water. Then, Mr. Reade supposes all the organisms in the bulk of water taken to die and fall to the bottom each day. Mr. Murray, in his calculations, supposes only one-sixteenth part to die each day. From the same data the former makes out a rate of accumulation of deposit of 1 inch in 29 years, the latter a rate of 1 inch in 470 years. Dana estimates the growth of a reef at not greater than one-sixteenth of an inch in one year, *i.e.* 1 inch in 16 years. Yet it will be admitted that a reef must grow much more rapidly than a deep-sea deposit. What then would justify us in accepting these figures as in any way representing what is now taking place in Nature? The fact is we much want definite information on the rate of growth of these calcareous deposits, and if Mr. Reade has the information his language would warrant, he should make it known for the benefit of science.

We know that these deposits do accumulate to hundreds of feet in thickness in some places, notwithstanding solution; and it seems to me that, as we can imitate in the laboratory the conditions of solution while we cannot those of secretion by organisms, then by experiments in this direction we may at least arrive at a knowledge of the minimum rate of accumulation of oceanic calcareous deposits. JAMES G. ROSS.

14 Argyll Place, Edinburgh, April 14.

Bernicle Geese on Coniston Lake.

THIS afternoon while walking by this lake I saw four large birds flying overhead. These birds, after making several circuits in the air, pitched on the lake. I had with me an excellent pair of field-glasses, and as I succeeded in approaching within 20 yards of them, I was enabled to examine them with sufficient accuracy to convince me that they were Bernicle geese (*Anser leucopsis*, Yarrell). What struck me as most worthy of remark was their extreme tameness, as they allowed me, first on land, and then in a boat, to approach within 20 yards of them. They were in excellent plumage, and seemed in good condition. After remaining about three hours swimming about on the lake, they rose, and after circling round once or twice, flew off in a northerly direction.

May I ask if this is a rare bird to see in the Lake District at this time of year? I have inquired in the neighbourhood, and do not think they could have come from any private water. Several people who have been here for many years assure me they have never seen this bird on the lake before, and this has certainly been my own experience. Is it possible their extreme tameness was due to fatigue? WILLIAM R. MELLY.

Tent Lodge, Coniston Lake, Lancashire, April 8.

The Muzzling of Oysters.

THIS practice, described in the current number of NATURE (p. 572) as owing "its existence to a careful study of the habits of the bivalve," is by no means new, though probably original on the part of the American naturalists. Our London fishmongers have muzzled oysters on a large scale from a time that is immemorial among them. Barrelled oysters are all very carefully muzzled, but without wires, as anybody may learn by watching an expert in the process of barrelling. It will be seen that he lays the oysters one by one carefully in tiers up to the top of the barrel, and then lays another tier rising *above* the level of the top. Having done this, he places the lid of the barrel on this exuberant tier, and thumps and rattles the barrel on a stone pavement or other solid ground until, by close packing of the whole, it descends to the level of the barrel top. The mass of oysters being thus compressed so as to render the slightest gaping of any one quite impossible, he firmly nails down the head of the barrel.

Experience has proved that oysters thus effectively muzzled may take long slow journeys (as they did in the old coaching days) and be kept fresh and without loss of flavour for two or three weeks, provided the barrels are unopened. If, however, they are loosely barrelled, a few days are too many. In some old country houses the barrels, unopened, were placed in salt water, and thus kept until required, but whether this was advantageous I cannot say.

W. MATTIEU WILLIAMS.

The Grange, Neasden, April 13.

SUGGESTIONS ON THE CLASSIFICATION OF THE VARIOUS SPECIES OF HEAVENLY BODIES.[1]

I.

I.—PROBABLE ORIGIN OF SOME OF THE GROUPS.

I. NEBULÆ.

IN a paper communicated to the Royal Society on November 15, 1887, I showed that the nebulæ are composed of sparse meteorites, the collisions of which bring about a rise of temperature sufficient to render luminous one of their chief constituents—magnesium. This conclusion was arrived at from the facts that the chief nebula lines are coincident in position with the fluting and lines visible in the bunsen burner when magnesium is introduced, and that the fluting is far brighter at that temperature than almost any other spectral line or fluting of any element whatever.

I suggested that the association or non-association of hydrogen lines with the lines due to the olivine constituents of the meteorites might be an indication of the greater or less sparseness of the swarm, the greatest sparseness being the condition defining fewest collisions, and therefore one least likely to show hydrogen. This suggestion was made because observations of comets and laboratory work have abundantly shown that great liability to collision in the one case, and increase of temperature in the other, are accompanied by the appearance of the carbon spectrum instead of the hydrogen spectrum.

The now demonstrated meteoric origin of these celestial bodies renders it needful to discuss the question in somewhat greater detail, with a view to classification; and to do this thoroughly it is requisite that we should study the rich store of facts which chiefly Sir William Herschel's labours have placed before us regarding the various forms of nebulæ, with the view of ascertaining what light, if any, the new view throws on their development.

To do this the treatment must be vastly different from that—the only one we can pursue—utilized in the case of the stars, the images of all, or nearly all, of which appear to us as points of light more or less minute, while, in the case of the nebulæ, forms of the most definite and, in many cases, of the most fantastic kind, have been long recognized as among their chief characteristics.

It will at once be evident that since the luminosity of the meteorites depends upon collisions, the light from them, and from the glow of the gases produced from them, can only come from those parts of a meteor-swarm in which collisions are going on. Visibility is not the only criterion of the existence of matter in space; dark bodies may exist in all parts of space, but visibility in any part of the heavens means, not only matter, but collisions, or the radiation of a mass of vapour produced at some time or other by collisions. The appearances which these bodies present to us may bear little relation to their actual form, but may represent merely surfaces, or loci of disturbances.

It seemed proper, then, that I should seek to determine whether the view I have put forward explains the phenomena as satisfactorily as they have been explained on the old ones, and whether, indeed, it can go further and make some points clear which before were dark.

To do this it is not necessary in the present paper to dwell at any great length either on those appearances which were termed *nebulosities* by Sir William Herschel or on irregular nebulæ generally; but it must be remarked that the very great extension of the former—which there is little reason to doubt will be vastly increased by increase of optical power and improvement in observing conditions and stations—may be held to strengthen the view that space is really a meteoritic plenum, while the forms indicate motions and crossings and interpenetra-

[1] The Bakerian Lecture, delivered at the Royal Society on April 12, by J. Norman Lockyer, F.R.S.

tions of streams or sheets, the brighter portions being due to a greater number of collisions per unit volume.

When we come to the more regular forms we find that they may be generalized into three groups, according as the formative action seems working towards a centre, round a centre in a plane, or nearly so, or in one direction only; as a result we have globular, spheroidal, and cometic nebulæ. I propose to deal with each in turn.

Globular Nebulæ.

The remarkable appearance presented by the so-called planetary nebulæ requires that I should refer to them in some detail. Sir William Herschel does not describe them at any great length, but in his paper on "Nebulous Stars" he alludes to the planetary nebulosity which in many cases is accompanied by a star in the centre, and finally comes to the conclusion that "the nebulosity about the star is not of a starry nature" (Phil. Trans., vol. lxxxi. p. 73, 1791).

Sir John Herschel, in his valuable memoir published in Phil. Trans., 1833, describes them as "hollow shells" (p. 500). It was so difficult to explain anything like their appearance by ordinary ideas of stellar condensation that Arago, as quoted by Nichol ("Architecture of the Heavens," p. 86), abandoning altogether the idea that they represented clusters of stars or partook in any wise of a stellar constitution, imagined them as hollow spherical envelopes, in substance cloudy and opaque, or rather semi-transparent; a brilliant body invisible in the centre illuminating this spherical film, so that it was made visible by virtue of light coming through it and scattered by reflection from its atoms or molecules. The mystery was explained to a certain extent by Lord Rosse, who (Phil. Trans., 1850, vol. cxl. p. 507) states that nearly all the planetary nebulæ which he had observed with his colossal instruments up to that time had been found to be perforated. In only one case was a perforation not detected, but in this ansæ were observed, introducing into the subject for the first time the idea of nebulous bodies resembling to a certain extent the planet Saturn. But Lord Rosse, although he thus disposed of the idea of Arago, still considered that the annular nebulæ were really hollow shells, the perforation indicating an apparently transparent centre.

Huggins and Miller subsequently suggested that the phenomena represented by the planetary nebulæ might be explained without reference to the supposition of a shell (or of a flat disk) if we consider them to be masses of glowing gas, the whole mass of the gas being incandescent, so that only a luminous surface would be visible (Phil. Trans., vol. cliv. p. 442, 1864).

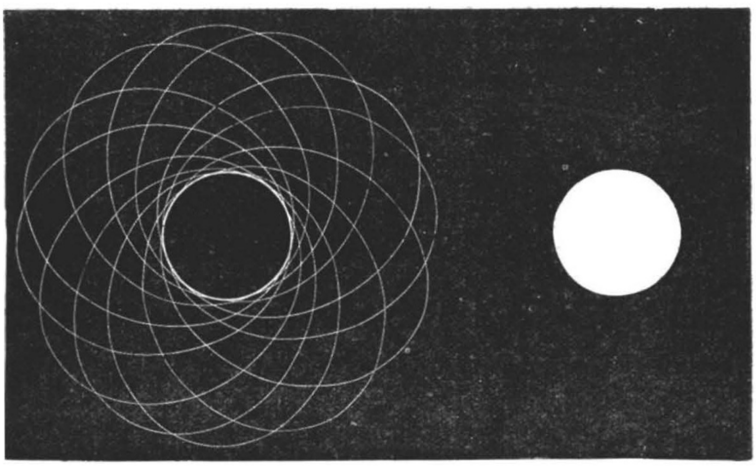

FIG. 1.—Suggested origin of the appearance presented by a planetary nebula. The luminosity is due to the collisions occurring along the sphere of intersection of the elliptic orbits of the meteorites. The left-hand diagram is a cross-section of the meteoric system, and the right-hand one shews the appearance of the collision shell as seen from a point outside.

It will be seen that all these hypotheses are mutually destructive; but it is right that I should state, in referring to the last one, that the demonstration that these bodies are not masses of glowing gas merely has been rendered possible by observations of spectra which were not available to Messrs. Huggins and Miller when their important discovery of the bright-line spectrum of nebulæ was given to the world.

It remains, then, to see whether the meteoritic hypothesis can explain these appearances when it is acknowledged that all the prior ones have broken down. Let us for the sake of the greatest simplicity consider a swarm of meteorites at rest, and then assume that others from without approach it from all directions, their previous paths being deflected. There will be at some distance from the centre of the swarm a region in which collisions will be most valid. Meteorites arrested here will begin to move in almost circular orbits round the common centre of gravity.

The major axes of these orbits may be assumed to be not very diverse, and we may further assume that, to begin with, one set will preponderate over the rest. Their elliptic paths may throw the periastron passage to a considerable distance from the common centre of gravity; and if we assume that the meteorites with this common mean distance are moving in all planes, and that some are direct and some retrograde, there will be a shell in which more collisions will take place than elsewhere. *Now, this collision surface will be practically the only thing visible, and will present to us the exact and hitherto unexplained appearance of a planetary nebula—a body of the same intensity of luminosity at its edge and centre—thus putting on an almost phosphorescent appearance.*

Such a collision surface, as I use the term, is presented to us during a meteoric display by the upper part of our atmosphere.

I append a diagram, Fig. 1, which shows how, if we thus assume movement round a common centre of gravity in a mass of meteorites, one of the conditions of movement being that the periastron distance shall be somewhat considerable, the mechanism which produces the appearance of a planetary nebula is at once made apparent. The diagram shows the appearance on the supposition that the conditions of all the orbits with reference to the major axis shall be nearly identical, but the appearances would not be very greatly altered if we take the more probable case in which there will be plus and minus values.

Globular Nebulæ showing Condensations until finally a Nebulous Star is reached.

If we grant the initial condition of the formation of a collision-shell, we can not only explain the appearances put on by planetary nebulæ, but a continuation of the same line of thought readily explains those various other classes to which Herschel has referred, in which condensations are brought about, either by a gradual condensation towards the centre, or by what may be termed successive jumps, showing that they are among the earliest stages of nebular development.

To explain these forms we have only to consider what will happen to the meteorites which undergo collision in the first shell. They will necessarily start in new orbits,

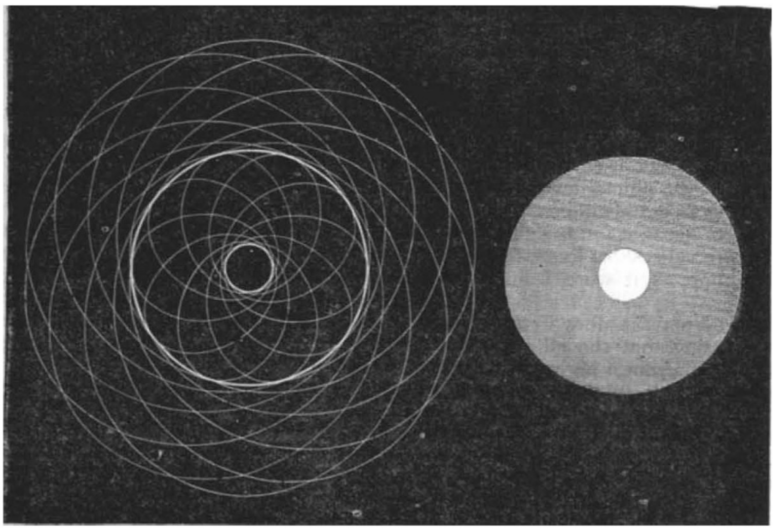

FIG. 2.—Suggestion as to the origin of a globular nebula with a brighter central portion. As in the former case, the luminosity of the fainter portion is due to the collisions which occur along the sphere of intersection represented by the larger circle. After collision the meteorites will travel in new orbits, and there will be an additional sphere of intersection, represented by the smaller circle. The left-hand diagram is a cross-section, and the right-hand one represents the appearance of the two collision shells as seen from a point outside.

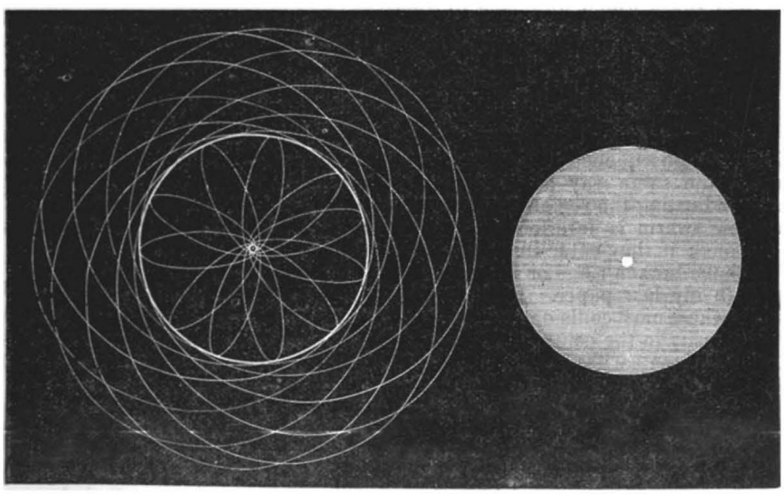

FIG. 3.—Suggestion as to the origin of a nebulous star. The orbits of the inner set of meteorites are very elliptic, so that the shell of intersection appears almost as a point. As in the previous cases, the left-hand diagram represents the meteoric systems in section, and the right-hand one the appearance from a point outside.

and it is suggested that an interior collision-shell will in this way be formed.

In consequence of the collisions the orbits will have a tendency to get more and more elliptic, while the pericentric distance will at the same time be reduced; the swarm will, in consequence of this action, gradually brighten towards the centre through collisions being possible nearer the centre, and ultimately we shall have nebulæ with a distinct nucleus, the nucleus then representing the *locus* of most collisions. This brightness may be sudden in places, or quite gradual, according to the collision conditions in each swarm. The final stage will be a nebulous star.

Effects of Subsequent Rotation.—Spheroidal Nebulæ.

In such meteor-swarms as those we have considered, it must be that rotation is sooner or later set up. Otherwise it would be impossible to account for the spheroidal nebulæ at all. I am aware that in Newton's opinion the cause of this rotation was not mechanical, but the moment we assume a meteoric origin of these globular clusters it is straining the facts to assume that the intake will be exactly the same at all points, and the moment the bombardment is more or less localized, rotation must follow sooner or later. Sir William Herschel, in his paper of 1811 (p. 319), says: "If we consider this matter in a general light, it appears that every figure which is not already globular must have eccentric nebulous matter, which, in its endeavour to come to the centre, will either dislodge some nebulosity which is already deposited, or slide upon it sideways, and in both cases produce a circular motion; so that, in fact, we can hardly suppose a possible production of a globular form without a subsequent revolution of nebulous matter, which in the end may settle in a regular rotation about some fixed axis."

Given, then, a globular swarm with a rotation around an axis, we have to discuss the phenomena produced by collisions under a new set of circumstances.

Here at once we have to account for the fact that the nearly spherical forms are very short-lived, for they are very rare; we seem to jump, as it were, from globes to very extended spheroids.

If it be conceded that from the above considerations we are justified in supposing that the elliptic and other spheroidal nebulæ really represent a higher stage of evolution than those presented to us in the globular form, it is clear that on the meteoritic hypothesis the greater part of the phenomena will represent to us what happens to such a system under the condition of a continuous bombardment of meteorites from without.

So soon as we have a minor axis, there will at first be more collisions parallel to it; the result of this will be that the equatorial plane will be intensified, and then, later on, if we conceive the system as a very extended spheroid, it is obvious that meteorites approaching it in directions parallel to its minor axis will now have fewer chances of collisions than those which approach it, from whatever azimuth, in what we may term the equatorial plane. These evidently, at all events if they enter the system in any quantity, will do for the equatorial plane exactly what their fellows were supposed to do for the section in Fig. 1, and we shall have on the general background of the symmetrically rotating nebula, which may almost be invisible in consequence of its constituent meteorites all travelling the same way and with nearly equal velocities, curves indicating the regions along which the entrance of the new swarm is interfering with the movements of the old one; if they enter in excess from any direction, we shall have broken rings or spirals.

This was suggested in my last paper. Various rings will indicate the regions where most collisions are possible, and the absence of luminosity in the centre by no means demonstrates the absence of meteorites there.

Researches by Lord Rosse and others have given us forms of nebulæ which may be termed sigmoid and Saturnine, and these suggest that they and the elliptical nebulæ themselves are really produced by the rotation of what was at first a globular rotating swarm of meteorites, and that in these later revelations we pick up those forms which are produced by the continued flattening of the sphere into a spheroid under the meteoric conditions stated. It is worthy of remark that all the forms taken on by the so-called elliptic nebulæ described by the two Herschels, and by the spiral, sigmoid, and Saturnine forms which have been added to them by the labours of Lord Rosse and others, are recalled in the most striking manner by the ball of oil in Plateau's experiment, when rotations of different velocities are imparted to it. It is my intention to repeat Plateau's experiments, and to take instantaneous photographs of the various phenomena presented, and to place them side by side with the drawings of nebulæ, of which they are almost the exact counterparts.

The Saturnine form may, indeed, in some cases represent either the first or last stages in this period of the evolutionary process. I say *may* represent, in consequence of the extreme difficulty in making the observations, so that in the early stages a spherical nebula, beginning to change into a spheroid, may have its real spheroidal figure cloaked by various conditions of illumination.

The true Saturnine form must, as in the case of Saturn itself, represent one of the latest forms in the meteor-swarm, because, if it be not continually fed from without, collisions must sooner or later bring all the members of the swarm to the centre of figure.

Cometic Nebulæ.

I do not know that any explanation has, so far, been suggested as to the origin of these curious forms, which were first figured by Sir William Herschel, and of which a number have recently been observed in the southern hemisphere ("Melbourne Observations"). It is clear that in them the conditions are widely different from those hitherto considered in this paper. I think that the meteoritic hypothesis satisfactorily explains them, on the supposition that we have either a very condensed swarm moving at a very high velocity through a sheet of meteorites at rest, or the swarm at rest surrounded by a sheet all moving in the same direction. It is a question of relative velocity.

If we consider the former case, it is clear that the collision region will be in the rear of the swarm, that the collision will be due to the convergence of the members of the sheet due to the gravity of the swarm, and that the collision region will spread out like a fan behind the swarm.

The angle of the fan, and the distance to which the collisions are valid, will depend upon the velocity of the condensed swarm.

Nebulous Origin of some Bodies which appear as Stars.

From this point of view it is also possible that many stars, instead of being true condensed swarms due to the nebulous development to which we have referred, are simply appearances produced by the intersection of streams of meteorites. They are, then, simply produced by an intensification of the conditions which gave rise to the brighter appearances recorded by Herschel here and there in his diffused nebulosities. The nebulous appendages sometimes seen in connection with stars strengthen this view.

II. Stars with Bright Lines or Flutings.

I pointed out in my last paper that those stars in the spectra of which bright lines had been observed were in all probability the first result of nebulous condensation, both their continuous spectrum and that of the surrounding vapour being produced by a slightly higher temperature than that observed in nebulæ in which similar though not identical phenomena are observed.

I have recently continued my inquiries on this point: and I may say that all I have recently learned has confirmed the conclusions I drew in my last paper, while many of the difficulties have disappeared. Before I refer to these inquiries, however, it is necessary to clear the ground by referring to the old view regarding the origin of bright lines in stellar spectra, and to the question of hydrogen.

Reference to the Old View by which it was supposed some of the Bright-line Phenomena might be accounted for.

In the views which, some years ago, were advanced by myself and others, to account for the bright lines seen in some of the "stars" to which reference has been made, the analogy on which they were based was founded on solar phenomena; the "stars" in question being supposed to be represented in structure by our central luminary. The main constituent of the solar atmosphere outside the photosphere is hydrogen, and it was precisely this substance which was chiefly revealed by these stellar observations and in the Novas, in which cases it was sometimes predominant. A tremendous development of an atmosphere like that of the sun seemed to supply the explanation of the phenomena.

Acting on this view in 1878,[1] I attempted to catch these chromospheric lines in α Lyræ, abandoning the use of a cylindrical lens in front of the slit with this object in view.

Further, it was quite clear that if such gigantic supra-photospheric atmospheres existed, their bright lines might much modify their real absorption-spectra; even "worlds without hydrogen" might be thus explained without supposing a *lusus naturæ*, and so I explained them.

That this view is untenable, as I now believe, and that it is unnecessary, will, I think, be seen from what follows. A long series of newly described phenomena, which are absolutely incomprehensible while it is applied to them, find, I think, a simple and sufficient explanation. I must hold that the view is untenable, because how a body constituted in any way like the sun could change its magnitude from the thirteenth to the sixth every year or so, or change its hydrogen lines from bright to dark once a week, passes comprehension; and the more closely a "star" resembles the sun the less likely are such changes to happen. Even the minor evolutionary changes are inexplicable on this hypothesis, chiefly because in a completely condensed mass the temperature must be very high and constant, while I have shown that the spectrocopic phenomena are those of a specially low temperature; and I may now add that many of the objects are extremely variable in the quantity and quality of the light they emit.

Another cause of the appearance of the hydrogen lines has been suggested by Mr. Johnstone Stoney (Proc. Roy. Soc., vol. xvii. p. 54). He considers it due to the clashing together of the atmospheres of two stars, the outer constituent of the atmosphere—hydrogen—alone being raised by the friction to brilliant incandescence.

Another objection we can urge against the old view is that all bodies in the universe cannot be finished suns in the ordinary sense, and that it leaves out of account all possible processes of manufacture, not only of single stars, but of double and multiple systems, at all stages between nebula and sun; while the new one, by simply changing the unit from the star to each individual constituent, it is hardly too much to say, explains everything, though it is perfectly true that in some of the steps a considerable acquaintance with spectroscopic phenomena is necessary to realize the beauty and the stringency of the solutions.

[1] ". . . The sun which we see, the sun which sends us the majority of the light we receive, is but a small kernel in a gigantic nut, so that the diameter of the real sun may be, say, two million miles. Suppose then that some stars have very large coronal atmospheres; if the area of the coronal atmosphere is small compared with the area of the section of the true disk of the sun, of course we shall get an ordinary spectrum of the star; that is to say, we shall get the indications of absorption which make us class the stars apart; we shall get a continuous spectrum barred by dark lines. But suppose that the area of the coronal atmosphere is something very considerable indeed, let us assume that it has an area, say fifty times greater than the section of the kernel of the star itself; now, although each unit of surface of that coronal atmosphere may be much less luminous than an equal unit of surface of the true star at the centre, yet, if the area be very large, the spectroscopic writing of that large area will become visible side by side with the dark lines due to the brilliant region in the centre where we can study absorption; other lines (bright ones) proceeding from the exterior portion of that star will be visible in the spectrum of the apparent *point* we call a star. Now it is difficult to say whether such a body as that is a star or a nebula. We may look upon it as a nebula in a certain stage of condensation; we may look upon it as a star at a certain stage of growth."—Proc. R.S. 1878, No. 185, p. 49.

The Question of Hydrogen in the Case of Bright-Line Stars.

It may be convenient also that I should summarize the various conditions under which the lines of hydrogen are observed in the meteorite swarms we are now considering.

In the "nebulæ" we begin with the widest interspaces. Future investigation may show that, as I have suggested, those in which the hydrogen lines are absent are the most widely spaced of all. Be this as it may, it is a matter of common knowledge that with the brighter nebulæ, such as that of Orion, to take an instance, we have hydrogen associated with the low-temperature radiation of olivine. That the hydrogen is electrically excited to produce this glow is proved by the fact that the temperature of the meteorites themselves must be very low; otherwise the magnesium would not show itself without the manganese and iron constituents, and the continuous spectrum would be much brighter and longer than it is.

In the former paper I showed that in my laboratory experiments, when the pressure was slightly increased in a tube containing gases obtained from meteorites, the carbon bands began to be visible. We should expect this to happen therefore in a meteor swarm at some point at which the mean interstitial space was smaller than that accompanied by the appearance of the hydrogen lines; and it would be natural that both should be seen together at an early stage and both feeble, by which I mean not strongly developed, as hydrogen is not strongly developed even in the nebula of Orion, none of the ultra-violet lines being visible in a photograph, while the magnesium line is.

The association of the low-temperature lines of hydrogen with the flutings of carbon is therefore to be expected, and I shall subsequently show that we have such an association in the so-called bright-line stars; and even at a further stage of development, in stars like α Orionis, the hydrogen is still associated with the carbon.

The Cometic Nature of Stars with Bright Lines in their Spectra.

Seeing that the hypothesis I am working on demands that the luminosity in stars and the bright lines in their spectra are produced by the collisions of meteorites, the spectra of those bodies must in part resemble those of comets, in which bodies by common consent the luminosity is now acknowledged to be produced by collisions of meteorites.

We must, however, first consider the vast difference in the way in which the phenomena of distant and near meteoric groups are necessarily presented to us; and, further, we must bear in mind that in the case of comets, however it may arise, there is an action which drives the vapours produced by impacts outward from the swarm in a direction opposite to that of the sun.

It must be a very small comet which, when examined spectroscopically in the usual manner, does not in consequence of the size of the image on the slit enable us to differentiate between the spectra of the nucleus and envelopes. The spectrum of the latter is usually so obvious, and the importance of observing it so great, that the details of the continuous spectrum of the nucleus, however bright it may be, are almost overlooked.

A moment's consideration, however, will show that if the same comet were so far away that its whole image would be reduced to a point on the slit-plate of the instrument, the differentiation of the spectra would be lost; we should have an integrated spectrum in which the brightest edges of the carbon bands, or some of them, would or would not be seen superposed on a continuous spectrum.

The conditions of observations of comets and stars being so different, any comparison is really very difficult; but the best way of proceeding is to begin with the spectrum of comets in which, in most cases, for the reason given, the phenomena are much more easily and accurately recorded.

But even in the nucleus of a comet as in a star it is much more easy to be certain of the existence of bright lines than to record their exact positions,[1] and as a matter of fact bright lines have been recorded, notably in Comet Wells and in the great comet of 1882.

The main conclusion to which my researches have led me is that the stars now under consideration are almost identical in constitution with comets between that condition in which, as in those of 1866 and 1867, they give us the absolute spectrum of a nebula and that put on by the great comet of 1882.

I am aware that this conclusion is a startling one, but a little consideration will show its high probability, and a summary of all the facts proves it, I think, beyond all question.

While we have bright lines in comets, it can be shown that some of them are the remnants of flutings. Thus in Comet III. of 1881, as the carbon lines died away the chief manganese fluting at 558 became conspicuously visible; it had really been recorded before then. The individual observations are being mapped in order that the exact facts may be shown. It may probably be asked how it happened that the fluting of magnesium at 500 was not also visible. Its absence, however, can be accounted for: it was *masked* by the brightest carbon fluting at 517, whereas the carbon fluting which under other circumstances might mask the manganese fluting at 558 is always among the last to appear very bright and the first to disappear.

In the great comet of 1882, which was most carefully mapped by Copeland, very many lines were seen, and indeed many were recorded, and it looks as if a complete study of this map will put us in possession of many of the lines recorded by Sherman in the spectrum of γ Cassiopeiæ. We have then three marked species of non-revolving swarms going on all fours with three marked species of revolving ones, and in this we have an additional argument for the fact that the absence in the former of certain flutings which we should expect to find may have their absence attributed to masking by the carbon flutings.

We have next, then, to show that there are carbon bands in the bright-line stars.

There is evidence of this. Among the bright lines recorded is the brightest carbon fluting at 517. This is associated with those lines of magnesium and manganese and iron visible at a low temperature which have been seen in comets.

But we have still more evidence of the existence of carbon. In a whole group of bright-line stars there is a bright band recorded at about 470, while, less refrangible than it, there appears a broad absorption band. I regard it as extremely probable that we have here the bright carbon band 467–474, and that the appearance of an absorption band is due to the fact that the continuous spectrum of the meteorites extends only a short distance into the blue.

If we consider such a body as Wells's comet, or the great comet of 1882, as so great a distance from us that only an integrated spectrum would reach us, in these cases the spectrum would appear to extend very far, and more or less continuously, into the blue; but this appearance would be brought about, not by the continuous spectra of the meteorites themselves, but by the addition of the hydrocarbon fluting at 431 to the other hot and cold carbon bands in that part of the spectrum.

There are other grounds which may be brought forward to demonstrate that the difference between comets and the stars now under discussion is more instrumental than physical.

Supposing that the cometic nature of these bodies be conceded, the laboratory work will show us which flutings and lines will be added to the nebula spectrum upon each rise of temperature; and the discussion, so far as it has gone, seems to show that such lines and flutings have actually been observed.

The difficulties of the stellar observations must always be borne in mind. It will also be abundantly clear that a bright fluting added to a continuous spectrum may produce the idea of a bright line at the sharpest edge to one observer, while to another the same edge will appear to be preceded by an absorption band.

III. Stars with Bright Flutings accompanied by Dark Flutings.

I also showed in the paper to which reference has been made that the so-called "stars" of Class III.*a* of Vogel's classification are not masses of vapour like our sun, but really swarms of meteorites; the spectrum being a compound one, due to the radiation of vapour in the interspaces and the absorption of the light of the red- or white-hot meteorites by vapours volatilized out of them by the heat produced by collisions. The radiation is that of carbon vapour, and some of the absorption, I stated, was produced by the chief flutings of manganese.

These conclusions were arrived at by comparing the wave-lengths of the details of spectra recorded in my former paper with those of the bands given by Dunér in his admirable observations on these bodies.[1]

The discovery of the cometic nature of the bright-line stars greatly strengthens the view I then put forward, not only with regard to the presence of the bright flutings of carbon, but with regard to the actual chemical substances driven into vapour. From the planetary nebulæ there is an undoubted orderly sequence of phenomena through the bright-line stars to those now under consideration, if successive stages of condensation are conceded.

I shall return to these bodies at a later part of this memoir.

IV. Stars in which Absorption Phenomena predominate.

I do not suppose that there will be any difficulty in recognizing, that if the nebulæ, stars with bright lines, and stars of the present Class III.*a* are constituted as I state them, all the bodies more closely resembling the sun in structure, as well as those more cooled down, must find places on a temperature curve pretty much as I have placed them; the origin of these groups being, first still further condensation, then the condition of maximum temperature, and then the formation of a photosphere and crust.

We shall be in a better position to discuss these later stages when the classifications hitherto adopted have been considered.

(To be continued.)

THE HITTITES, WITH SPECIAL REFERENCE TO VERY RECENT DISCOVERIES.[2]

IV.

THOSE who have attempted to decipher the Hittite inscriptions have not always regarded a fact which may be discerned with tolerable facility. The inscriptions from Hamath, and those from Jerablûs or Carchemish, though no doubt deriving their origin from a common source, yet present, as we know them, two distinct types. Symbols usual and frequently repeated on the Jerablûs monuments are wholly absent from those of

[1] "*Observations of Comet III.*, 1881, *June* 25.—The spectrum of the nucleus is continuous; that of the coma shows the usual bands. With a narrow slit there are indications of many lines just beyond the verge of distinct visibility."—Copeland, *Copernicus*, vol. ii. p. 226.

[1] "Les Étoiles à spectres de la troisième classe," *Kongl. Svenska Vitenskaps-Akademiens Handlingar*, Bandet 21, No. 2, 1885.

[2] Based on Lectures delivered by Mr. Thomas Tyler at the British Museum in January 1888. Continued from p. 562.