THE SUN'S PLACE IN NATURE.[1]

IV.

THE difference in the appearance of spectral lines and flutings having been explained, I now go on to state that the luminosity referred to, as seen in the meteorite experiment was not one of the lines in the spectrum of magnesium, but one of the flutings. I will next throw this on the screen (Fig. 17), and you will at once see the point. Here is the spectrum of magnesium obtained at the lowest temperature at which we can get any light from it at all. We have a fluting, which resembles closely the carbon fluting, but in a different part of the spectrum. We see that its brightest part is coincident with a certain part of the solar spectrum; and it so happens that the position of the line which Dr. Huggins had observed in the nebulæ lies very near the same position of the solar spectrum.

That, then, was one argument out of a great number in favour of the view that the luminosity to which the bright line of the nebula was due, might really be produced in the nebulæ by collisions of meteorites among themselves, rendering luminous the vapours of magnesium which we knew to be wherever there are meteorites.

Now, an additional argument for that view was found in the fact that almost every observer, including Dr. Huggins himself, had stated that as seen in the spectrum of a nebula the line did look somewhat different on one side to what it did on the other, and references were made to its being more degraded on the left-hand side than it was on the right. I had frequently

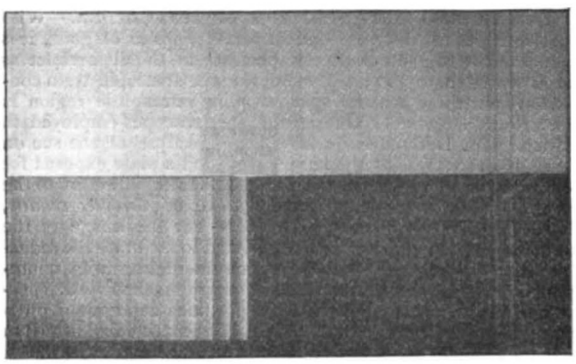

FIG. 17.—Spectra of burning magnesium compared with solar spectrum. (1) Sun. (2) Magnesium.

observed myself that the line representing the chief line of the nebulæ was degraded to the left, never to the right, over parts of the nebula of Orion which were more brilliant than the others; and at the same time that another line—about which more presently—instead of being degraded to the left, like this one, was equally eased off on both sides (Fig. 18), so that the argument was complete that the appearances presented by this line were not due to any instrumental defect, because, in that case, all the lines would have behaved in the same abnormal manner. Hence then I found myself justified in concluding and subsequently stating (1) that the position of the meteoritic dust-line was coincident with the line of the nebulæ in the apparatus which I used, and (2) that it resembled it in appearance.

What I had next to do in the matter was, of course, to carry the thing as far towards the truth as I could. We can never find out the whole truth, but it is better to have a part of the truth than none at all. Hence I started a new method of attack, which, you will note, differs very considerably from anything you have seen before. I have here a beautiful instrument invented by that eminent Frenchman Foucault, called a "siderostat." The essential part is a plane mirror (Fig. 19) which, when it is properly adjusted to the sun or moon or any star in any part of the sky, lays hold of a beam of light from it about twelve inches in diameter, and sends that beam in a horizontal direction due south, and keeps it there; so that the light falls fairly on the optical apparatus, and we can go on observing it for a long time. Next the instrument was adjusted to throw the light from the nebula of Orion on a powerful horizontal telescope placed in front of a large spectroscope, both rigidly fixed. In order to check the observations as far as possible, I placed in front of the object-glass of the telescope an arrangement by which the light from a magnesium wire might enter the slit of the spectroscope at the same time as the light of the nebula, so that if the light from the nebula and the light from the magnesium wire perfectly agreed in wave-length, we should get one line; if it differed, we should get two.

The slit of this spectroscope was exactly in the focus of the ten-inch object-glass, and then the light was passed through four dense prisms, so that we got a considerable amount of dispersion, and the exact position of the line, whether single or double, was observed. That of course was a very much more powerful dispersion than had been employed by Dr. Huggins in his first observations, and much more powerful than had been employed by myself in my first investigation. But what I wished to do in those first investigations was to understand and to clearly follow the observations which had been made previously by others; if therefore I had attempted to go over the ground with instruments ten times better, giving me ten times finer results than my predecessors had obtained, it would have been the worst possible way to go to work, because it was essential for me to make the necessary comparisons with the old observations while not exceeding the instrumental means which had been employed to obtain them.

The long and short of my various methods of observation was that they seemed entirely to confirm the idea which I got in the first instance from using telescopes and spectroscopes of very much smaller power. That, however, fortunately for

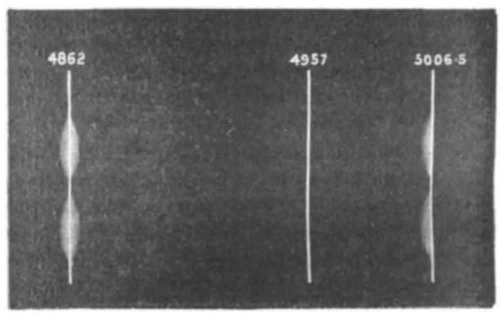

FIG. 18.—Appearance of principal lines in spectrum of Orion nebula, as observed at Westgate-on-Sea.

science, did not satisfy Dr. Huggins; he very wisely appealed to the American astronomers, and I am glad to say that the skilful astronomers of the Lick Observatory took up this work with interest, and employed instruments in the investigation more powerful than any I possessed, thus carrying matters a stage further. There were really two distinct bits of work to be done: first of all, one wanted the exact position of the line in the nebulæ, and after having got its right position, its origin could be thought out. We wanted also to see what the real physical appearance of the line was, *i.e.* whether it was most likely a line or a fluting. It is not a little curious to note that all the statements which had been made suggesting a fluted character of the line were at once withdrawn when I referred its origin to the magnesium fluting.

The Lick telescope is one of very considerable power indeed, and it is so solidly built that a very powerful spectroscope can be put on one end of it and used under almost the best possible conditions for determining the position of lines. Still the Lick telescope is not the best possible telescope to employ for any branch of work connected with nebulæ, if the work requires a great amount of light, because the longer the telescope, the larger the image which the object-glass gives; for instance, if you are dealing with a nebula one degree in diameter, if your one degree is written on a circle with a radius of sixty feet, it will be a very much bigger thing than if on a radius of ten feet, so you get a large image without increasing the light, and therefore are spreading your light over a very large area. As the slit of the spectroscope is a very small thing, all the light which is thrown outside the slit is of no use for your spectroscopic observation, so, whatever the size of the spectroscope may be, you want to deal with the smallest and

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 567.

brightest possible image in order to get the best use out of your spectroscope, and that cannot be done with a long focus telescope. However, the important question for the American observers was to determine the exact position of the line; and we have lately been given some very interesting results.

Fig. 20 represents the way in which Dr. Keeler puts his last result. The upper part is a representation of the solar spectrum; the numbers represent wave-lengths on Rowland's scale. According to his latest value the wave-length of the nebular line is 5007·05. He also shows in relation to it the lines of nitrogen as well as the fluting of magnesium, and you see at once that, although according to this drawing the magnesium does not quite correspond with the line of the nebulæ, it is very much nearer to it than is either the line of lead or the lines of nitrogen. The publication of this result necessitated a fresh investigation, to see what the exact facts were when we no longer compared the nebula with magnesium, but compared the magnesium with the solar spectrum, and therefore sought the true position in which to place the magnesium line in relation to the solar spectrum.

Here is the result. You will see that there is a very small difference between the position of the magnesium fluting and the nebular line. In short, the more the work done on this question the more and more coincident have these lines become, and there are some considerations which have not yet been taken into account.

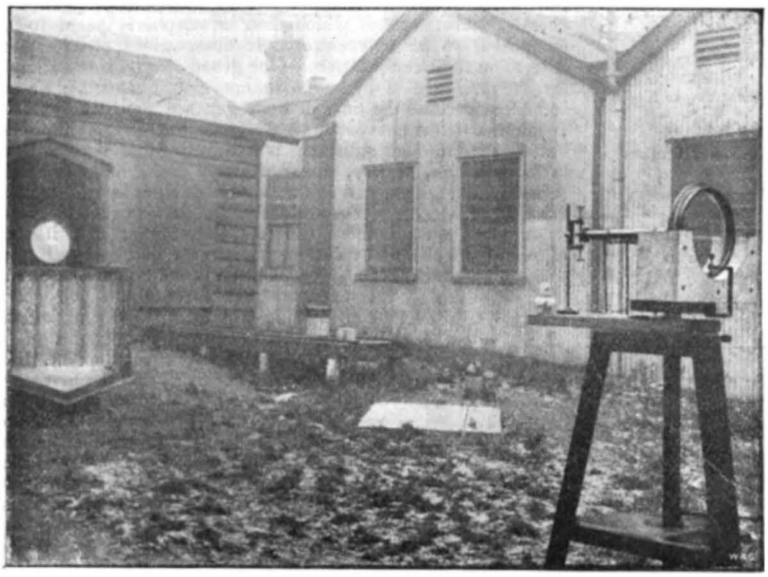

FIG. 19.—Showing object-glass of horizontal telescope used with siderostat.

I have referred to this point somewhat at length, although the coincidence with the fluting of magnesium is not fundamental for the establishment of the view which even Dr. Huggins has now accepted, in the way I have already stated.

Now, what was the chemical constitution of the nebulæ stated to be as a result of the first spectroscopic observations? Dr. Huggins, in his paper of 1865, to which I have already referred, was of opinion that the chief line was due to nitrogen.

Here are two tubes, one containing hydrogen, the other nitrogen. You will see at once that these two gases, when I set them glowing by the passage of an electric current, are very different in colour; we get in one an excess of red light, in the other we have a purple tinge. When nitrogen is observed by means of a spectroscope, a double line is seen very nearly coincident with the line of the nebulæ. Dr. Huggins thought that one of the constituent lines was exactly coincident with it, and because there was apparently no line whatever corresponding with the other, he thought also that the nitrogen might not be nitrogen like that with which we are familiar, but an unknown form of it. There was no doubt from the beginning that another line was a line of hydrogen, although there was some slight doubt as to whether the hydrogen in the nebulæ behaved exactly like hydrogen on the earth.

Nobody believes in the nitrogen constituent of the nebulæ now; and I presume Dr. Huggins has withdrawn in fact, if not in words, his statement concerning the coincidence, for in his address as President of the British Association, in which, as I have already stated, he withdrew his published statement as to the position of the nebulæ among the various bodies that people space, he remarks, "the progress of science has been greatly retarded by resting important conclusions upon the apparent coincidence of single lines in spectroscopes of very small resolving power," an *apologia* of which every one will see the propriety, for you will gather from Dr. Keeler's diagram that the nearest nitrogen line is three times further removed from his position of the nebula line than is the magnesium fluting. I trust I shall not be thought to be exceeding the bounds of decorous criticism when I remark that while Dr. Huggins has referred to the inaccuracy of my work in relation to this line, which is apparently indicated by Dr. Keeler's results, he has never pointed out the three times greater inaccuracy of his own.

In order to give you an idea of the relative accuracy which all these references to wave-length indicate, let us suppose that we are trying to define the position of a place in London on an E. and W. line running through Charing Cross, and then you will see exactly how matters stand. Assuming Dr. Keeler's value to be absolutely true—and I expect it is as near the truth as we are likely to get for some time—we will suppose that it represents the nebular line situated on the statue of King Charles at Charing Cross. When Mr. Huggins first measured it, he brought it to the East India Docks; his next attempt brought it to Hammersmith. Dr. Keeler's first observation brought it to Albert Gate; his next, in 1891, brought it to St. James's Palace. Subsequent work at Kensington, not yet completed, has brought it nearer still.

There is another argument in favour of the now accepted view which may be gathered from a careful examination of the forms of these different nebulæ, and by endeavouring to reason out from the form what the actual conditions at work may be. One of the most wonderful spiral nebulæ in the heavens is that in the constellation of Canes Vanatici, which has been photographed by Dr. Roberts (Fig. 7, p. 397). This is a nebula which we look down upon; we see it in plan; we are, so to speak, at the pole of the system, so that it is not foreshortened.

There is no question about the wonderful spirals being connected with the central condensation and stretching away from it, and the point which I made with regard to the one in Ursa Major is even more decided here, when I call your attention to these points of condensation right along one of the spiral branches, and when you get the possible intrusion of two spirals one on the other you see a confused mass of light. Now, if we imagine ourselves dealing there with a mass of pure gas, whether it is hydrogen or nitrogen or ammonia—that is, a combination of both—or any other, it would be extremely difficult to see why there should be any change in temperature in different parts of that mass; but the moment you assume that you are dealing with cool materials—meteoritic dust—you will see that such a picture as this is important to us, for the reason not that it shows us what is there, but because it shows us what is going on there. These bright spots do not represent the presence of matter, and the dark ones the absence of matter; but these brighter portions represent the stream lines where collision is possible—the intervals those regions where collisions are less likely, and you will see from the very configuration of this system, that if all the dust, or meteorites, or conglomerations of particles, whatever they may be, are going the same way, there will be a condition in which we shall get a minimum of collisions, and therefore a minimum of temperature.

The probability, therefore, is that we are not dealing with gas, but with masses of matter in certain regions of which, in consequence of general action, there is greater luminosity given off by the particles of which the nebulæ are composed; in

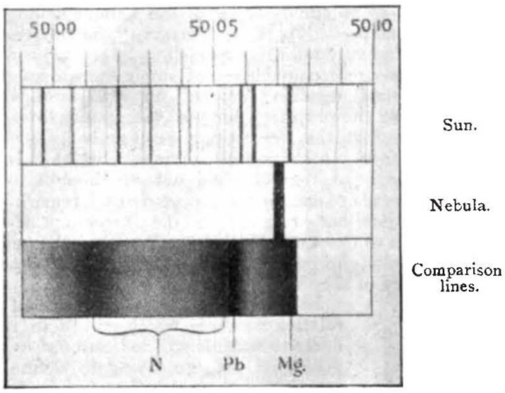

FIG. 20.—Normal position of the chief nebular line, according to Keeler.

other regions where there is less action, we have lower temperature and less light.

If, as was at first imagined, these nebulæ are gases at enormous temperatures, it would be a question of seeing them or not seeing them; there would be no special parts to be picked out at all. But, in the case of those nebulæ to which modern photographic methods have been applied, we find that

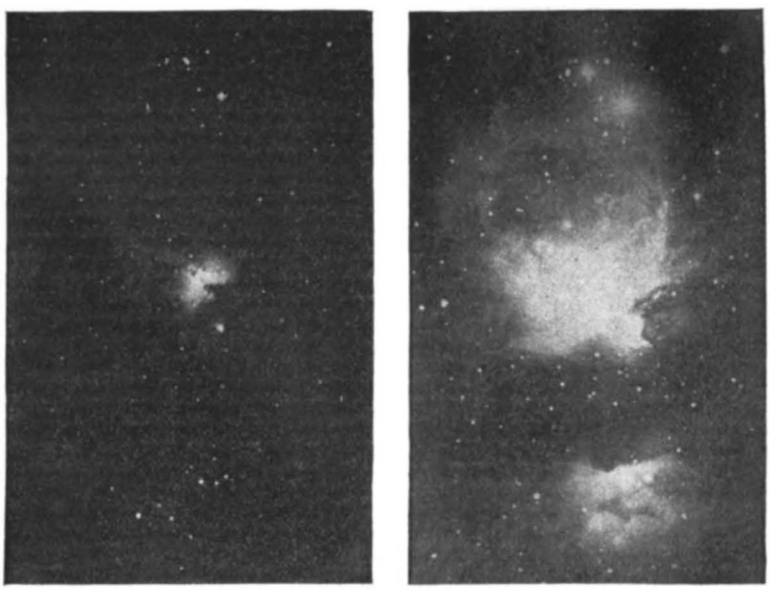

FIG. 21.—Orion nebula photographed with short and long exposures.

the nebula which we see ordinarily in our telescopes is only a very, very small fraction of the real nebula as it really exists, when we can get at it under the best possible observing conditions. Many of you, I hope, have seen the nebula of Orion in an ordinary telescope. Here it is as it has been photographed by means of a telescope powerful enough to give us the brighter portions. Here is another photograph of the nebula exactly on the same scale, in which the nebula that we usually see occupies only a very small portion; the only difference between the two photographs is that one has been exposed for a very long time to enable us to fix and to study the very dim reproduction of certain parts of it, whilst the first one was exposed only for a short time, in order that we might dwell effectively on that part only of the nebula which is generally visible to the human eye with an ordinary telescope. (Fig. 2.) If we were dealing with incandescent gas, the incandescent gas ought to leave off suddenly; but all round this nebula, where there appears to be nothing at all, the longer exposure brings before us other portions of the nebula just as rich in details, just as exquisite in their variety and tone as those ordinarily seen with the naked eye. Such a condition as that cannot be brought about by a mere homogeneous mass of gas at high temperature, but we can explain it quite easily by assuming that in such a nebula as that we are dealing with, the luminosity is brought about by disturbances, these disturbances giving rise to collisions among the particles which are apt to collide and give out luminosity. The nearer they are to the centre of gravity of the swarm, the greater will be their chance of collision, and the greater will be the luminosity of their central portion.

Still another consideration. Astronomers, since the time of Rutherfurd, who was the first to begin stellar spectroscopic work in the United States, between 1860 and 1870, have established many different classes of stars as defined by the chemical substances of which their atmospheres seem to be composed, so far as spectroscopic observations enable us to determine their composition. One group of stars is remarkable for the presence of hydrogen in enormous quantities; we assume that because the lines of hydrogen are inordinately thick. In another we get not so much hydrogen, although it is still there, but the predominant substance is iron. In other stars we get little hydrogen, if any, apparently no iron, but carbon in enormous quantities, and again there is another substance, the quantity of which varies enormously, and that is calcium. Now, if stars contain all these different substances, and if they represent epochs of evolution, they must be produced from something which actually or potentially contained these substances, so that there again you get a considerable argument in favour of the chemical complexity of the nebulæ.

Finally, we reach the second point. It is now generally conceded that the first stage in the development of cosmical bodies is not a hot gas, but a swarm of cold meteorites. From the point of view of evolution, keeping well in touch with the laws of thermodynamics, the nebulæ must begin cool if they are to develop into hot stars.

J. NORMAN LOCKYER.

(To be continued).

NEW COMPOUNDS OF PHOSPHORUS, NITROGEN, AND CHLORINE.

A SERIES of new compounds of phosphorus, nitrogen, and chlorine, and likewise a series of acids derived from them, have been discovered by Mr. H. N. Stokes, and an account of them is contributed to the *American Chemical Journal.* A familiar compound of the three elements in question, the chlorophosphuret of nitrogen, discovered by Liebig in 1832, has been the subject of frequent study, and its nature has comparatively recently been very fully demonstrated by Dr. Gladstone. It has been shown, from vapour density determinations, that this remarkably stable compound, which may be distilled in steam and boiled with acids and alkalies without appreciable change, possesses the molecular composition $P_3N_3Cl_6$. Mr. Stokes now shows that this substance is only one of a homologous series of compounds having the general formula $(PNCl_2)n$, and that these are the chlorides of a series of acids $(PNO_2H_2)n$, which he terms metaphosphimic acids. The second member of the series, $(PNCl_2)_4$, has been isolated from the product of Dr Gladstone's reaction for the preparation of $(PNCl_2)_3$, that be