Variable Stars

Star	R.A. h. m.	Decl. ° ′		h. m.	
R Ceti	2 20·3	0 41 S.	Apr. 14,		*M*
Algol	3 0·8	40 31 N.	,, 12,	3 7	*m*
			,, 14,	23 56	*m*
ζ Geminorum	6 57·4	20 44 N.	,, 10,	2 0	*m*
			15,	2 0	*M*
U Monocerotis	7 25·4	9 35 S.	,, 15,		*m*
R Virginis	12 32·8	7 37 N.	,, 13,		*m*
δ Libræ	14 54·9	8 4 S.	,, 13,	21 29	*m*
R Herculis	16 1·2	18 41 N.	,, 15,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 13,	3 24	*m*
			and at intervals of	20 8	
W Sagittarii	17 57·8	29 35 S.	Apr. 10,	3 0	*m*
U Sagittarii	18 25·2	19 12 S.	,, 11,	21 0	*m*
			,, 14,	20 0	*M*
R Lyræ	18 51·9	43 48 N.	,, 15,		*M*
η Aquilæ	19 46·7	0 43 N.	,, 14,	2 0	*m*
δ Cephei	22 25·0	57 50 N.	,, 13,	4 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers

	R.A.	Decl.
Near β Ursæ Majoris	162°	57° N.
ν Virginis	180	7 N.
μ Draconis	249	51 N.
μ Herculis	270	25 N.

GEOGRAPHICAL NOTES

MR. GEORGE GRENFELL has recently made a successful ascent of the great Quango tributary of the Congo. In company with Mr. Bentley, in the steamer *Peace*, he succeeded in reaching the Kikunji Falls, the point at which Major von Mechow, descending the Quango from the south, was obliged to turn back in 1880. About six miles above the junction of the Kasai and the Quango they found another large tributary, the Djuma, entering the river from the east, which presented so great a volume of water that it was a matter of uncertainty which was the larger stream. A little beyond this the course of the Quango veered round, first south-south-west, and then west; at 4° 30′ S. lat. it had come back to its usual northerly course, and maintained it for the remainder of the journey. The Kikunji Falls (5° 8′ S. lat.) are about 3 feet high, and though insurmountable to the *Peace*, are said by Mr. Grenfell to be no obstacle to communication by canoes and small craft.

IN a letter from the Rev. W. G. Lawes, dated Port Moresby, January 20, it is stated that an Expedition is being equipped under the leadership of Mr. Vogan, the Curator of the Auckland Museum, with the intention of attempting, as soon as the rainy season was over, to cross South-East New Guinea, from Freshwater Bay to Huon Gulf.

THE April number of the Proceedings of the Royal Geographical Society is largely devoted to papers on Central Asia. First we have Mr. Delmar Morgan's account of Prjevalsky's journeys and discoveries in Central Asia. Mr. Morgan also contributes a translation in abstract of a recent lecture by M. Potanin on his journey in North-Western China and Eastern Tibet, which is followed by an account of the travels of Messrs. James, Younghusband, and Fulford in Northern and Eastern Manchuria. In this last will be found some welcome details concerning of the country not previously described.

ACCORDING to Dr. Hans Schinz, who has been recently in the Lake Ngami region, that lake is not dried up, though its dimensions are gradually decreasing. The River Okovango forms an extensive marsh on the north-west, which sends very little water a part into the lake during the dry season.

IN a paper by Dr. Ochsenius in the *Zeitschrift* of the Berlin Geological Society, on the age of certain parts of the South American Andes, are some details of geographical and ethnological interest. The author believes that the South American Cordilleras, or at least a portion of them, are no older than the Quaternary (as contrasted with the certainly older coast Cordilleras), and infers, therefore, that Lake Titicaca and the surrounding region must have been raised to its present elevation of about 13,000 feet within the historical period. Dr. Ochsenius therefore maintains that the enormous ruins of the old Inca city Tihuanaco on that lake admit of no other explanation than that these colossal monoliths were not worked at their present elevation, far less transported thither; it is incredible that the highly civilised Incas would have located their emporium on a tableland now almost uninhabitable. The author supports his conclusions by the fact that representatives of the Pacific fauna still live in Lake Titicaca.

NEWS of Herr G. A. Krause, who is now investigating the district between the Gold Coast and Timbuctoo, has reached Berlin. The traveller arrived at Woghodogho, the capital of Mosi, in October 1886. He obtained permission from the King of Mosi to continue his journey in a northerly direction to Duensa, on his way to Timbuctoo. He hoped to reach the former place in seventeen or eighteen days, to arrive at Sarafaram in four or five days more, and then to descend the Niger to Kabara, the port of Timbuctoo. Herr Krause describes the country between Salanga and the capital of Mosi as being perfectly plain at first, and then followed by a district of low hills and another plain. A day's journey north of Walawala, the traveller crossed the Upper Volta, the source of which lies probably in a north-easterly direction.

ON CERTAIN MODERN DEVELOPMENTS OF GRAHAM'S IDEAS CONCERNING THE CONSTITUTION OF MATTER[1]

II.

A QUARTER of a century has elapsed since Graham formulated his conceptions concerning the constitution of matter. I wish now to indicate, as briefly as may be, how these conceptions have developed during these five-and-twenty years.

The idea of the essential unity of matter has a singular fascination for the human mind. It may be that it has its germ in the persistency with which every mind, even that of a child, seeks to get at first principles. The most superficial reader of the history of intellectual evolution cannot fail to perceive how greatly it has modified and directed the development of scientific thought. The whole course of chemistry, for example, has been controlled by this fundamental conception. The half-educated student of to-day may smile at the notion of the transmutation of the metals which held such sway over the minds of the early alchemists, but the men who followed this "*Ignis fatuus*" with weary faltering steps, and who frequently sank under the burden of disappointed hope and the sense that to them it was not given to know the light, felt that this idea rested on a rational basis. They, like we, could give a reason for the faith that was in them. And yet no article of scientific doctrine has in these later times suffered greater vicissitude. Men's ideas concerning the essential unity of things must have received a rude shock when it was found that such a thing as water was not only complex, but was made of bodies strangely contrasted in properties; that the air was still less simple in composition; and that, as it appeared, almost every form of earth could, by torture, be made to give up some dissimilar thing. The brilliant discoveries of Davy, which made the early years of this century an epoch in the history of science, seemed to open out a vista to which there was no conceivable ending. The order of things was not towards simplification: it tended rather towards complexity. And yet Davy himself seemed unable or unwilling to push his way along the path of which the world regarded him as the pioneer. It may be that he was unable to shake himself free from the domination of the schoolmen, or that he unconsciously felt the truth of the principles to which his own discoveries seemed opposed. It is difficult otherwise to account for the tardiness with which he accepted the hypothesis of Dalton; even to the last the Daltonian atom had nothing distinctive to Davy beyond its combining weight. Davy never wholly committed himself to a belief in the indivisibility of the atom: that indivisibility was the very essence of Dalton's creed. In arguing with a friend concerning the principle of multiple proportion, Dalton would clinch the discussion by some such statement as "Thou knows it must be so, for no man can split an atom." Even Thomas Thomson, whom I have already characterised as the

[1] The Triennial "Graham Lecture," given in the Hall of the Andersonian Institution, Glasgow, on March 16, by Prof. T. E. Thorpe, F.R.S. Continued from p. 524.

first great exponent of Dalton's generalisation, was torn by conflicting beliefs until he found peace in the hypothesis of Prout and Meinecke that the atomic weights of all the so-called elements are multiples of a common unit, and which he sought to establish by some of the very worst quantitative determinations to be found in chemical literature. It is curious to note the bondage in which the old metaphysical quibble concerning the divisibility or indivisibility of the atom held the immediate followers of Dalton. Graham, however, never felt such trammels. To him the atom meant something which is not divided: not something which cannot be divided. With Graham, as with Lucretius, the original atom may be far down.

Every philosophic thinker to-day has, I should imagine, come to be of this opinion. Not many years ago it was the fashion to maintain that Stas's great work had for ever demolished the doctrine of the primordial *ylé*, and that Roger Bacon's aphorism that "barley is a horse by possibility, and wheat is a possible man, and man is possible wheat," was henceforth an idle saying. Stas's work is a monument of experimental skill, and it has furnished us with a set of numerical ratios which are among the best determined of any physical constants. It may be that it demolished Prout's hypothesis in its original form, but it has not touched the wider question. Whether indeed the wider question is capable of being reached by direct experiments of the nature of those of Stas is very doubtful, unless the weight of the common atom is some very considerable fraction, say one-half or one-fourth, of that of the hydrogen atom. Dumas has, as you know, modified Prout's hypothesis in this sense, by assuming as the common divisor half the atomic weight of hydrogen, but there is no *a priori* reason why we should stop at this particular subdivision. The exact relation of Stas's work to Prout's law has, I think, been fairly stated by Prof. Mallet at the conclusion of his admirable paper on the atomic weight of aluminium, in the Philosophical Transactions for 1880 (vol. clxxi. 1033). Stas's main result, says Mallet, "is no doubt properly accepted if stated thus, that the differences between the individual determinations of each of sundry atomic weights which have been most carefully examined are distinctly less than their difference, or the difference of their mean from the integer which Prout's law would require. But the inference which Stas himself seems disposed to draw, and which is very commonly taken as the proper conclusion from his results, namely, that Prout's law is disproved, or is not supported by the facts, appears much more open to dispute. It must be remembered that the most careful work which has been done by Stas and others only proves by the close agreement of the results that fortuitous errors have been reduced within narrow limits. It does not prove that all sources of constant error have been avoided, and, indeed, this never can be absolutely proved, as we never can be sure that our knowledge of the substances we are dealing with is complete. Of course, one distinct exception to the assumed law would disprove it, if that exception were itself fully proved, but this is not the case. As suggested by Marignac and Dumas, anyone who will impartially look at the facts can hardly escape the feeling that there must be some reason for the frequent recurrence of atomic weights differing by so little from accordance with the numbers required by the supposed law." Prof. Mallet, in tabulating the atomic weights which may be fairly considered as determined with the greatest attainable precision, or a very near approach thereto, and without dispute as to the methods employed, points out that out of the eighteen numbers so given ten approximate to integers, within a range of variation less than one-tenth of a unit. And he then proceeds to calculate the degree of probability that this is purely accidental, as those hold who carry to the extreme the conclusions of Berzelius and Stas, and he finds that the probability in question is only equal to 1 : 1097·8. And he concludes that not only is Prout's law not as yet absolutely overturned, but that a heavy and apparently increasing weight of probability in its favour, or in favour of some modification of it, exists, and demands consideration.

It would be impossible for me to attempt to traverse the whole ground of this question which has been opened up during the past fifteen or twenty years. Even if I could claim the time and your indulgence, there is hardly the necessity for such a demand on your patience. Mr. Crookes, only so recently as September last, gave an admirably complete exposition of the present state of the case in his address to the Chemical Section of the British Association at the Birmingham meeting, and for me to go over the ground again with you would be simply to plough with Mr. Crookes' heifer. Some years ago Mr. Norman Lockyer, as you doubtless know, approached the subject from another point of view, and in his recent work, "The Chemistry of the Sun," you will find a summary of the evidence which the spectroscope has afforded us concerning the dissociation of "elementary" matter at such transcendental temperatures as we have in the sun.

Now, when we pass in review all this evidence; when we reflect upon the mode of distribution of the elements, and especially their tendency to associate in correlated groups; when we bear in mind the absolute analogy which exists in the general behaviour and mode of action of the radicles which are confessedly compound with those which are assumed to be simple; when we have regard to the phenomena of allotropy, isomerism, and homology,—the mind insensibly appeals to the principle of continuity, and refuses to believe that the seventy and odd "elemental" forms, to which our processes of analysis have reduced all the kinds of matter we see around us, differ in essence from bodies which are known to be compound.

The connexion between the properties of the "elements" and the relative weights of their atoms, as developed by Newlands, Mendelejeff, Lothar Meyer, Carnelley, and others, has served to strengthen this conviction. The discovery that the physical and chemical properties of the elements are as periodic functions of their atomic weights, is unquestionably the most important generalisation we have had in chemical philosophy during the last five-and-twenty years. Its bearings upon the question of the origin of the "elements" have been worked out in the Presidential address I have already referred to. Mr. Crookes, like Mr. Lockyer before him, in seeking to apply to this question of the genesis of the elements the same principles of evolution which Laplace has already applied to the creation of the heavenly bodies, and which Lamarck, Darwin, and Wallace have applied to that of the organic world, is again appealing to the law of continuity. The mind which holds that Nature is one harmonious whole is fain to believe that the probability that the elements have originated by chance and are eternally self-existent is just as remote as that the animals and plants of to-day are primordially created things. I think in what I am now saying I may fairly claim to be reflecting the opinion on this matter of every philosophic thinker of to-day. Nay more, you must allow that the germ which has been kept alive for so many centuries, and which has come down to us through the brains of a succession of thinkers like Leucippus, Aristotle, Lucretius, Bacon, Newton, Dalton, and Graham, has become quickened and endowed by the light which modern science has shed upon it from all sides, with a vitality which will persist and strengthen.

Having thus traced the development of the idea held by Graham of the essential oneness of matter, let us spend the few remaining moments in considering, in the most general way, how the science of the last twenty-five years has worked out and extended his conceptions concerning the properties of the atom and its mode of motion.

The treatment which "the few grand and simple features of the gas," to quote Graham's phrase, have received at the hands of Clausius, Clerk Maxwell, Helmholtz, Sir William Thomson, and a score of workers in this country and on the Continent who have been actuated by their influence, has served to dispel much of the metaphysical fog which has enshrouded the notion of the atom, and to-day we are able to reason about atoms, as physical entities, having extension and figure, and of their number and dimensions and peculiarities of movement, with the confidence which is based on well-ascertained facts.

We have, of course, not yet attained to a complete molecular theory of gases. But we know the relative masses of the molecules of various gases, and we have calculated in miles per second their average velocity. The phenomena of diffusion indicate that the molecules of one and the same gas are all equal in mass. For, as was pointed out by Clerk Maxwell, if they were not, Graham's method of using a porous septum would enable us to separate the molecules of smaller mass from those of greater, as they would stream through porous substances with greater velocity. We should thus be able to separate a gas, say hydrogen, into two portions, having different densities and other physical properties, different combining weights, and probably different chemical properties of other kinds. As no chemist has yet obtained specimens of hydrogen differing in this way from other specimens, we conclude that all the molecules of hydrogen are of sensibly the same mass, and not merely that their mean mass is a statistical constant of great stability (see art. "Atom," "Encyclopædia Britannica," 9th edition). This line of argument has, it seems to me, an important bearing upon a question which has been raised by Marignac, Schutzenberger, and others, and which has again been raised by Mr. Crookes in the address I have already referred to. Mr. Crookes thinks that it may well be questioned whether there is an absolute uniformity in the mass of every ultimate atom of the same chemical element, and that it is probable that our atomic weights merely represent a mean value, around which the actual atomic weights of the atoms vary within certain narrow limits, or in other words that the mean mass is a statistical constant of great stability. The facts of diffusion would seem to lend no support to such a supposition.

Graham was still living when Loschmidt published what Prof. Exner calls his "epoch-making paper" on the "Size of the Air Molecule." Although the numerical estimate which Loschmidt deduced from the mean free path of the molecules and their volume has now only an historical interest, it has exercised a profound influence on the development of molecular physics in demonstrating that in dealing with molecules we are dealing with masses of finite dimensions, and further that these dimensions are by no means immeasurably small. The very manner in which Loschmidt stated his conclusions was well calculated to rivet attention. He showed that these magnitudes, small as they are, are yet comparable with those which can be reached by mechanical skill. The German optician Nobert has ruled lines on a glass plate so close together that it requires the most perfect microscopes to observe the intervals between them; he has drawn, for example, as many as 4000 lines in the breadth of a millimetre, that is about 112,000 lines to the inch. Now, if we assume, with Maxwell, that a cube whose side is 1/4000 of a millimetre is the smallest volume observable at present, it would follow that such a cube would contain from 60 to 100 millions of molecules of oxygen or nitrogen, and if we further assume that the molecules of organised bodies contain on an average 50 "elementary" atoms it further follows that the smallest organised particle visible under the microscope contains about two million molecules of organic matter. And as at least half of every living organism is made up of water, we arrive at the conclusion that the smallest living being visible under the microscope does not contain more than about a million organic molecules. I could have wished, had time permitted, to have dwelt a little upon the intensely interesting questions which such a conclusion at once raises. In the article "Atom" in the present edition of the "Encyclopædia Britannica," from which I have quoted, you will find Clerk Maxwell points out its relation to physiological theories, and especially to the doctrine of Pangenesis. Molecular science, says Maxwell, "forbids the physiologist from imagining that structural details of infinitely small dimensions can furnish an explanation of the infinite variety which exists in the properties and functions of the most minute organisms."

In the year following Graham's death Sir William Thomson still further developed the modes of molecular measurement, and from a variety of considerations based upon the kinetic theory of a gas, upon the thickness of the films of soap-bubbles, and from the electrical contact between copper and zinc, he arrived at estimates which, although sensibly different from that of Loschmidt, are still commensurable with it. In a subsequent lecture to the Royal Institution, given about four years ago, he extended the lines of his argument and arrived at the conclusion that in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than 1/5,000,000 and greater than 1/1,000,000,000 of a centimetre. And in order to give us some conception of the degree of coarse-grainedness implied by this conclusion he asks us to imagine a globe of water or glass as large as a football to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be more coarse-grained than a heap of small shot, but probably less coarse-grained than a heap of footballs (NATURE, vol. xxviii. p. 278).

Here I think we may leave the subject, at all events for tonight. I am painfully conscious that I have left unsaid much that ought to have been said, and possibly said some things that might well have been left unsaid. But my main purpose will have been served if I have succeeded in indicating to you Graham's position as an atomist, and in showing you how his ideas respecting the constitution of matter have germinated, and, like the seed which fell upon good ground, have borne fruit an hundredfold.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 17.—"On the Total Solar Eclipse of August 29, 1886 (Preliminary Account)." By Arthur Schuster, F.R.S.

The instrument intrusted to me by the Eclipse Expedition was similar to that employed in Egypt during the eclipse of 1882. The equatorial stand carried three cameras, one of which was intended for direct photographs of the corona, while the two others were attached to spectroscopes.

Photographs of the Corona.—The lens had an aperture of 4 inches, and a focal length of 5 feet 3 inches; giving images of the moon having a diameter of about 0·6 of an inch.

During the first minute of totality the corona was covered by a cloud, which was, however, sufficiently transparent to allow the brightest parts of the corona to show on the ten photographs exposed during that time.

During the remaining time, that is to say, during about two minutes and a half, the sky was clear, but there were clouds in the neighbourhood of the sun.

The time of exposing the photographs which had been fixed beforehand had to be altered in consequence of the uncertainty of the weather, and for this reason I can only give the actual times of exposures very approximately and from memory. One photograph on sensitive paper shows only little detail; but three photographs on glass were obtained, which, as regards definition, I believe to be equal to those obtained in Egypt. The approximate exposures were 15 to 20 seconds, 10 to 15 seconds, and about 5 seconds.

With the view of possibly increasing the amount of detail which it has hitherto been possible to obtain on the photographs of the corona, I have, on this occasion, given considerable attention to the different adjustments, so as to fix the cause which at present limits the definition, and I have come to the conclusion that, if we are to obtain better photographs of the corona, we can only hope to do so by means of a better mechanical arrangement for moving the camera.

Photographs of the spectrum of the corona were obtained by means of two instruments, one being identical with that employed at Caroline Island in 1883. This spectroscope has two prisms of 62° refracting angle, the theoretical resolving power being about 10 in the yellow. (The unit of resolving power is the resolving power which allows of the separation of two lines differing by the thousandth part of their own wave-length.) The slit of this spectroscope was placed so that it was tangential to the image of the sun formed by the condensing lens. One plate was exposed during the whole of totality. The results are good; a number of lines belonging to the prominences and to the corona are very distinct and can be measured with fair accuracy. The difficulty of measurement lies in the multitude of lines. I have measured at present between forty and fifty distinct corona lines between the solar lines F and H, and a number remain unmeasured.

A comparison between the photographs of 1882 and 1886 shows that, although the lines seem to be in the same position, their relative intensity has greatly altered. The strongest corona line during the last eclipse had a wave-length of about 4232; it is slightly but distinctly less refrangible than the strong calcium line at 4226.

The second spectroscope had its slit placed so as to take a radial section of the corona. It had one large prism giving a theoretical resolving power of 11·4; slightly larger therefore than the two-prism spectroscope.

The film was one prepared by Capt. Abney so as to be more sensitive in the green than the ordinary plates.

The photograph obtained is faint, but I believe will ultimately give good results.

A good drawing of the corona was obtained by Capt. Maling at the station occupied by Capt. Darwin and myself.

The plates were prepared by Capt. Abney, whose valuable help I have had in the whole of the preliminary arrangements.

March 24.—"Preliminary Note on the 'Radio-Micrometer,' a New Instrument for measuring the most Feeble Radiation."[1] By C. Vernon Boys.

The author considered that, if the thermo-electric power of a

[1] I have learnt that an instrument essentially the same in principle as the radio-micrometer was made and shown by M. D'Arsonval to the French Physical Society; it is hardly necessary to say that I was not aware of this before reading the paper.—C. V. B.