THURSDAY, SEPTEMBER 13, 1877

STAR OR NEBULA?

FOLLOWING close upon the publication of Dr. Vogel's paper on the new star in Cygnus, Lord Lindsay has communicated an interesting letter to the *Times* announcing the fact that the new star has now put on the appearance presented ordinarily by the so-called planetary nebulæ.

Of all the lines chronicled by Cornu and Vogel only one remains, that namely which the latter observer showed to be constantly increasing in brightness while all the rest were waning, and which, moreover, as Vogel also distinctly showed, is coincident in position in the spectrum with that observed in the majority of the nebulæ.

The observations of such rare phenomena as the so-called new stars, are of such vast importance, and will no doubt ultimately provide us with a clue to so many others of a different order, that we may well congratulate ourselves that the recent *Nova* was so well watched, and that there is such perfect completeness and unity in the chain of recorded facts.

It should have been perfectly clear to those who thought about such matters that the word star in such a case is a misnomer from a scientific point of view, although no word would be better to describe it in its popular aspect. The word is a misnomer for this reason. If any star, properly so called, were to become "a world on fire," were to "burst into flames," or in less poetical language, were to be driven either into a condition of incandescence absolutely or to have its incandescence increased, there can be little doubt that thousands or millions of years would be necessary for the reduction of its light to the original intensity.

Mr. Croll has recently shown that if the incandescence observed came for instance from the collision of two stars, each of them half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, light and heat would be produced which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

A very different state of affairs this from that which must have taken place in any of the Novas from the time of Tycho to our own, and the more extreme the difference the less can we be having to deal with anything like a star properly so called.

The very rapid reduction of light in the case of the new star in Cygnus was so striking that I at once wrote to Mr. Hind to ask if any change of place was observable, because it seemed obvious that if the body which thus put on so suddenly the chromospheric spectrum were single, *it might only weigh a few tons or even hundredweights*, and being so small might be very near us. Mr. Hind's telescope was dismounted, and I have not yet got any information as to change of position; and as I am now writing in the Highlands, away from all books, I have no opportunity of comparing the position now given by Lord Lindsay in R.A. 21h. 36m. 52s., Dec. + 42° 16′ 53″, with those given on its first appearance by Winnecke and others.

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 exceeding 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.

In connection with this, perhaps I may be permitted to quote the following from one of my "Manchester Lectures":—

"There is one point to which I think I may be permitted to draw your attention, although at present it rests merely upon an unendorsed observation of my own. I thought it would be worth while to try what would happen if I inclosed specimens of meteorites, taken at random, in a tube from which I subsequently exhausted the air by a pump. After the pumping had gone on for some considerable time, of course we got an approach to a vacuum; and arrangements were made by means of which an electric spark could pass along this apparent vacuum, and give us the spectra of the gases evolved from the meteorites. Taking those precautions which are generally supposed to give us a spark of low temperature, and passing the current, we got a luminous effect which, on being analysed by the spectroscope, gave us that same spectrum of hydro-carbon which Mr. Huggins, Donati, and others have made us perfectly familiar with as the spectrum of the head of a comet. There, then, we get the atmosphere of meteorites, not necessarily carbonaceous meteorites, but meteorites taken at random; and this atmosphere is exactly what we get in the head of a comet.

"Now let me go one step further; and to take that step with advantage, allow me to refer to another point . . . . that whereas Schiaparelli has connected meteorites and falling stars with comets, Professors Tait and Thomson, on the other hand, have connected comets with nebulæ, both of them being, according to those physicists, clouds of stones. Now how was one to carry these spectroscopic observations into the region of the nebulæ? A Leyden jar was included in the circuit, and we had what is generally supposed to be an electric current giving us a very much higher temperature than we had before. What, then, was the spectrum; the spectrum, so far as the known lines were concerned, was the spectrum which we get from the nebulæ; for the hydro-carbon spectrum, which we get from the atmospheric meteorites at a low temperature, was replaced by the spectrum of hydrogen; the spectrum of hydrogen coming, of course, from the decomposition of the hydro-carbon, with the curious, but at present unexplained, fact that we got the spectrum indications of hydrogen without indications of carbon. In my laboratory work I have come across other curious cases in which compound vapours, when dissociated, only gave us one spectrum at a time—by which I mean that in a vapour consisting of two well-known substances, under one condition we only get the spectrum of one substance, and under another condition we get the spectrum of the other substance alone, so in others again of both combined. The evidence seems, therefore—though I do not profess to speak with certainty—entirely in favour of the ideas of Sir William Thomson and Prof. Tait on the one hand, and of Schiaparelli on the other. I note this because I shall have again to refer to the conclusion to be drawn from it, namely, that there is probably an intimate connection between nebulæ, comets, meteorites, and falling stars."

I have given the above extract to show that a mass of meteorites at a temperature higher than that found to

exist in a comet's head could give us the hydrogen spectrum which was discovered with such richness in the *Nova*, and which is represented in the spectra of most nebulæ.

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.

The nebular hypothesis in its grandeur and simplicity remains untouched by these observations; the facts so far from being in direct opposition to it help us, I think, all the better to know exactly what a nebula is.

There is another point of extreme interest to the spectroscopist if we accept the bright line observed in the star by Dr. Copeland and others to be veritably the chief nebula line.

It is clear from Dr. Vogel's diagram (given in last week's NATURE) that this line brightened relatively with each decrease in the brilliancy of the hydrogen lines. On December 8, 1876, it was much fainter than F, while by March 2, 1877, F was a mere ghost by the side of it. On any probable supposition the temperature must have been higher at the former date.

Now it is well known that within certain limits the lines in the spectrum of a compound body get brighter with *decrease* of temperature, because at the higher one the compound almost entirely ceases to exist as such, and we get the lines of its constituents. It is a fair theory then to suggest that the famous nebula line may belong to a compound. Nay the fact as it stands alone further points to the possibility that the compound in question contains hydrogen as one of its constituents.

J. NORMAN LOCKYER

Craig Dhu, Kingussie, September 10

THE GLACIAL GEOLOGY OF ORKNEY AND SHETLAND

NO one can claim to speak with more authority on matters Orcadian than Mr. Laing, and few men are better fitted to judge of evidence and probabilities. His interesting letter (see p. 418 of this number of NATURE) calls attention to certain points which he regards as affording a crucial test of the value of some contending hypotheses in geology.

He asserts (1) That there is no evidence that the Orkney and Shetland Islands have ever participated in the general glaciation of Britain. (2) That these islands contain no raised beaches or marine terraces to prove any alteration of the relative levels of sea and land.

I. It would indeed be extraordinary on any hypothesis that no traces of glaciation should exist in Orkney. Could it reasonably be supposed that at a time when "the adjacent islands of Great Britain and Ireland" lay under a deep mantle of snow and land-ice which protruded even from the opposite shores of Caithness, these northern islets enjoyed a happy immunity from the cold which sealed up the more frigid south? I am afraid that on the contrary we must believe Orkney to have been in as evil case as its neighbours, no matter even if it should have succeeded in subsequently divesting itself of all traces of its wintry garb. It will not be necessary to discuss the bearing of Mr. Laing's facts upon any rival geological doctrines if it can be shown that the facts themselves do not exist. He courteously invites examination and disproof, and I think with all deference to him that I can point to evidence which when he next revisits his county will satisfy him that Orkney is no exception to the general glaciated condition of Scotland.

I have twice visited Orkney, and each time was too intent upon the curious history of the Old Red Sandstone of that region to have time to note all the features bearing upon the glaciation of the islands. But these features were too striking to escape notice, and I find in my note-books and on my map records of the observations jotted down at the time. So far from there being, as Mr. Laing asserts, no trace of ice-work among these islands, I found them to be well glaciated and to contain in particular, excellent illustrations of (1) *roches moutonnées*, (2) boulder-clay, and (3) valley-moraines.

1. Mr. Laing mentions the granitic axis which runs north from Stromness. When he has occasion to cross it again, gun in hand, let him stop here and there on the exposed hummocks and he will find them admirably ice-worn and striated. Well-preserved surfaces of this kind overlook the wild cliffs of Yesnaby, and others, of equal clearness, occur on the slopes behind Stromness. But further examination will show him that these markings are not confined to the hard granite and gneiss. Thus on the roadside at the south-east end of the Lake of Stennis, beautifully striated flagstones may be seen, the striæ in all these cases running north-west and south-east, as if produced by a movement from the latter quarter. Nay, even among the soft yellow sandstones of Hoy, well smoothed and striated surfaces may be noticed on the summit of the cliffs near the Old Man, at a height of 600 or 700 feet above the waves of the Atlantic.

2. Unmistakable boulder-clay occurs in Orkney. It is not generally or thickly spread over the surface, as in the lowlands of Scotland, but rather, as in Caithness, lies here and there in hollows, the rest of the surface of the islands being covered with a thin argillaceous soil, derived, as Mr. Laing points out, from the decay of the underlying flagstones. A thick mass of this boulder-clay lies on the north-west side of Shapinsha, another in the sheltered hollow of Kirkwall Bay, and a third forms a notable feature on the north coast of Flota. Mr. Laing cannot but be familiar with these and other localities, and he probably refers the deposits there to disintegration of the rock underneath. Of course the boulder-clay consists here, as elsewhere, mainly of the *débris* of the rocks below, and as these rocks are flagstones, breaking up into sharp-edged fragments, the stones in the clay are very commonly more or less angular. If, however, he finds, as he will assuredly do, that many of the stones are well polished and striated along their major axis, he may be satisfied that the deposit is a glacial one.

3. So far, the evidence which I have adduced shows that the Orkney Islands participated in the general wide-spread glaciation of the adjacent mainland. But we may believe that in so northern a locality, if the form and height of the ground in any manner permitted, the lingering snows would still form glaciers on the hills, though they had retreated from the lower grounds. Now there is only one mass of high ground in Orkney—the island