was so. The remains of primitive architecture in Greece—particularly at Tiryus—show that wood must have entered largely into architectural constructions; amongst other evidences, the traces of wooden door-cases cannot be explained away. Perrot and Chipiez, with whatever amount of fancifulness there may be (and there is no doubt much which is altogether hypothetical) in their restorations, do come legitimately to an explanation of the Doric guttæ both under the triglyphs and beneath the mutules, as typical of the ends of wooden pegs or trenails in timber construction, which is sufficient for the argument in the review, in which there was no intention to approve Perrot and Chipiez' restorations and deductions any further than that.

(2) As to the second objection taken to the review—the remark respecting the cupola of St. Paul's. The remark in the review had reference to the objection that the external outline of the dome was distinct from the internal, and not to the question of support of the lantern; but with reference to the latter point, when the lantern of St. Peter's is quoted as supported by a more legitimate construction than that of St. Paul's, it may be asked: Why the construction of St. Peter's dome, which is absolutely dependent for its safety on the iron chains by which it is hooped together, is preferable to that of St. Paul's, where the lantern has a much securer, and therefore not less legitimate, support in Sir Christopher Wren's cone?

(3) One remark only on the objection raised to the style of Milan Cathedral. The detail is said to be wretched. That it does not conform to the canon of Northern Gothic can be readily conceded, but that the shafts of the magnificent forest of pillars which support the interior are wretchedly designed, and unsuitable to the intended effect, is not so easy to admit.

THE REVIEWER.

Do the Components of Compound Colours in Nature follow a Law of Multiple Proportions?

THIS question, put by Mr. F. Howard Collins in NATURE (p. 438), may be answered in the negative.

In practical work there is no indication of such a law. It is found that the two rays, which together produce a compound natural colour, may be in any proportions; when there is a multiple proportion, and in some cases there must be, it is only as forming part of a series of variations, such as are frequently found within the limits of a single popular colour term. How wide these proportions may be, can be illustrated by comparing them to the varying proportions of two irregular curves towards each other.

The examples of foliage quoted can only be taken as representing individual instances. Variations of climate, age, cultivation, and aspect alter the colour proportions of a given variety of leaf; indeed, such variations are sometimes found in the same leaf.

JOSEPH W. LOVIBOND.

Salisbury, September 23.

IN view of the letters, recently printed in NATURE, by Mr. H. H. Pillsbury and Mr. Herbert Spencer, it may be well to state that Chevreul published an "Exposé d'un moyen de définir et de nommer les couleur d'après une méthode précise et expérimentale" (Paris, 1861, also *Mem. de l'Acad.* xxxiii.), in which elaborate charts are given showing the colours defined by a decimal system and in ten degrees of saturation.

Recently Prof. W. Hallock, of this College, has painted discs with standard colours, and determined their wave-lengths with the spectroscope. These discs were then used to study 6000 samples of coloured objects, and formulæ were determined for some 500 named colours. These formulæ have been used for defining the names of colours in the new "Standard Dictionary" (Funk and Wagnall's, New York).

J. MCKEEN CATTELL.

Columbia College, New York, September 20.

A Problem in Thermodynamics.

IT may interest some of your readers to know that the problem in thermodynamics, propounded by Mr. Blass in your number of August 29, has actually been put to the test. I pointed out Mr. Blass's letter to my brother, who is a freezing engineer, and he showed me a copy of the *Zeitschrift für die Gesammelte Kälte-Industrie* (Munich) for August, in which an account is given of a machine on exactly the principle Mr. Blass suggests, by which Herr Linde has succeeded in liquefying air. It would appear, therefore, that the "theoretical minimum of temperature produced at C" would be determined by the point of liquefaction of the gas employed; with a perfect unliquefiable gas it would, I suppose, theoretically, be absolute zero.

EDWARD T. DIXON.

Cambridge, September 22.

THE NEW MINERAL GASES.

OUR knowledge of the spectra and other conditionings of the new mineral gases has received an important addition in the communication from Drs. Runge and Paschen which appeared in last week's NATURE. The employment of exposures extending over seven hours has given a considerable extension in the number of lines, and the bolometer has been called in to investigate lines in the infra-red; better still, they have employed well-practised hands in searching for series of lines. Operating, by chemical means, upon a crystal of clèveite free from any other mineral, they have obtained a product so pure that from these series there are no outstanding lines. Very great weight, therefore, must be attached to their conclusions, and there are several points of contact with the work upon which I have been engaged from a slightly different stand-point since last April, when Prof. Ramsay made his fortunate discovery of a terrestrial source of helium.

I will touch upon some of these points *seriatim*.

In the first place, there has never been the slightest doubt in my mind that it was a question of gases and not of a gas. The spectroscopic evidence in the laboratory alone was complete, and the case was greatly strengthened when the behaviour of the various lines in the sun and stars was also brought into evidence. Drs. Runge and Paschen also declare that the gas given off even by a pure crystal of clèveite is not simple, but consists of two constituents. To the one containing the line D_3, which I discovered in 1868, the name helium remains; the other for the present, we may call "gas X." The chief lines of these two constituents are as follows, according to Runge and Paschen:

Helium.	Gas X.
5876	6678
4713	5048
4472	5016
4026	4922
3889	

Last May I wrote as follows[1]:—

"The preliminary reconnaissance suggests that the gas obtained from bröggerite, by my method, is one of complex origin.

"I now proceed to show that the same conclusion holds good for the gases obtained by Profs. Ramsay and Clève from clèveite.

"For this purpose, as the final measures of the lines of the gas as obtained from clèveite by Profs. Ramsay and Clève have not yet been published, I take those given by Crookes, and Clève, as observed by Thalén.

"The most definite and striking result so far obtained is that in the spectra of the minerals giving the yellow line I have so far examined, I have never once seen the lines recorded by Crookes and Thalén in the blue. This demonstrates that the gas obtained from certain specimens of clèveite by chemical methods is vastly different from that obtained by my method from certain specimens of bröggerite, and since from the point of view of the blue lines, the spectrum of the gas obtained from clèveite is more complex than that of bröggerite, the gas itself cannot be more simple.

"Even the blue lines themselves, instead of appearing

1 *Proc.* Roy. Soc., vol. lviii. p. 114.

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en bloc, vary enormously in the sun, the appearances being—

4922 (4921·3) = 30 times.
4713 (4712·5) = twice.

"These are not the only facts which can be adduced to suggest that the gas from clèveite is as complex as that from bröggerite, but while, on the one hand, the simple nature of the gases obtained by Profs. Ramsay and Clève, and by myself, must be given up, reasoning on spectroscopic lines, the observations I have already made on several minerals indicate that the gases composing the mixtures are by no means the only ones we may hope to obtain."

It will be seen that the laboratory separation of D_3 from the lines 5048, 5016, and 4922 was complete, and we now know that they belong to different series.

Later on, in the same month, I returned to this subject, and showed that the lines at D_3 and 447 behaved in one way, and that at 667 behaved in another.

I wrote as follows [1] :—

"(1) In a simple gas like hydrogen, when the tension of the electric current given by an induction coil is increased, by inserting first a jar, and then an air-break into the circuit, the effect is to increase the brilliancy and the breadth of all the lines, the brilliancy and breadth being greatest when the longest air-break is used.

"(2) Contrariwise, when we are dealing with a known compound gas; at the lowest tension we may get the complete spectrum of the compound without any trace of its constituents, and we may then, by increasing the tension, gradually bring in the lines of the constituents, until, when complete dissociation is finally reached, the spectrum of the compound itself disappears.

"Working on these lines, the spectrum of the spark at atmospheric pressure, passing through the gas, or gases, distilled from bröggerite, has been studied with reference to the special lines C (hydrogen), D_3, 667, and 447.

"The first result is that all the lines do not vary equally, as they should do if we were dealing with a simple gas.

"The second result is that at the lowest tension 667 is relatively more brilliant than the other lines; on increasing the tension, C and D_3 considerably increase their brilliancy, 667 relatively and absolutely becoming more feeble, while 447, seen easily as a narrow line at low tension, is almost broadened out into invisibility as the tension is increased in some of the tubes, or is greatly brightened as well as broadened in others (Fig. 1).

[1] *Proc.* Roy. Soc., vol. lviii. p. 193.

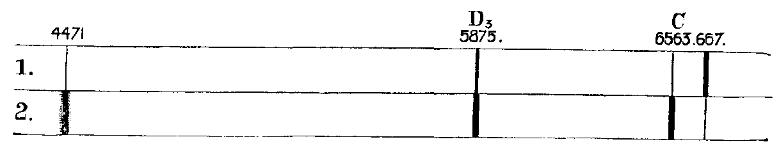

FIG. 1.—Diagram showing changes in intensities of lines brought about by varying the tension of the spark. (1) Without air-break. (2) With air-break.

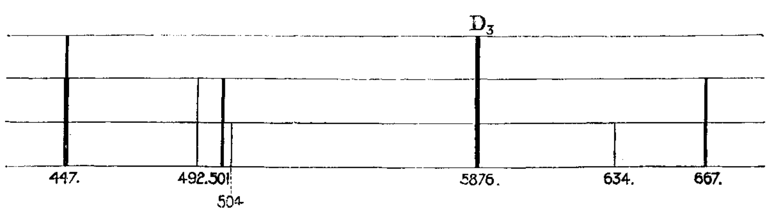

FIG. 2.—Diagram showing the order in which the lines appear in spectrum when bröggerite is heated.

These lines have now been differentiated by Runge and Paschen by a different but equally satisfactory method.

Nor is this all. The difference between the results obtained by Thalén and myself seemed susceptible of explanation by admitting a fractional distillation, according to which D_3 and 447 came off first, and 4922, 5016, and 667 later on (Fig. 2).

Here also I got the same result as in the diffusion experiment referred to by Drs. Runge and Paschen. They found similarly—

Less bright.	More bright.
D_3	5016
	6678

All these various lines of evidence tend therefore to complexity, and there can be little doubt from the convergence of all these lines of work, the results of which agree among themselves, that we are in presence of at least two distinct gases, the complete spectra of which are those given by Drs. Runge and Paschen.

The second point is that there is no connection whatever between either of these gases and argon. Argon is of the earth, earthy, but helium and gas X are distinctly celestial, even more celestial than I thought when I claimed for them last May [1] the dignity of "a new order of gases of the highest importance to celestial chemistry." It was supposed at first that the spectra contained any number of common lines, next that there were two coincidences in the red between the new gases and argon; one I found broke down with moderate dispersion, the other has yielded to the still greater dispersion employed by Drs. Runge and Paschen; and, more than this, I have not found a single coincidence between argon and any line in the spectrum of any celestial body whatever. This happens, as everybody knows, also in the case of oxygen, nitrogen, chlorine, and the like.

The third point is as follows. So far I have worked upon some eighty minerals, and I have found the yellow line in sixteen; among the lines which I have already reported to the Royal Society are included all the stronger ones in the various series determined by the German physicists, but I can now add that in the region over which my work has extended, there is scarcely a single line in their series which I have not either seen or photographed in the spectrum of some celestial body or another. The following tables will show the results I have already obtained with all the six series of lines indicated by Drs. Runge and Paschen.

[1] *Proc.* Roy. Soc., vol. lviii. p. 117.

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HELIUM.

11220	Sun.		Star or Nebula.
3889	C	E	N. III. γ
3188			
2945			
2829			
2764 *			
2723			
2696			
2677			
5876	C 100	E	
4472	C 100	E	
4026	C 25	E	
3820		E	α Cygni
3705			
3634			
3587			
3555			
3513			
3499 *			
3488			
3479			
3472			
3466			
3461			
7066	C 100		
4713	C 2	E	
4121		E	N. α Cygni
3868		?	
3777		E	Bellatrix
3652			
3599			
3567			
3537 *			
3517			
3503			
3491			
3482			

GAS X.

	Sun.		Star or Nebula.
5016	C 30	E	
3965		?	III. γ
3614		E	
3448			
3355			
3297 *			
3258			
3231			
3213			
6678	C 25		
4922	C 30	E	
4388		E	N. III. γ
4144		E	III. γ
4009			III. γ
3927			Bellatrix
3872			Bellatrix
3833		E	Hid by H line
3806			Bellatrix
3785 *			
7282			
5048	C 2		
4438			Bellatrix
4169			Bellatrix
4024		?	N. III. γ
3936	Hid in K.		
3878	C	E	α Cygni
3838	C	E	α Cygni
3803 *			

* Means that these lines are out of the range of my observations.

In the tables, under "Sun," C, followed by a number, indicates the frequency as given by Young; E indicates the lines photographed during the eclipse of 1893. Under "star or nebula" the references are to the tables given in my memoir on the nebula of Orion (*Phil. Trans.* vol. clxxxvi. (1895), p. 86 *et seq.* N = Nebula of Orion).

Hydrogen, helium, and gas X are thus proved to be those elements which are, we may say, completely represented in the hottest stars and in the hottest part of the sun that we can get at. Here then, in 1895, we have abundant confirmation of the views I put forward in 1868 as to the close connection between helium and hydrogen.

J. NORMAN LOCKYER.

RESEARCH IN ZOOLOGY AT OXFORD.[1]

THE second volume of the Linacre Reports, which has lately been printed, shows that the zoological laboratory at Oxford continues to be a source of production of many interesting and valuable contributions to knowledge.

In the course of a little more than one year the colleagues and pupils of Prof. Lankester have published a number of memoirs and essays, which, when collected together, form a bulky octavo volume, illustrated by numerous lithographs and woodcuts.

There is, as might be expected, considerable range in the interest and importance of the several items composing the volume, but not one of them could have been omitted without lessening its value to the zoologist. At least four of the memoirs are of such importance that they may be considered to be standard works to which reference must be frequently made in future by naturalists of all nationalities. Of these, perhaps, the most important is Prof. Poulton's memoir on the structure of the hair and bill of the duck-billed Platypus, which contains not only an excellent account of certain histological features of this rare animal, but some extremely suggestive remarks, derived from this research, on the relations of hairs and scales.

Dr. Benham's beautifully illustrated essay on the brain of the interesting Chimpanzee "Sally," which recently lived and died in the Zoological Gardens in London, forms an important chapter in "Man's place in Nature." The careful comparison which Dr. Benham gives of the large and valuable series of anthropoid and human brains which he has examined, makes this memoir one of special interest and importance.

Mr. Bourne's monograph on the post-embryonic development of Fungia gives us, at last, detailed information and good illustrations of a subject which has long interested zoologists.

The description of Prof. Lankester's collection of the species of Amphioxus and the genera allied to it, which has been carefully and ably written by Miss Kirkaldy, forms a memoir which will be welcomed heartily by zoologists in all civilised countries.

The other contributions to this volume are of less importance, perhaps, than those referred to above, but they are all useful additions to our knowledge of many widely separated branches of zoology, and being carefully written, and the result of work done under excellent advice and guidance, cannot be neglected by those who are specially interested in the branches of zoology of which they treat.

With such a volume of good useful work before us, it is truly lamentable to read in Prof. Lankester's editorial preface of the general indifference prevailing in the governing bodies of the Oxford colleges towards the progress of natural knowledge. The University of Oxford and the colleges together are the possessors of very large endowments for the cultivation of learning in all its branches. No university in the empire is so fortunately

1 "The Linacre Reports." Vol. ii.

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