of nitrate of uranium, and possibly the fluorescent material in the asphalt, which may be related to the solid hydrocarbon fluorescing green which Becquerel mentions (*La Lumière*, tome i. p. 382).

In this general connexion let me mention that I have observed that while the acid salts of quinine generally are fluorescent, the chloride is not, and that hydrochloric acid will decompose the acid sulphate so as to destroy its fluorescence.

There are several other points in connexion with this and the foregoing subject, which I must leave for a subsequent discussion.

P.S.—August 1. I have just obtained results with turmeric which seem to indicate that its fluorescence is due to the presence of a substance not yet observed, soluble in water, and without any colour.—Silliman's *American Journal*, September, 1871.

ON THE SPECTRA OF THE SIMPLE GASES.
BY M. A.-J. ÅNGSTRÖM.

In the *Recherches sur le Spectre solaire* which I published in 1868, I already announced that the spectrum-observations to which I had devoted myself had not convinced me of the correctness of the opinion of Plücker that one and the same gas, in the state of incandescence, could give spectra varying with its temperature. I rather believed that in the appearance of the spectra a modification may be observed which consists in the elevation of the temperature bringing about a greater abundance of lines, and that the relative luminous intensity of these lines may also undergo some changes, but that nevertheless the spectrum preserves its character unaltered. It is true that in disruptive discharges it happens, when the tension of the gas is increasing, that the spectral lines spread, and even end by uniting so as to form a continuous spectrum; but even then one cannot say that the result is a new spectrum.

Several distinguished physicists, however, are of the opposite opinion; and probably the researches of M. Wüllner (according to which hydrogen would have no less than four spectra, oxygen three) have strengthened this conviction in the minds of many savants. M. Dubrunfaut has expressed his doubts of the correctness of these results: he remarks that the multiple spectra of oxygen and hydrogen may be due to nitrogen or mercury-vapour introduced by the pump into the tubes. M. Wüllner, however, has shown (*Comptes Rendus*, Jan. 17, 1870) that this explanation is inadmissible.

Yet, as the question of the multiple spectra of the gases is a vital one for spectral analysis, and in this light M. Wüllner's observations are truly important, perchance the following analysis of the phenomena observed will not be without interest. Permit me to commence with a preliminary remark. According to the experience acquired, at least by me, the results obtained concerning the spectra of the gases are not absolutely sure when the rarefaction is carried to its utmost limits. In proof of this I cite the following fact:—On one occasion, when I rarefied as much as possible, by means of a mercurial pump, the atmospheric air in a Geissler's tube, at the same time causing the discharge of a Ruhmhorff coil to pass in the tube, I obtained in succession the following spectra:—1, the ordinary air-spectrum; 2, the fluted spectrum of nitrogen: 3, that of carbonic oxide; 4, when the rarefaction was at its maximum, the lines of sodium and chlorine. If to this we add that when a mercurial pump is employed the mercury-lines may present themselves, just as those of sulphur may when sulphuric acid is used to dry the gas, the result may easily be a multiplicity of spectra which it would be wrong to attribute to one and the same gas.

As far as I know, I was the first to observe (in 1853) the spectrum of hydrogen. Using on that occasion a Leyden jar to produce incandescence of that gas, which was at the pressure of the atmosphere, I obtained a spectrum consisting of an intense line in C not clearly limited, and two maxima of light in F and G; the third maximum, in *h*, was only observed later. Afterwards Plücker found that by operating with rarefied hydrogen a spectrum is obtained with clearly determined lines. It may thus be regarded as a fact long known, that the spectrum-lines of hydrogen spread when the discharge becomes disruptive, and that they end, when the tension of the gas is augmented, by forming a continuous spectrum. M. Wüllner's spectrum No. 4, then, is only the ordinary spectrum of hydrogen.

Plücker was the first who indicated a second spectrum for hydrogen, principally characterized by a multitude of lines on both sides of D and towards C. This spectrum generally appears simultaneously with the preceding, but is distinguished from it by several important characters. By causing the discharge of a Ruhmkorff coil to pass in a Geissler's tube containing rarefied hydrogen, in a revolving mirror two images of the incandescent gas are obtained, which correspond to the two spectra: one of them appears as an isolated line, indicating that the light there is of very short duration; the other, on the contrary, widens into a zone traversed horizontally by striæ alternately bright and dark. It is necessary, in this experiment, to regard the Geissler's tube and the axis of rotation of the mirror as placed vertically. The duration of the last image, in one experiment, was from 5 to 6 thousandths of a second *.

This image disappears immediately the discharge is made disruptive by the addition of a condenser. This property, as well as the stratification of the light which accompanies it, indicates that we have here a combination of hydrogen, either with itself or a foreign body; the latter is the most probable. M. Berthelot has published, in the *Comptes Rendus*, some observations on a spectrum which he obtained by means of a combination of hydrogen and benzole. He submits that this spectrum belongs to *acetylene*, and that it has not been previously observed. This, however, is not the case; having repeated M. Berthelot's experiments with some benzole, I ascertained that the spectrum obtained is no other than M. Wüllner's hydrogen-spectrum No. 2. Still, if (as M. Berthelot has shown)

* To determine the duration of an image, I used M. Kœnig's flames. By projecting simultaneously on the revolving mirror the flame agitated by the pipe, we have a scale by means of which we can easily determine the duration of the luminous phenomena when that duration is very short.

acetylene mixed with a sufficient quantity of hydrogen remains unaltered in a Geissler's tube, so that a decomposition, if produced, is always accompanied by a corresponding combination, there is nothing to prevent us from admitting that the hydrogen-spectrum No. 2 belongs to *acetylene*.

I pass now to the third of the spectra which M. Wüllner thinks he has found for hydrogen. This spectrum, which would be quite new if it belonged to the gas in question, is in all probability only that of *sulphur*. This, I believe, is most positively demonstrated by the following Table, which contains the wave-lengths of the vapour of sulphur and those determined by M. Wüllner for this third spectrum of hydrogen. The differences met with in the two series are easily explained by this consideration—that the wave-lengths cited, both for sulphur and for M. Wüllner's spectrum, have not all the accuracy desirable.

Supposed Spectrum of Hydrogen.		Sulphur-Spectrum.
1. Group of three lines; the middle line	5647	5671 5645 5613
2. Group of three lines; the middle line	5469	5474 5451 5432
3. Group of two lines; the second line......	5334	5345 5322
4. Group of two series; the first line........	5221	5207 5191
5. Group of three lines; the middle line	5015	5027 5013 4994
6. Group of more than six lines; the middle line.	4930	4926

I persist, therefore, in the opinion that hydrogen has only one spectrum—the one found in the light of the sun and of the stars.

Beside the known spectrum of oxygen, M. Wüllner has observed two new ones, which, for brevity, we will designate by the numbers 2 and 3. According to the description given of No. 2, it is composed principally of four shaded bands with clean edges on the side towards the red field of the spectrum. In order to obtain a more exact idea of the position of the bands, I constructed the spectrum itself with the aid of the minima of deviation given by M. Wüllner. I found that it presented much analogy with the spectrum of the oxide of carbon. I afterwards determined the wave-lengths of the four bands, by construction and with the aid of the wave-lengths already calculated by M. Wüllner. The following Table gives the values obtained, and the wave-lengths for the oxide of carbon:—

Supposed Spectrum of Oxygen, No. 2.		Spectrum of the oxide of carbon.
1. First band	5620	5609
2. Second band	5203	5196
3. Third band	4835	4834
4. Fourth band	4506	4510

As is seen, the agreement is perfectly satisfactory; let us add that it is equally so in the details, such as the presence of a fainter shaded line between 1 and 2, and of two shaded bands in the red field. Hence it is impossible to doubt that the spectrum No. 2 belongs to the *oxide of carbon.*

There now remains spectrum No. 3. This no more than the other belongs to oxygen. Perhaps some lines of the known spectrum of oxygen are found in it; but most of the lines belong to *chlorine.* This assertion is put beyond doubt by the following Table, which contains the wave-lengths of the chlorine-spectrum and those of spectrum No. 3.

Supposed Spectrum of Oxygen, No. 3.

1. A large group	the first line ..	5461	5460	Chlorine.
	the last line ..	5404	5399	
2. A large group	the first line ..	5215	5213	
	the middle line, very bright..	5152		Oxygen?
	the last line ..	5090		
3. Group of six series.	the first	4938	4940	
	the last	4893	4895	
4. A large group .. .	the last line ..	4805	4820 4808 4793	Chlorine.
5. Group of three lines	the first......	4652	4647	
	the second....	4644	4642	Oxygen.
	the third	4637	4630	Chlorine.
6. A violet line		4418	4417	Oxygen.
7. A violet line		4621	?	

The result of the above examination will therefore be, that we know not yet any other oxygen-spectrum than the one observed by me in 1853, and which has subsequently been studied with great care by Plücker.

I ask permission to add a few words on the action exerted by magnetism on the spectra of gases; these considerations have an intimate connexion with what precedes. Under the influence of this action the spectrum assumes, according to M. Trève, a quite different aspect; so that we should be able to produce not merely by a rise of temperature, but also by magnetism, the multiple spectra which, in the opinion of divers savants, present themselves with gases. This is correct in several respects; but the explanation of the phenomenon appears to me to be different from that which has been given of it. In fact the modification in the appearance of the spectra depends simply on this—that *the action of magnetism causes, at the incandescent state, the occurrence of other substances or other combinations.* In certain cases the effect of magnetism may be compared almost to that which is produced by the addition of a condenser to the Ruhmkorff coil; but magnetism appears also to exert a sort of chemical action, obstructing the production of certain combinations, and facilitating the production of others.

Thus a Geissler's tube has given, between the poles of an electro-magnet, the ordinary spectrum of carburetted hydrogen, whereas, without the intervention of magnetism, it gave the carbonic-oxide spectrum without the lines of hydrogen being visible.

In another tube, filled with hydrogen obtained by the decomposition of water and dried with sulphuric acid, which gave Plücker's two hydrogen-spectra, under the influence of magnetism there appeared those sulphur-lines which M. Wüllner has regarded as forming the hydrogen-spectrum No. 3, while the spectrum of carbonic oxide was shown on the polar wires.

It would doubtless be premature to endeavour to formulate a law according to which these changes take place; but a positive fact is, that they do not appear to introduce any new spectrum peculiar to the action of the magnetic forces.—*Comptes Rendus*, August 7, 1871.

ON THE TESTIMONY OF THE SPECTROSCOPE TO THE TRUTH OF THE NEBULAR HYPOTHESIS. BY PROFESSOR DANIEL KIRKWOOD, OF BLOOMINGTON, INDIANA.

In March 1846 the partial resolution of the great nebula in Orion was announced by Lord Rosse. In September of the following year the late Professor W. C. Bond, of Harvard University, stated, in confirmation of this interesting discovery, that the part of the nebula about the Trapezium "was resolved into bright points of light" by the great refractor of Cambridge. "It should be borne in mind," continued Professor Bond, "that this nebula and that of Andromeda have been the last stronghold of the nebular theory—that is, the idea, first thrown out by the elder Herschel, of masses of nebulous matter in process of condensation into systems."

These grand achievements were regarded by the majority of astronomers as fatal to the claims of the nebular hypothesis. It is not to be denied, however, that this celebrated theory has more than recovered from the shock which it then received, that it has, in fact, been materially strengthened by the researches and discoveries of the last twenty years. The truth of this remark is strikingly exemplified by the revelations of the spectroscope. The man who at the middle of the nineteenth century would have been bold enough to predict the discovery of the physical constitution of the heavenly bodies, or the determination of the elements of which they are composed, would have been generally deemed a scientific enthusiast. This, however, and more than this, has been actually accomplished. In the hands of Huggins, Secchi, Young, and others the *spectroscope*, that marvel of modern science, has yielded satisfactory testimony, not only in regard to such stars as are reached by our unassisted vision, but even respecting the telescopic nebulæ, apparently on the outskirts of the visible creation. A detailed account of these wonderful achievements would not comport with our present purpose. Such *results*, however, as bear directly upon the theory of Laplace will be briefly noted.