*. The records of the Congress will be published under their authority. The members of each of the several committees have appointed their president or a president and secretary, and will communicate the results of their work at general meetings. It is probable that scientific committees will be established, and that the Congress will dissolve after having appointed them, or possibly adjourn to a future occasion. The same routine will be followed as on the occasion of the Congress of Electricians, of which this Congress must be considered as the sequel. The funds for conducting the experiments have been voted, as we reported, by the French Houses of Parliament, to the extent of 90,000 francs, but practically to any amount.

THE Congress on Cable Protection may be said to have been established in furtherance of a deliberation taken unanimously by the Congress of Electricians, but this deliberation was not acknowledged as binding by the French Government, and was not proclaimed at the time. The consequence is that the French Foreign Office caused a special programme to be drawn out after having consulted the Postal Telegraph Office and foreign governments. The schedule for the direction of the deliberation is divided into three parts:—1. The protection of cables. 2. The protection of shipping laying cables. 3. The right of property in the bottom of the sea, and rules for laying cross or parallel lines and repairing them. Although a certain number of delegates sit in both Congresses, the majority of them belong to the legal or maritime profession. The two Congresses will hold general meetings this week, and at the end of each the Minister of Postal Telegraphy will hold a levée at his official residence. The names of the British delegates are the following:—Sir William Thomson, Prof. Carey Forster, Lord Rayleigh, Prof. Fleeming Jenkin, Dr. Hopkinson, F.R.S. Cable Protection:—Mr. Kennedy, Mr. Patley, Mr. Trevor; Mr. Farnall of the Foreign Office will act as the Secretary of the British delegation.

IN a postscript to Mr. G. B. Bennett's letter to Sir J. H. Lefroy on the comet (see p. 623), the writer says:—"Since closing my letter, I have been informed that Miss North has left Wynberg for the interior. She is bent on depicting the Welwitschia. She will have to go into the Damara country to find one. I can scarcely believe that she has gone to such a distance."

A DESPATCH from Montreal, dated October 9, states that the Montreal City Council has been officially notified by the Secretary of State of the Dominion that the British Association will meet there in 1884, and has been asked that due arrangements be made.

THE French Minister of Public Instruction presided on October 11 at the inaugural sitting of a new commission created by M. Ferry, to determine the best measures for the hygiene of school children. The number of members of this commission is forty-five, amongst whom are eight females, either professional teachers or connected with the efforts made recently for promoting female education in France.

ON October 10 the Swiss Universities celebrated the fiftieth anniversary of the beginning of the scientific career of Dr. Valentin. The five Swiss Universities and no less than twenty foreign ones sent him diplomas of honour and congratulations. The health of the eminent physiologist is, however, so bad that he was confined to bed, at Geneva.

THE first provincial dinner of the Institute of Chemistry was held at the Great Western Hotel, Birmingham, on Friday, the 20th inst., and was numerously attended, both by members residing in and around this busy centre of chemical industry and by members from London. In replying to the toast of the evening, "Success to the Institute," the President, Prof. Abel, briefly traced its history. It was established to supply the want, keenly felt by the chemical profession, of an organisation to protect their interests. Its fundamental objects were the promotion of a thorough study of chemistry and the adoption of whatever measures might be necessary to advance the interests of the profession. A suitable course of training had been laid down after careful consideration and the attainment of the grade of Member and Associate was gradually coming to be regarded as a proof of fitness for election to technical, professional, or official appointments. On Friday and Saturday the members visited Earl Dudley's Iron Works, Messrs. Chance's Alkali and Glass Works, the Mint, Gas Works, &c.

MR. THOMAS COATS, of Ferguslie, Paisley, has handed over to the keeping of the Paisley Philosophical Institute an observatory erected on Oakshaw Hill, the total value of the gift being 12,000*l.*

A SUBSCRIPTION towards the English Darwin memorial has been opened in Stockholm. The Swedish press warmly supports the same, pointing out that it is not money which England asks for, but the tribute of a cultivated nation to one of her greatest *savants*.

A LETTER dated September 22 has been received by the promoter of the Danish Polar Expedition, Herr Gamel, from Lieut. Hovgaard, in which he states that the *Dijmphna* is frozen in near Novaya Zembla, but he hoped to get free during the equinoctial gales and reach the Jenisei. All was well on board.

THE Captain-General of the Philippine Islands telegraphs from Manila, October 21, that a tremendous hurricane had almost entirely destroyed that town. In less than an hour from its commencement not a single native house and not a single wooden house was left standing. Almost all the stone buildings, even those having iron rafters, were unroofed and made uninhabitable. Comparatively few casualties had taken place among the population. In a later telegram the Captain-General says that the authorities of Balacan and the interior of the island report a similar destruction as caused by the hurricane, and fifteen thousand more persons are houseless. Singularly enough, on the first day after the hurricane not a single case of cholera occurred in Manila or the island. The tornado not only swept over the entire Archipelago, but was felt many hundred miles out at sea, especially to the south and west. It is believed that more lives have been lost by shipwreck than on land.

AN interesting experiment has been made in Paris by M. Mangin, a member of the Académie d'Aérostation. A small balloon, measuring about 100 cubic feet, and filled with pure hydrogen, was sent up, being held captive by a rope containing two copper wires. A Swan incandescent light having been placed in the gas and attached to the top of the balloon, was lighted, and the whole aërial machine, which was quite translucent, was splendidly illuminated. It was shown by systematic interruptions that the dots and dashes of the Morse system could be imitated for giving military signals at a great distance.

NEWS from Verona states that the subterranean shocks continue. Houses have been destroyed by earthquakes at Cassone, Brescia, and Verona, and between Campiore and Forbefice a landslip occurred. A severe earthquake was experienced at Silchar, India, and in other districts in a lesser degree on the 13th inst.

THE Corporation of the town of Sheffield having resolved to apply to Parliament, under the Electric Lighting Act, for extensive powers for the illumination of the borough by electricity, not only in the streets and public buildings, but also in the private houses, Mr. Conrad W. Cooke, who has been appointed consulting engineer to the Corporation, has been instructed to prepare and report upon a scheme to be adopted by the Corporation.

MR. T. V. HOLMES, F.G.S., M.A.I., will read a paper on "Dene-Holes," at the meeting of the Essex Field Club to be held at 3, St. John's Terrace, Buckhurst Hill, at seven o'clock on Saturday next, the 28th inst. The paper and the discussion thereon will have special reference to the Club's projected explorations at Grays, Purfleet, and Tilbury. Archæologists and others interested in these mysterious relics are invited to attend the meeting.

THE *New Zealand Times* of September 1 contains an account of the presentation of degrees at Wellington in connection with the New Zealand University examinations. The chair was occupied by Dr. Hector, Vice-Chancellor, who said it had been decided by the Synod of the New Zealand University that the presentation of degrees should in future be made in public. The Chancellor being unable to be present, the duty of presenting the degrees had been deputed to him. The New Zealand University had been in operation since 1870, and there had been 155 graduates, of whom forty-nine had taken degrees. This might appear a small result, but the object of the University was to raise the standard of education, and this had been done. The system of scholarships had been continued with the University course, and a large portion of the funds had been spent in this way. For some time past the examiners had been appointed in London, and the degrees granted had a value, in the eyes of the outer world, equal to those granted by the London University. Owing to its charter, the New Zealand University could not grant degrees for science, but there was every prospect that the barrier would soon be removed. Dr. Hector then referred to the disaffiliation of Wellington College, which has been converted into a high school for secondary education. The step, he said, was necessary in order that they might get a University College.

A CURIOUS project in the way of recreation, by M. Joyeux, is published in *La Nature*. Suppose a large circular wooden chamber, lit from above, but giving no view of outer objects from within, and rotated smoothly and rapidly on a vertical axis. A person standing in it would have to bend his body towards the centre, by reason of centrifugal force, and the more so the further he might be from the centre and the higher the speed. M. Joyeux supposes he would be subject to the illusion that the floor was inclined upwards from his position to the centre; if he had to place himself at an angle of 45°, the floor would seem inclined at this angle, and a person standing in the corresponding place on the opposite side would seem horizontal, for he, too, would have to make an angle of 45°. Only at the centre would the floor seem horizontal; and if a number of persons were in the chamber, it is only there one would see them in their real positions. A person walking round the circumference would seem to be at the outside of the base of a cone, which turned under him. To facilitate the position of persons, M. Joyeux would make the floor, not horizontal, but inclining upwards at a certain distance from the centre. M. Tissandier does not feel certain that the illusions described would actually occur, but regards the scheme as an attractive curiosity. The apparatus is named a *plagioscope*.

IN a recent paper to the Belgian Academy, M. van der Mensbrugghe seeks to explain the calming influence of oil on rough water, in accordance with the principle he has laid down, that whenever a liquid mass in motion acquires rapidly a free surface, more or less, there is developed a growing quantity of potential energy at the expense of the kinetic energy of the mass; and reciprocally to a rapid diminution of free surface corresponds always an increase of kinetic energy. Oil hinders the successive superposition of liquid layers, and so, the increase of the kinetic energy of the liquid mass. Floating bodies of various kinds (branches, sea-weed, ice-crystals, &c.) have a like action; immediately after the gliding of a very small number of liquid layers over them they obey the thrust that brings them to the surface, and so render impossible the increase of kinetic energy corresponding to loss of potential energy of a large number of superposed layers.

IT has been observed by M. Fredericq (*Bull. Belg. Acad.*) that the blood of crabs and other crustaceans at Ostend has the same strong and bitter taste as the sea-water, and proves to have the same saline constitution. Crabs in brackish water, on the other hand, have a less salt blood, and the crayfish of rivers have very little of soluble salts in their blood. An exchange of salts seems to take place, in these animals, between the blood and the outer medium, producing approximate equilibrium of chemical composition. This probably occurs through the respiratory organ, and is according to the simple laws of diffusion. On the other hand, the blood of sea-fishes has an entirely different saline composition from that of the water; it is more or less isolated, presenting herein an evident superiority over the invertebrates referred to.

A USEFUL complement to M. Marey's recent method of applying photography to physiological experiments (in which a bright body moving before a dark screen is photographed several times in quick succession) has been supplied by M. Ch. Petit in a process which he calls *similigravure*; whereby the photographic picture is easily reproduced for insertion in a text. Two specimens are given in *Comptes Rendus* of October 2; one of them, showing the successive attitudes of a man marching at the parade step, the other, those of a white horse, with rider, leaping over an obstacle. The process is not described; but those pictures present at a glance (M. Marey points out) much that is instructive, showing, in the former case, *e.g.* the position of different parts of the body during the step (which was executed in 6·10ths of a second).

IN the October number of *Petermann's Mittheilungen* are two papers of scientific interest: one on the Geology of the Balkan Peninsula, with map, by Prof. Franz Toula; and the other on the Distribution of the Aurora Borealis in the United States, by Prof. H. Fritz.

M. LISCH, inspector of historic monuments, has recently discovered a whole Gallo-Roman town in the environs of Poitiers. It includes a temple, 14 m. long, and with 70 m. of facade, a thermal establishment covering 2 hectares, and still possessing its piscinæ, hypocausts, pipes, flagging, &c., a theatre, the stage of which is 99 m. in width; entire streets, and more than 7 hectares of buildings (the excavations are not yet finished). "It is," he says, "a small Pompeii in the centre of France." The sculptures are in the best style, and thought to date from the second century.

THE additions to the Zoological Society's Gardens during the past week include a Vervet Monkey (*Cercopithecus lalandii* ♂) from South Africa, presented by Mr. H. T. Hardcastle; a Common Marmoset (*Hapale jacchus*) from Brazil, presented by Miss Katie Thomason; a Common Paradoxure (*Paradoxurus typus*) from India, presented by Mr. J. Wood; a Naked-eared Deer (*Cariacus gymnotis* ♀) from Ecuador, presented by Miss Lake; an Oyster-catcher (*Hæmatopus ostragelus*), British, presented by Mr. W. R. Temple; a Maholi Galago (*Galago maholi*) from South Africa, deposited; a Ruff (*Machetes pugnax*), a Redshank (*Totanus calidris*), British, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

COMET 1882 *b* (FINLAY, SEPTEMBER 8).—The following ephemeris is deduced from the same elements as that given last week:—

At 18h. Greenwich M.T.

	R.A. h. m.	N.P.D. ° ′	Log. distance from Earth.	Log. distance from Sun.
Nov. 2 ...	9 52·7 ...	110 23 ...	0·1700 ...	0·1509
6 ...	9 45·8 ...	111 47 ...	0·1716 ...	0·1749
10 ...	9 38·3 ...	113 9 ...	0·1728 ...	0·1970
14 ...	9 30·1 ...	114 27 ...	0·1739 ...	0·2176
18 ...	9 21·1 ...	115 40 ...	0·1751 ...	0·2368
22 ...	9 11·3 ...	116 47 ...	0·1766 ...	0·2549

At the time we write a sufficient number of observations before perihelion passage to allow of a reliable determination of the orbit prior to the close approach to the sun, is not available.

By the way it strikes us it is about time that M. Crul's name was disassociated from this comet; if personal names are to be attached to naked-eye comets, a practice which to say the least, is inconvenient, Mr. Finlay, so far as is known at present, has the prior claim. The system generally adopted for some years, of assigning letters, *a*, *b*, *c*, &c., to comets discovered in a particular year, until their order of perihelion passage is definitely known, was, we think, an advantageous one, and its discontinuance in some quarters is a retrograde step.

COMET 1882 *c* (Barnard, September 10).—From the first observation at Harvard College on September 14, and observations by Prof. Millosevich, at the Collegio Romano in Rome, on September 22 and October 7, Mr. Hind has calculated the following elements of this comet:—

Perihelion passage 1882, November 13·0067 G.M.T.

	° ′
Longitude of perihelion	354 47·6
,, ascending node	249 8·9
Inclination	83 43·1
Logarithm of perihelion distance ...	9·97998

Motion—retrograde.

From these elements it appears that the comet will be observable in the southern hemisphere for some weeks after it descends below the horizon in Europe. At perihelion passage on November 13, its right ascension will be about 199°·4, with 66⅓° south declination, which places it near η in the constellation Musca; on December 10 it will be situate between the stars γ and η in Ara, with 58° declination, and an intensity of light one-third greater than at the first Harvard observation, and on January 9 its place will be near ϵ Telescopii, with one-half the intensity of light of September 14.

PONS' COMET OF 1812.—MM. Schulhof and Bossert have published a continuation of their extensive ephemerides to facilitate the search for this comet from October 28 to February 4, and for equal intervals of true anomaly from $-97°$ 30′ to $+82°$ 30′. By their new and complete discussion of the observations, including a series by Blanpain at Marseilles, which they discovered in the original, and which they consider the best of all, the most probable elements in 1812 were found to be as follows:—

Perihelion passage, 1812, September 15·33210 Paris M.T.

	° ′ ″	
Longitude of perihelion	92 19 48·2	Mean Equinox, 1812·0
,, ascending node	253 0 43·7	
Inclination	73 57 35·8	
Excentricity	0·9555842	
Logarithm of perihelion distance ...	9·8904903	

The corresponding period is 73·18 years, but the probable error of this period of revolution is ± 4·5 years. Notwithstanding this large amount of uncertainty, MM. Schulhof and Bossert have calculated the effect of the action of the planets Jupiter, Saturn, Uranus, and Neptune during the actual revolution, and find the most likely epoch of the next perihelion passage to be 1884, September 3·65, M.T. at Paris.

THE TRANSIT OF VENUS.—Mr. Marth, who has charge of the proposed station at Montague Road, Cape Colony, left for Cape Town in Messrs. Currie and Co.'s mail steamship *Conway Castle* on the 13th inst., and Mr. Talmage, of Mr. J. Gurney Barclay's observatory at Leyton, proceeded in the R. M. steamship *Nile* on the 17th for Barbados, with Lieut. Thomson, R.A., as his colleague. Mr. J. Plummer, in charge of Col. Tomline's observatory at Orwell Park, Ipswich, with Lieut. Neate, R.N., have also left for New York, on their way to Bermuda. All the British expeditions are therefore *en route*.

Brazil will furnish four stations, with similar instrumental equipment, including equatorials of 6 inches aperture. M. Cruls proceeds to a point in the Straits of Magellan, and Baron de Jeffé, of the Brazilian Navy, to St. Thomas. The other stations will be Pernambuco, and the Imperial Observatory at Rio Janeiro. M. Faye, who made a communication to the Paris Academy of Sciences on the 16th inst., in the name of the Emperor of Brazil, who takes a lively personal interest in his observatory, mentions that it is in contemplation to effect a chronometric connection of the station in the Straits of Magellan with Montevideo, an important undertaking, as viewed with reference to the telegraphic determination which the Board of Longitudes is about to execute across the American continent, from Montevideo or Buenos Ayres, to Santiago and Lima.

A SPECTROSCOPIC STUDY OF CHLOROPHYLL[1]

THE study of chlorophyll has great fascination; it also has its difficulties. We did not propose adding to the many elaborate attempts to isolate and purify this body; but the beauty and definite character of the spectrum which it gives induced us to try whether some insight into its character and constitution could not be obtained from the study of the spectroscopic changes which it can be made to undergo; and as one of us has already shown that in the case of the cobalt salts, the spectroscope enables us to follow many chemical changes, we thought that it might be possible to interpret the spectroscopic changes of chlorophyll, and so gain some knowledge of the properties and nature of this body.

The extraction of the green colouring matter from leaves was effected in most cases by breaking up the leaves in a mortar with a mixture of two parts of alcohol and one of ether. The colour of the liquid thus obtained is of a dark green, varying in shade according to the nature of the leaves used, and the solution always has the well-known red fluorescence. This liquid, when examined spectroscopically, gives what is known as the chlorophyll spectrum. According to Krauss, it consists of seven bands; the three at the most refrangible end of the spectrum are difficult, as Krauss says, to observe, and with our source of light, a gas-flame, we could see in an ordinary chlorophyll solution little or nothing of them; but under special circumstances, which will be described further on, the least refrangible of the three becomes very visible. We have confined our observations principally to the four least refrangible bands. Other solvents, such as chloroform, disulphide of carbon, benzene, &c., were used occasionally; they give a similar spectrum, but in most cases they do not dissolve the colouring matter so readily as alcohol and ether do. The ethereal solution appears always to give a clearer and more brilliant spectrum than the alcoholic solution. Fig. 1 shows the spectrum of the solution obtained as above described from the majority of the leaves we have examined.

Among common outdoor plants, the vine and the Virginian creeper may be cited as apparent exceptions, giving a different spectrum. (Fig. 2.) The second band in this case has moved towards the more refrangible end of the spectrum, the band from 589 to 573 has disappeared, and now there is a very marked band from 545 to 532. The cause of this change in the spectrum we shall explain further on.

Fig. 1 then, as far as it goes, represents the spectrum given by the alcohol and ether extract of most leaves. It is important at once to give a definite meaning to the term chlorophyll, and we would therefore state that we mean by it the body or bodies capable of giving this particular spectrum, and of course we found our conclusions on the assumption that a particular absorption-spectrum is a complete identification of a substance.

As is well known, the exact position of these bands alters with the solvent used; in all cases, when no mention is made to the contrary, a mixture of alcohol and ether is the solvent we have used. Apparently the statement that the higher the specific gravity of the solvent, the nearer are the bands to the red end of the spectrum, is not in all cases true, for we find that the chlorophyll bands are nearer to the red in carbon disulphide than in chloroform. All our observations have been made with a Dasaga's spectroscope having a single heavy glass prism, and the position of the bands is given in millionths of a meter, reduced from the observations by graphical interpolation. Capt. Abney has also been kind enough to take photographs of the different spectra, and these agree with our eye observations. They also prove that there are no bands in the ultra-red.

The first point we would note with regard to chlorophyll is

[1] By W. J. Russell, Ph.D., F.R.S., and W. Lapraik, F.C.S.

that, as far as our experiments go—and we have now tried a large number of different leaves—although there are apparent exceptions, this particular substance we call chlorophyll exists in all green leaves.

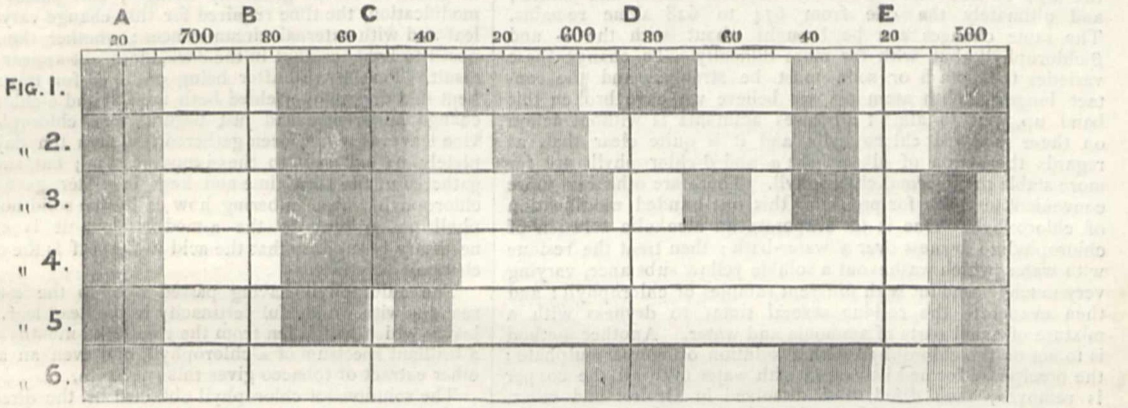
If thinner and thinner strata, or more and more dilute solutions of the same thickness be examined, the fainter bands are seen gradually to fade out, and what is of importance, the dominant band, the last to disappear, thins out to a band from 670 to 660.

Passing over a large number of experiments on other points, we shall limit our present communication as far as possible to an account of the action of acids and alkalis on this so-called chlorophyll.

It is a body exceedingly sensitive to the action of acids. If for instance a mere trace of hydrochloric acid gas be introduced into the air of a test-tube containing a chlorophyll-solution, on shaking the tube, the 628 band will be found to have moved slightly towards the blue, and the next band to have become fainter. This action of the acid, specially with regard to the 628 band, is very remarkable; the addition of acid gradually causes this band to move bodily towards the blue, till it reaches 611—599. So constant and complete is this action, that the position of the band is an indication up to a certain amount of the quantity of acid present. On adding a little more hydrochloric acid gas to the air of the test-tube, and again shaking, this second band will be found to have moved from 615 to 596, the 589—573 band will have disappeared, and the other band at 545—532 will remain unmoved, but will have become much darker. On still further increasing the amount of acid, the second band comes to 611—589, and now a new band appears from 573—558, and the band at 545—532 has also again increased in intensity. Further, the blue end of the spectrum has considerably opened. This spectrum, Fig. 3, is permanent, for on adding more acid, even a large amount of liquid acid, no further alteration takes place. The action of hydrochloric acid on chlorophyll appears then to be very definite, and is well shown by the two drawings, Figs. 2 and 3, which represent two well-marked stages; in the first the movement of the 628 band and the disappearance of the 589 band, the other two bands remaining unaltered in position; in the second (Fig. 3) the 628 band has moved to its furthest extent, and a new band has appeared at 573—558, the most and least refrangible of the four bands remaining still unaltered in position.

We have described in detail these spectra, for they have great interest and importance, owing to the fact that these changes do not arise from the formation of any chlorine-compound, but are produced by the action of the hydrochloric acid simply as an acid. Substitute any strong acid, sulphuric, nitric, &c., for the hydrochloric acid, and exactly the same changes will occur. Use a weak acid, an organic acid, such as tartaric, citric, oxalic, &c., and the action does not go beyond the first stage (Fig. 2). Carbonic acid is without action on the chlorophyll.

There is also another way in which the same changes may be brought about without the presence of acid, namely, by the application of heat. If, for instance, the solution of chlorophyll be evaporated to dryness on a water-bath at a temperature of 80° or above, then on redissolution it will be found to have changed and to give no longer the original, but the second spectrum. Let the evaporation take place at ordinary temperatures in a current of air, or under the air-pump, then, on at once redissolving the residue, no change will have occurred; if, however, after the evaporation, the dry mass be kept for a short time, it will change even at ordinary temperatures. Further, if the alcoholic solution be diluted with water, and then boiled, the body giving spectrum No. 2 is formed; and the addition of certain salts, such as mercuric chloride, ferric chloride, &c., causes a similar change. Alum precipitates the colouring matter, and if the precipitate be collected, washed, and dried at ordinary temperatures, and again dissolved, it will give the second spectrum. On the other hand, basic acetate of lead precipitates the chlorophyll unchanged.



Acids, heat, metallic solutions, all action the chlorophyll, and all give rise to an identical spectrum, and therefore, we conclude, to the same body. Further, it is of interest to note the identity of these processes with those used to coagulate albumin, and consequently the probability that the change in both cases is of a similar character.

Since these changes are produced by processes and reagents which differ so materially, we are bound to conclude that the change is a molecular, not a chemical one. In these cases the least refrangible band does not alter, for if the solution be diluted, it always thins down to a band from 670 to 660; the other three bands, on the contrary, all change, the 628—607 moving towards the blue, the 589—573 band disappearing, and the 544—531 band becoming very much darker. In fact, although a shadowy indication of this last band is constantly visible in the normal solution, it is often so small in amount that it should be regarded rather as an accidental impurity than as a necessary part of the normal spectrum. Again, the essential and characteristic distinction between the two spectra, Figs. 2 and 3, is the presence in the latter of the band at 573—558. This band, as far as we know, is produced solely by the presence of a strong acid in considerable excess, and all specimens of chlorophyll, either normal or not, yield it on the addition of hydrochloric, nitric, or sulphuric acid.

There is obviously a considerable resemblance between these three spectra, but at present, notwithstanding the beautiful work of Abney and Festing, we can hardly deduce from these indications alone the nature and relationship between these bodies; but from the processes used for obtaining them, there can, we think, be little or no doubt that they are simply molecular modifications of the original chlorophyll, and we propose at present to designate them as α- and β-chlorophyll.

With regard to the different purifying processes that have been used for obtaining chlorophyll from leaves, &c., in some cases the normal chlorophyll has been extracted; in others the leaves have first been dried at steam-heat, or the alcoholic solution has been boiled, and it is the α-chlorophyll that has been obtained. We have tried several of these processes, and, efficacious as they undoubtedly are in removing many, if not all, of the numerous bodies existing in more or less intimate connection with the chlorophyll, still they appear to produce really no change in the spectrum. With regard to general absorption, no doubt they do produce marked effects, specially at the blue end of the spectrum; this is well seen in the methods of purification recommended by Conrad. He obtained, as he believed, a separation of chlorophyll into a green and a yellow body by means of benzene. Observation shows, however, that the band-giving body, the chlorophyll, remains quite unchanged by the benzene, but that certain bodies which absorb in the blue are insoluble in this menstruum: hence the change in colour.

Hydrochloric acid has apparently considerable power of destroying certain of these blue-absorbing bodies, for on adding this acid to an ordinary chlorophyll solution, blue rays come through, where before the addition it was quite dark. This fact has also this application: by means of it chlorophyll can be obtained more free from blue-absorbing matter than in any other way we are acquainted with. If to an alcoholic chlorophyll solution dilute hydrochloric acid be added, a precipitate is obtained, and if this be washed, dried, and dissolved in ether or in a mixture of alcohol and ether, it gives a solution which shows, not only the bands of the α-modification, but also a band at the blue end of the spectrum, which was before alluded to, quite dark and distinct from 513 to 499. In all probability this band is present in other cases, but is masked by general absorption.

The action of alkalis on chlorophyll is quite as marked and as characteristic as the action of acids. On adding either an alcoholic or an aqueous solution of potash or soda to a chlorophyll solution, two effects are produced: one is the fading out of all except the least refrangible, the dominant band, and the other is the spread of this band towards the blue, extending from 674 to 628. The action of alkali does not, however, stop here, for if a considerable excess be present, another, and an exceedingly interesting change sets in the dominant band now from 674 to 628 dividing into two distinct bands,[1] one from 674 to 660, and the other from 646 to 632; then if sufficient alkali be present, the 674 to 628 band gradually becomes fainter and fainter, and ultimately the one from 674 to 628 alone remains. The same changes can be brought about with the α- and β-chlorophyll, but with far more difficulty. To change these varieties the potash or soda must be stronger, and the contact longer. With ammonia we believe we have broken this band up, but in almost all cases ammonia is without action on these modified chlorophylls, and it is quite clear that, as regards the action of alkalis, the α- and β-chlorophylls are far more stable than normal chlorophyll. There are other and more convenient methods for preparing this one-banded modification of chlorophyll. One is to evaporate an alcoholic solution of chlorophyll to dryness over a water-bath; then treat the residue with water, which washes out a soluble yellow substance, varying very much in amount with different samples of chlorophyll; and then evaporate the residue several times to dryness with a mixture of equal parts of ammonia and water. Another method is to act on the chlorophyll with a solution of copper sulphate; the precipitate formed is washed with water until all the copper is removed, then dried, and dissolved in alcohol and ether. It gives a spectrum identical with that obtained by the ammonia process, and like it the band is capable of being split up into two bands. In the filtrate from the above precipitate there is always much chlorophyll remaining, but this, curiously enough, has also been modified, and now gives only the one-band spectrum. When we first obtained this one-banded substance, the position of this band appeared so nearly to correspond with that of the dominant band in a strong solution, that we were inclined to believe that we had really separated the bodies giving the more refrangible bands, from those which give the less refrangible; but evidently this is not the case; neither does it now seem at all probable that such a separation would be possible.

We have used the term one-banded modification of the chlorophyll, and are aware of the possible ambiguity that this band can be split into two; but this change is really brought about only by the continued action of alkalis, for on simply diluting the solution down even to the vanishing point of the band, there is no indication of two bands being present.

The solution of this one-banded substance is still of a beautiful green colour, and is very remarkable for its stability; neither a trace nor an excess of acid of any kind produces any change in its spectrum, and it may even be dissolved in strong sulphuric acid and reprecipitated by water without alteration.

If the action of caustic potash or soda be pushed to an extreme, for instance if chlorophyll be heated with solid potash, then it is apparently completely decomposed, the dominant band disappearing, and two bands different in position from any of the former ones being produced; these are shown in Fig. 6.

To return now to the fact of different leaves giving different spectra; for instance, when vine-leaves are treated with alcohol and ether, the liquid gives strongly the α- not the normal spectrum. As is well known, the juices of the vine-leaf are very acid: consequently during the extraction of the colouring-matter, the acid has time and opportunity for action, and hence the cause of what appears at first to be an anomaly. In the leaf itself the chlorophyll is in the normal condition, for if to the bruised leaf precipitated calcium carbonate or carbonate of soda be added, together with the alcohol and ether, the filtered liquid then gives, not the α- but the normal spectrum; and even without the addition of the calcium carbonate, on rapidly extracting the colouring-matter from the leaf and examining it immediately, the spectrum is normal. It is therefore evident that although both chlorophyll and acid are present in the leaf, they are not under such conditions that they can act on one another; but bring them into solution, and the change commences immediately.

Virginia creeper, Bigonia, and other leaves, act exactly like the vine. The acid in the Bigonia can be entirely removed by water, and if the colouring-matter be then extracted, it gives the normal spectrum.

The way we now generally adopt in extracting the chlorophyll from leaves is to add with the alcohol and ether precipitated calcium carbonate; then, whether the juice of the leaf be very acid or not, is a matter of indifference. We have already stated that in all the different leaves which we have examined, the chlorophyll has been found to be in the normal condition. This applies of course only to freshly-gathered leaves; the chlorophyll in gathered leaves gradually changes, and passes over the α-modification, the time required for this change varying with the leaf and with external circumstances; whether the leaf be exposed to light, or kept in the dark, does not appear to affect the result. Pear leaves, after being gathered for three weeks and kept in a dry room, yielded both normal and α-chlorophyll; the change apparently had just begun. The chlorophyll in some vine leaves that had been gathered less than ten days had completely passed over to the α modification; but similar leaves, gathered at the same time and kept in water, gave only normal chlorophyll. Remembering how easily the solid normal chlorophyll passes over to the α-modification, it is evidently not necessary to suppose that the acid in the leaf is the cause of this change.

The chlorophyll having passed over to the α-modification, remains with wonderful pertinacity in the dead leaf. Dead pear leaves which had fallen from the tree seven months ago still gave a brilliant spectrum of α-chlorophyll, and even an alcoholic and ether extract of tobacco gives this spectrum.

The solutions of chlorophyll obtained by the direct treatment of leaves with alcohol and ether, contain a large number of substances, and the chlorophyll, as well as the other bodies, undergoes change on keeping. The length of time during which these solutions retain their green colour varies very much; expose them to light, and the rapidity of the change is enormously increased. If acid be present in the solution, the chlorophyll quickly passes over to the α-modification, and even if the extract has been made with calcium carbonate present, the same change occurs, only more slowly. These changes take place even in the dark. Besides this change of the chlorophyll, other and more complicated changes occur. Solutions from some leaves can be kept in the dark apparently without change for months, whereas others rapidly alter, and the chlorophyll disappears from them. The extract from rhubarb, for instance, very soon changes, the solution becoming of a tolerably bright red colour, and the chlorophyll bands disappearing. This red substance and the other products of decomposition from their solutions do not give visible spectra, and the same remark applies to at least the majority of the colouring matters in flowers. If these green solutions be exposed to light, they are, without exception, rapidly decomposed, and lose entirely their green colour, becoming either red, yellow, or of some intermediate shade. Brilliant sunshine in an hour or two will completely decompose all the chlorophyll in a dark green solution, not even a vestige of the dominant band remaining. If a solution of the α-chlorophyll, dissolved in alcohol and ether, be exposed to light, it is far more difficult of decomposition, and will withstand its action for a few days. That this stability is not due to the absence of certain substances in the solution of the α-modification, is shown by dissolving some of this modified chlorophyll in a normal and readily decomposable solution, when it will be found that, although there will be a change of colour

[1] Chautard, as long ago as 1836, mentions this; he naturally concludes that it is the original dominant band split up (*Compt rend.*, lxxvi. 570).

owing to the decomposition taking place in such a solution, still the green colour from the modified chlorophyll will long remain. A single drop of hydrochloric acid added to the green extract, although it at once changes the bright green to a darker and browner green, enables the solution to resist this action of light to a much greater extent than it could have done if no acid had been added.

In the one-banded modification of chlorophyll we appear to have a body on which light has no action; solutions of this body have been, for the last three months, exposed continuously to all the light and sunshine we could get, and they are unchanged in colour and constitution; another proof of the really wonderful stability of this substance. Again, as a confirmation of the properties and formation of this form of chlorophyll, a single drop of sulphate of copper added to an ordinary chlorophyll extract renders the green colour of the solution permanent.

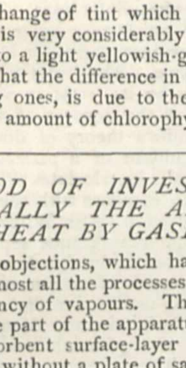
The very striking change of tint which occurs when a strong chlorophyll solution is very considerably diluted, whereupon it changes from a dark to a light yellowish-green, forcibly suggests to us the probability that the difference in shade of old leaves as compared with young ones, is due to the same cause, namely, the greater or smaller amount of chlorophyll in a given area.

ON A METHOD OF INVESTIGATING EXPERIMENTALLY THE ABSORPTION OF RADIANT HEAT BY GASES [1]

THERE are grave objections, which have been only partially overcome, to almost all the processes hitherto employed for testing the diathermancy of vapours. These arise chiefly from condensation on some part of the apparatus. Thus when rock-salt is used, an absorbent surface-layer may be formed; and, when the pile is used without a plate of salt, the effect of radiant heat may be to cool it (the pile) by the evaporation of such a surface film. The use of intermittent radiation is liable to the same objection.

Some time ago it occurred to me that *this* part of the difficulty might be got rid of by dispensing with the pile, and measuring the amount of absorption by its continued effects on the volume and pressure of the gas or vapour itself.

Only preliminary trials have, as yet, been made. They were carried out for me by Prof. Mac-Gregor and Mr. Lindsay.



Their object was *first* to find whether the method would work well, *second* (when this was satisfactorily proved) to find the best form and dimensions for the apparatus.

The rough apparatus is merely a double cylinder, placed vertically. Cold water circulates in the jacket, and steam can be blown into the double top. The changes in the pressure of the gas are shown by a manometer U tube at the bottom, which contains a liquid which will not absorb the contents. This apparatus was 4 feet long, with 2 inches internal radius. The results of a number of experiments show that it should be shorter and much wider. The former idea I was not quite prepared for, the latter is obvious.

The effects on the manometer are due to five chief causes:—

1. Heating of the upper layer of gas by contact with lid.
2. Cooling ,, ,, ,, ,, sides.
3. Heating of more or less of the column by absorption.
4. Cooling of do. by radiation.
5. ,, ,, contact.

(1) and (2) only are present in a perfectly diathermancus gas, and in a perfectly adiathermanous gas or vapour.

All five are present in a partially diathermanous gas or vapour.

The preliminary experiments show that the manometer effect is only *very slightly less* for dry olefiant gas than for dry air, while moist air shows a markedly smaller effect than either of the others.

This is conclusive as to the absorption of low radiant heat by aqueous vapour, but it shows also that the absorption is so small as to take place throughout the whole column.

Even with the present rude apparatus I hope soon to get a very accurate determination of the absorbing power of aqueous vapour, by finding in what proportions olefiant gas must be mixed with air to form an absorbing medium equivalent to saturated air at different temperatures.

I have to acknowledge valuable hints from Prof. Stokes, who, before I told him the results I had obtained (thus knowing merely the *nature* of the experiments) made something much higher than a guess) though somewhat short of a prediction, of the truth.

In these preliminary trials no precaution was taken to exclude *dust*. The results, therefore, are still liable to a certain amount of doubt, as Mr. Aitken's beautiful experiments have shown.

The *point* of the method is that there can be no question of surface-layers.

[Since the above was written, Messrs. Mac-Gregor and Lindsay have made an extended series of experiments with dry and moist air, and with mixtures of dry air and olefiant gas in different proportions. The cylinder employed was 9 inches in radius. The results will soon be communicated to the Royal Society of Edinburgh.—P. G. T.]

[1] Letter from Prof. Tait, read by Sir W. Thomson at the Southampton meeting of the British Association.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—In addition to the courses in Natural Science described in last week's NATURE, the following will be given during the present Michaelmas term:—Prof. Pritchard will give a course of six lectures on the Theory of the Transit Instrument, Equatorial, and Sextant, to be followed by six lectures on the Lunar Theory. There will be eight lectures on Instrumental Practice, and eight "Evenings with the Telescope," the latter being of a popular and untechnical character.

Prof. Lawson has announced the following courses of lectures for the ensuing year at the Botanical Gardens:—

Course I. Vegetable Histology; Michaelmas Term, 1882.

Course II. Special Morphology; Lent Term, 1883, and Trinity Term, 1883 (continued).

Course III. Descriptive Botany; every Saturday in Lent and Trinity Terms, 1882.

Prof. Prestwich gives a course on Theoretical Geology at the University Museum, and Prof. Westwood on Certain Groups of Anthropoda.

The Regius Professor of Medicine gives notice that an examination for certificates in Preventive Medicine and Public Health will be held this term, and secondly that Bachelors of Medicine may proceed to the degree of Doctor in any term, on due notice being given.

Natural Science Scholarships are offered this term at Balliol and at Christ Church.

The notice issued by Balliol College states there will be an election to a sholarship on the foundation of Miss Hannah Brackenbury, "for the encouragement of the study of Natural Science," worth 80*l*. a year (55*l*. and tuition free), tenable during residence for four years: open to all such candidates as shall not have exceeded eight terms from Matriculation. This examination will begin on Thursday, November 16, at ten o'clock. Papers will be set in the following subjects:—(1) Mechanical Philosophy and Physics; (2) Chemistry; (3) Biology. But candidates will not be expected to offer themselves in more than two of these. There will be a practical examination in one or more of the above subjects, if the examiners think it expedient. There will also be an optional paper in Mathematics; and the literary qualifications of the candidates will be tested by an English essay, or by a paper of general questions.

At Christ Church *at least* one scholar will be elected in Natural Science. Papers will be set in Biology, Chemistry, and Physics, but no candidate will be allowed to offer more than two of these subjects. An optional paper will be set in Elementary

Mathematics. Candidates will be tested in Classics, and required to show sufficient knowledge to pass Responsions. The emolument is 80*l*. annually. The examination begins on November 23. Candidates must not have exceeded the age of nineteen. The election in the first place is for two years. The tenure will be renewed for another two years if the College is satisfied with the progress and good conduct of the scholar. For special reasons the scholarship may be prolonged for a fifth year.

The formation of the new Boards of Faculties will not be proceeded with this term; it is proposed to defer the elections till a day not later than February 3, 1882. The appointment of examiners will therefore rest this term with the Vice-Chancellor and Proctors as before.

Prof. Max Müller has been elected a permanent Delegate of the Clarendon Press.

CAMBRIDGE.—Mr. James Ward is appointed Lecturer on the Science of Education at Cambridge for the present year; Mr. W. N. Shaw, of Emmanuel College, is approved as a teacher of Physics, and Mr. J. N. Langley, of Trinity College, as a teacher of Physiology for the purpose of Medical Studies.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, October.—Mohr's geographical theory of earth-pressure, by G. F. Swain.—The platinum-water pyrometer, by J. C. Hoadley.—Experiments on the fatigue of small spruce beams, by F. E. Kidder.—Theory of the stereoscope, by W. Leconte Stevens.—Report on European sewerage systems, &c. (continued), by R. Hering.—The manufacture of potash alum from felspar, by H. Pemberton, jun.—Report of the committee on the Fowler cloth-cutting machine.

Revue internationale des Sciences for September, 1882, contains: On the psychology and writings of Broca, by M. Zabrowski.—On the structure and on the movements of the protoplasm in the vegetable cell, by H. Frommann.—On orientation and its organs in man and animals, by M. Viguier.

SOCIETIES AND ACADEMIES

SYDNEY

Linnean Society of New South Wales, August 30.—The president, Dr. James C. Cox, F.L.S., &c., in the chair.—The following papers were read:—By the Rev. J. E. Tenison-Woods, F.G.S., &c., Botanical notes on Queensland, No. 4. This paper contained the author's observations on some of the Queensland species of *Myrtaceæ*, chiefly of the *Eucalypti*.—By the Rev. J. E. Tenison-Woods, F.L.S., &c., &c., on a coal plant from Queensland. This is an account of a fossil species of *Equisetum* found in the Ipswich coal beds, and provisionally named *E. rotiferum*, from the wheel-like shape of the diaphragm. No *Equisetum* had previously been found in the Australian coal beds.—By William Macleay, F.L.S., &c., Observations on an insect injurious to the vine.

PARIS

Academy of Sciences, October 16.—M. Jamin in the chair.—The following papers were read:—On the shock of two balls placed on a billiard table, by M. Resal.—On the catalogue of six hundred tornadoes observed in the United States in the course of this century, by M. Faye. This relates to a report by Sergeant Finley, of the U.S. meteorological service. The rapid increase of tornadoes recorded shows the rapidity with which population has increased. Trombes and tornadoes are short epiphenomena of cyclones. *Inter alia*, the mean velocity of gyration in a tornado is about 174 metres per second; the usual diameter is about 300 m. to 400 m.; the mean velocity of translation 17 m. per second. Most go from S.W. to N.E. They traverse about 11 leagues on an average, and last three-quarters of an hour. Several tornadoes may occur in one cyclone. They are formed exclusively in the dangerous semicircle of a cyclone, and nearly always a little in advance. They show a marked predominance in April, June, and July, and from 4 p.m. to 6 p.m.—On the functions of seven letters, by M. Brioschi.—Rational conception of the nature and propagation of electricity deduced (1) from consideration of the potential energy of ethereal matter associated with ponderable matter; (2) from the mode of production and transmission of work arising from variations of this energy, by M. Ledieu.—On the processes employed for the construction and plan of the metric standards, by M. Tresca. He has been unwell, but promises a complete memoir on the subject shortly.—Brazilian missions for observation of the transit of Venus, by M. Cruls. These are four in number, and will act at St. Thomas, Magellan, Pernambuco, and Rio de Janeiro, the respective heads being Capt. Jeffé, M. Cruls, M. Lacaille, and Capt. Jacques. Each station will have a 6-in. equatorial, a 4½-in. astronomical telescope, a meridian instrument with collimator, an excellent compensated pendulum, electric chronograph, &c. A chronometric junction of Magellan with Montevideo will be undertaken.—On the comet 1812 (Pons) and its approaching return, by MM. Schulhof and Bossert.—On the metric and kinematic properties of a sort of conjugated triangles, by M. Stephanos.—Ordinary and extraordinary indices of refraction of Iceland spar for rays of different wave length as far as the extreme ultra-violet, by M. Sarasin. The measurements referred to the principal lines of the visible solar spectrum and the lines of cadmium (induction spark) between two cadmium points). M. Soret's fluorescent ocular was used for the ultra-violet lines. The columns for the two prisms used show satisfactory agreement, as do also the author's values for the ordinary index for D and F with those of M. Mascart and M. Cornu.—The forces of induction which the sun develops in bodies by its rotation vary, all other things equal, in inverse ratio of the squares of the distances, by M. Quet.—On M. Helmholtz's theory of double electric layers; calculation of the magnitude of a molecular interval, by M. Lippmann. The interval ϵ he calculates to be 1-35,000,000 mm., which it is interesting to compare with the number, nearly the same (1-30,000,000) arrived at by Sir William Thomson by quite another way, for the minimum distance separating copper from zinc.—On the electrolysis of hydrochloric acid, by M. Tommasi. He examines the two cases of concentrated and dilute acid, platinum electrodes being used.—On the reduction of nitrates in arable land, by MM. Dehérain and Maquenne. Nitrates, in being reduced in arable land, liberate under certain conditions protoxide of nitrogen. The reduction occurs only in arable land containing much organic matter, and has been observed only when the atmosphere of the ground was absolutely free from oxygen.—On the industrial richness of crude alunite, in powder, by M. Guyot. The proportion of the base varies considerably (17·5 to 32 per cent.).—On chronic poisoning by antimony, by MM. de Poncy and Livon. A cat weighing 867 gr. at first was made to absorb, in a regular progressive way, 0·628 gr. of white oxide of antimony between April 26 and August 13. The animal did not pass through a period of embonpoint (as with arsenic), but it gradually fell into disease, took diarrhœa and died. All the tissues were pale and colourless, and nearly all the organs showed fatty degeneration.—Two maps of part of the Newfoundland coast, by Admiral Cloué, were presented.

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