THE SUN'S PLACE IN NATURE.[1]

VIII.

TWO objections, however, have been made to these hypothetical two swarms. It has been urged that the secondary swarm which we saw moving in a closed orbit round the primary one would soon spread out into a line along the orbit, so that there would always be some parts of it mixed up with the constituents of the parent swarm. That is a perfectly fair objection, supposing we are dealing with millions and billions of years, but I think that those who have made it do not know the history of astronomy. Let us take, for instance, the history of the November swarm which cuts the earth's orbit, so that in certain Novembers, generally about thirty-three years apart, we get this swarm of meteorites passing through our atmosphere, getting burnt out in that passage, and giving us one of the most magnificent sights which it is possible for mortals to see—a whole hemisphere of sky filled with shooting stars. Some of you may remember such a phenomenon as that in the year 1866, some of us are hoping to see the recurrence of it in 1899, for which we have not long to wait. But the fact that we only get this appearance every thirty-three years shows that, at all events, that swarm of meteorites to which the phenomena are due has not changed during our life-time—nay, it has not changed during the last thousand years, for man has known of that November swarm for more than a thousand years, and we have only known of the variability of Mira for 300 years; so that you see such an objection as that is entirely out of court, because it lacks the historical touch.

Another objection which has been urged is that there are certain irregularities in the light-curves of these bodies; that Mira, for instance, does not always come up to the same amount of brightness at its maximum, and perhaps, for all we know, does not always go down to the same low magnitude when it is at its lowest. That also is perfectly true, and on this account: there is no reason why we should suppose that these phenomena of the waxing and waning light of the body are produced by the movement of one body only; suppose, for instance, that there is some cosmic eye a billion miles away from our solar system, so beautifully and exquisitely wrought, so delicate in its construction, that it can see an increase in the light of the sun every time a big comet goes round it. Now we know from our own experience of comets that it would be absolutely impossible for that delicately constructed eye to see anything like a constant variability in the light of the sun under these conditions, because sometimes the brightest comets which come to us are absolutely unpredicted, they come at irregular times. It must also be pointed out in connection with this objection that there are other obvious causes for considerable variations in the light, both at the maximum and at the minimum. You remember that I showed you those beautiful spiral nebulæ of which Dr. Roberts has given us such magnificent photographs; suppose them to represent the parent swarms, and that another minor swarm tries to pass them; it is impossible to imagine that the minor swarm would exactly pass through all the intricacies of those magnificent spirals, and go and come through it precisely on the same path. It would be certain that in consequence of perturbations, the secondary swarm would sometimes go through a denser portion, at other times through a less dense portion, and then you see that would be quite sufficient to give us a considerable difference of luminosity.

I have another interesting series of diagrams, which will show you that almost any amount of variability and *irregular* variability in the light curves of these bodies may be explained on very simple grounds, supposing we acknowledge that we are dealing with the movements of more than two bodies. For instance, suppose we have one cause at work which gives us a maximum and minimum, and another cause which gives us two very much smaller maxima and minima occurring at a different period represented in Fig. 34 in the upper part of the diagram.

If we add these two together, we get the irregular light curve shown below the two simple curves in the diagram. But the amount of irregularity may possibly only reveal the amount of our ignorance, and when the time comes when we can isolate these two causes, and thus see how the addition of them should be made, we shall find that every part of this curve is really the result of a most beautiful law. I am very glad to say that quite recently Mr. Maxwell Read, of the Harvard Observatory, has put forward this very same suggestion, so that we may hope that it will soon be worked out on pretty broad lines.

But suppose for a moment that this view of two bodies is not accepted. What have we got in place of it? Well, we have to explain all the phenomena of variability by one body. That has been attempted more or less happily. Suppose, for instance, we have the case of a body waxing and waning quite regularly; you have only to say that body is like a soup-plate, and rotates on an axis, so that sometimes you see the face, sometimes only the edge. But that is not very satisfactory, because we do not know any stars which are like soup-plates. Another way is to say that the stars which are variable have great dark patches on one side of them, great bright patches on the other. Well, of course you can get a variation of light by such a scheme as that; but we have not observed that, we are simply inventing, merely suggesting ideas to nature that I fancy nature will tell us by and by are quite erroneous. For instance, I have shown you the facts with regard to β Lyræ. What is the explanation put forward for the variability of that star? Simply this, that it is a surface of revolution, the ratio

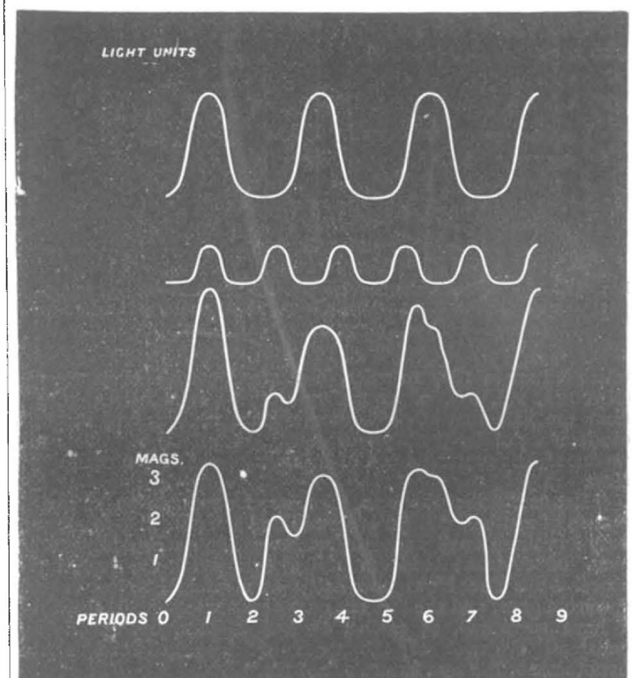

FIG. 34.—Indicating how apparently irregular light-curves may be due to the summation of two regular light variations.

of the axes being 5 to 3, *i.e.* elliptic beyond any experience of ours with regard to any other bodies; there is a dark portion at one end of the axis symmetrically situated. This thing then has to turn and twist with its axes and the black spot, and so on, and at the end of the chapter you are to have such a light curve as that of β Lyræ. That you see is blown into thin air by the spectral facts. I think you will acknowledge that these things are irrational, because they have no true basis of fact, and we must remember that in all this work we must deal strictly with the facts in accordance with the rules of philosophising; *i.e.* we must never have a complicated explanation until we are perfectly certain that a simpler explanation will not do, and the simplest explanation of all is that which occurs most frequently in the region of facts. That puts the soup-plate theory with regard to variable stars entirely out of court. Further, remember that supposing those gentlemen who still hold to the one-body theory, one star, one variability, object to the possible explanation of variability by the meteoritic hypothesis, they will find it very much more difficult to explain the departure from regularity by any geometric system, because a geometric system must certainly be

[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 207).

more rigid than any other, and therefore any irregularity under it would be almost impossible.

Closely associated with this reference to double swarms in the case of variable stars are the phenomena of so-called "new stars." Indeed, the whole conception of the meteoritic hypothesis arose from a consideration of those bodies which sometimes quite suddenly make their appearance in the heavens. We have had during the last thirty years five of these new stars, and it was during the appearance of one in the constellation Cygnus in 1876 that I was led to the views which I still hold with regard to their origin.

One of the most remarkable features of these new stars is the rapidity with which they lose their brilliancy, and it was this

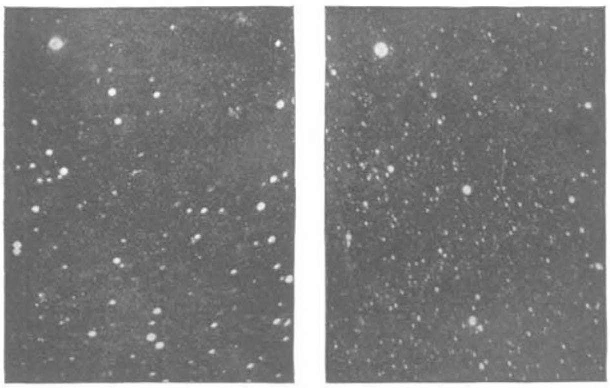

FIG. 35.—The region in the heavens where Nova Aurigæ was observed (1) after its disappearance; (2) when brightly visible (nearly in the centre).

which led me in 1877 to write, in connection with Nova Cygni (NATURE, vol. xvi. p. 413, 1877): "We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceedingly slow.

"Let us consider the case, then, on the supposition of small masses of matter. Where are we to find them? The answer is easy; in those small meteoric masses which, an ever-increasing mass of evidence tends to show, occupy all the realms of space. . . . The Nova now exists as a nebula, so far as its spectrum goes, and the fact not only goes far to support the view I have suggested, as against that of Zöllner, but it affords collateral evidence of the truth of Thomson and Tait's hypothesis of the true nature of nebulæ."

we get the greater light formed at the moment when two swarms, one revolving round the other, are nearest together.

Fortunately for science, one of these new stars appeared in 1892; it is known as Nova Aurigæ, and two photographs will give us an idea of the sort of thing which an astronomer sees in the heavens when the discovery of a new star is announced. The photographs show a portion of the constellation of Auriga, and a star which is very clearly seen in the photograph taken very soon after this star had burst upon us, is absent from one taken a few months later.

Since the spectroscope was first applied to the stars, five new stars have been observed and spectroscopically examined. One appeared in Corona Borealis in 1866, one in Cygnus in 1876, and one in Andromeda in 1885; then came the one in Auriga in 1892, to which reference has already been made, and last of all was one in the southern hemisphere, discovered in 1893. The first three of these were observed by eye only, but in the two recent ones we have the immense benefit of photographic records.

It was therefore a very interesting point when a new star came along, to see whether there was any additional light thrown by it upon the problem of two bodies; and further, upon one of the points in which, if the meteoritic hypothesis failed, it was worth absolutely nothing at all. If there was any truth in the idea of the light of these bodies being produced by the clash of meteor-swarms, when the clash was over the swarms should go back into their native obscurity, or condition of low temperature, and should, if they were seen at all, put on the spectrum of sparse swarms in other parts of the sky; that is, they should put on the spectrum of a nebula.

That, you see, was a very crucial point; it was a point which could be settled by the spectroscope, provided always we had one of these marvellous bodies at such a distance from us that we could still observe it spectroscopically, and see what the different changes really amounted to.

Already in the case of Nova Cygni, the spectrum had been observed to change from a rather complicated one of bright lines and flutings to a very simple one, similar to that of a planetary nebula. The observations did not, however, furnish any direct evidence that more than a single body was concerned in the outburst.

The appearance of Nova Aurigæ, however, furnished a splendid opportunity of testing the many theories which have been at various times advanced to account for the phenomena. This Nova was discovered at Edinburgh by Dr. Anderson, who was modest enough to announce his discovery by sending an anonymous post-card to Dr. Copeland, the Astronomer Royal for Scotland, on February 1, 1892. It was then a star of the fifth magnitude, and on confirming the true nature of the newly-discovered star by means of the spectroscope, Dr. Copeland made the news public. Information was received at most observatories on February 3, and on the same evening two photographs of the spectrum were taken at South Kensington. During the next two or three weeks the star fluctuated considerably in brightness, though being generally on the down grade; and by April 26 had fallen to the

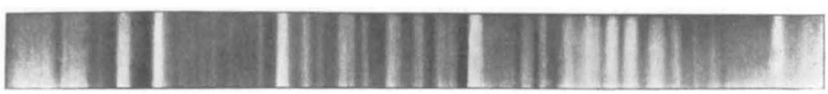

FIG. 36.—Photograph of the spectrum of Nova Aurigæ, taken at South Kensington, February 7, 1892.

Returning to the subject of new stars in 1887, in a general discussion of the meteoritic hypothesis, I saw no reason to change my views, and an inquiry into the spectroscopic phenomena led me to state that—"New stars, whether seen in connection with nebulæ or not, are produced by the clash of meteor swarms, the bright lines seen having low temperature lines of elements, the spectra of which are most brilliant at a low stage of heat."

A very detailed investigation of all the new stars which had been observed up to 1890 formed the subject of a communication to the Royal Society, and it was shown that the hypothesis would explain the fluctuations of light, the changes of colour, and the spectroscopic appearances.

To make a very long story short, it was suggested that the phenomena of new stars were produced by exactly the same cause as that which was at work in the variable stars in which

16th magnitude, so that it could only be picked up at all in the very largest telescopes. Thanks to the photographic records of the stars, it was possible to learn something of the earlier history of the new star. It had really been photographed by Prof. Pickering two months before its existence was known.

Fig. 36 shows us a photograph of the spectrum of this wonderful star itself, and it will be seen that in the case of all the chief lines we get a bright line and a dark line side by side. There are the hydrogen lines; that is, in the spectrum of that body we were dealing with the giving out of hydrogen, and the absorption of hydrogen. Now, the same set of particles cannot be producing bright and dark lines at the same time. We were then obviously dealing with two sets, and the first photograph, therefore, which was taken of the spectrum of this strange body, put beyond all question the fact that we were really dealing with two bodies, and not with one. That was very important; but you will see from the photograph, that it is very unlike the spectrum of nebulæ, so that it required a certain amount of faith when the spectrum was observed to be such as you see it here, to suppose that after a certain time, when the action which produced the greater luminosity was reduced and the light toned down, we should eventually get the spectrum of a nebula.

Well, as a matter of fact, the Nova reappeared in August 1892, and was observed to have increased in brightness from the 16th magnitude in April to about 9th magnitude. What, then, was the spectrum? It had almost completely changed; and among the first to observe the new spectrum was Prof. Campbell, of the Lick Observatory. This observer then stated that "the spectrum resembles that of the planetary nebulæ." In the following month the spectrum was also observed by Drs. Copeland and Lohse, and their observations seemed to them to "prove beyond doubt that Nova Aurigæ is now mainly shining as a luminous gas nebula." The most striking evidence on this point, however, is that afforded by the photographic investigations of Von Gothard. He not only shows us the photographic

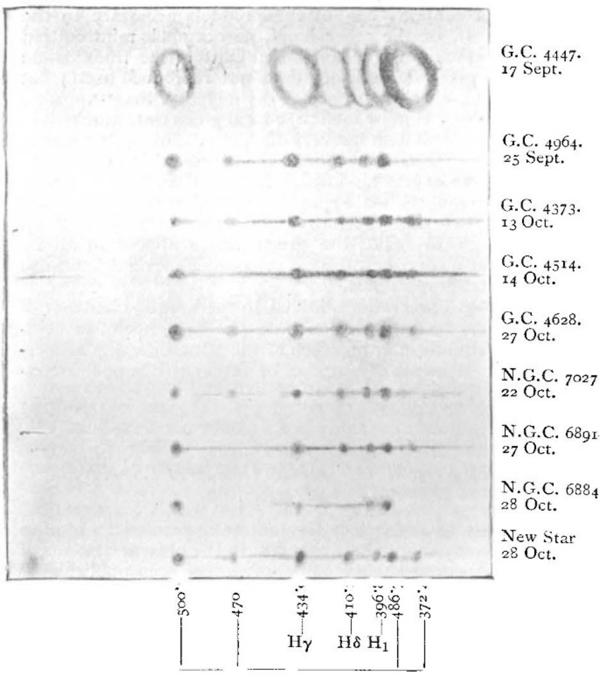

FIG. 37.—The spectrum of the new star in Auriga, as compared with the spectra of planetary nebulæ (Gothard).

spectrum of the new star at this stage of its history, but gives us also the spectra of several nebulæ to compare with it; and it is evident that we were certainly dealing, in the case of the Nova, with the same spectrum as in the nebulæ. Dr. Gothard, at least, was satisfied on this point, and stated that "the physical and chemical state of the new star resembles at present (September and October 1892) that of the planetary nebulæ."

So you see we get, first of all, the hard fact that the spectrum indicated the existence of two bodies; and then the very much harder fact for some, that, after the war was over, we got back to the condition of the nebulæ. I need not tell you that there is not universal agreement on this point, and chief among those who do not yet acknowledge it are Dr. and Mrs. Huggins. Writing of their observations of February 1893, they say: "We wish to speak at present with great reserve, as our knowledge of the Nova is very incomplete; but we do not regard the circumstance that the two groups of lines above described fall near the positions of the two principal nebular lines as sufficient to show any connection between the present physical state of the Nova and that of a nebula of the class which gives these lines."

But I may say, at all events, that I have the great authority of the names of Campbell, Copeland, and Gothard, who state that they have certainly observed the spectrum to be that of the nebulæ, and in reply to Dr. Huggins, Prof. Campbell says: "If the spectrum is not conceded to be nebular, I must ask what else we should expect to find in that spectrum, if it were nebular?" The answer to that is, that you would not expect to find anything else because it is all there already. In fact, out of nineteen lines observed or photographed by Prof. Campbell in the spectrum of the Nova, eighteen correspond perfectly with nebular lines. "Therefore the spectrum is nebular, and the fact that the lines have remained broad, or may have remained multiple, does not militate against the theory."

Further, there is even telescopic and photographic evidence of the fact that Nova Aurigæ became a nebula. Dr. Max Wolf's photographs of the Nova and its surroundings in 1893, resulted in the discovery of a number of new diffuse nebulæ in its vicinity, "and there even appeared to be traces of nebulous appendages proceeding from the star itself."

Another new star appeared in the southern constellation Norma in 1893. This was discovered on October 26, on a photograph taken at Arequipa, Peru, on July 10, 1893. Fortunately the photograph was one showing the spectra of stars instead of the simple images of the stars themselves, and the spectrum was seen to be identical with that of Nova Aurigæ. Even more important were the observations of Campbell in February and March 1894, when the star was about 10th magnitude. As the result of his work, he stated that "there can be no doubt that the spectrum of Nova Normæ is nebular."

J. NORMAN LOCKYER.

(To be continued.)

THE FLUORESCENCE OF ARGON, AND ITS COMBINATION WITH THE ELEMENTS OF BENZENE.[1]

M. BERTHELOT read the following paper, containing observations by M. Deslandres and himself, before a recent meeting of the Paris Academy of Sciences:—

I have thought it useful to study more closely the conditions of the combination with benzene under the influence of the silent discharge and those of the special fluorescence which accompanies it.

M. Deslandres, whose great competence in spectroscopic questions the Academy is well aware of, has been kind enough to help me in these new determinations, made with higher dispersion, and rigorously determined by photography. It is my duty to thank him here for this long and laborious work.

We must remember that the combination of argon with the elements of benzene, under the influence of the silent discharge, is a slow process; according to the present research, it is accomplished with the help of mercury, which intervenes under the form of a volatile compound. The use of very frequent discharges appears not to modify the general characters of the reaction.

At the beginning, nothing is seen in daylight, and it is only in a dark room that one perceives a feeble violet glow, similar, in its intensity, to that which the discharge develops generally in gaseous systems. At the end of an hour, when in a dark room, a green glimmer is seen, which occupies the middle of the interval between the spirals of the platinum band wound round the discharge tube, the luminous spectrum gives two yellow lines at λ 579 and 577, a green line at λ 546, and a green band at λ 516·5. These different lines will be defined by-and-by.

The photographic spectrum, taken during this time, with an hour's exposure, shows the principal bands of nitrogen, as well as a blue line λ 436, a violet line λ 405, and an ultra-violet line λ 354; these latter being more feeble than the bands of nitrogen.

During the following hours, the green glow constantly increases, the yellow lines and line λ 546 increase, and the band λ 516·5 diminishes. At the end of eight hours, the bands of nitrogen have almost entirely disappeared in the photograph; without doubt it is because the corresponding nitrogen has been absorbed by the benzene.

Seven additional hours of sparking bring the fluorescence to a brilliant emerald colour, visible in broad daylight; the intensity of this phenomenon, as I have already had occasion to say,

[1] Translated from *Comptes rendus*, June 24, pp. 1386–1390.