Meteors.

Date.	Radiant. R. A.	Radiant. Dec.	Character.
April 20-23	189°	−31	Slow; long.
20-21	261	+36	Swift; bluish white.
20-22	271	− 2	Swift: streaks.
20-25	218	−31	Slow: long paths.
30	291	+59	Rather slow.

Planets.

Venus—Is an evening star. It sets 2 hrs. 29 mins after sunset.

Saturn.—The position of this planet on 15th of April at 8 p.m. will be R. A. 2 hrs. 24 mins. 25 secs. Dec. 12°-3′-17″ N. Time of its setting is 6 hrs. 53 mins. p.m.

Mars.—The position of the planet on 15th of April at 8 p.m. will be R. A. 21 hrs. 47 mins. 59 secs. Dec. 14°-44′-52″ S. The time of its rising will be 2 hrs. 16 mins. a.m. on 16th April.

Jupiter.—The position of the planet on 15th April at 8 p.m. will be R. A. 14 hrs. 38 mins. 29 secs. Dec. 14°-0′-1″ S. The time of its rising will be 7 hrs. 6 mins. p.m.

The Sun.

A total eclipse of the Sun will take place on April the 28th. The eclipse will be invisible in this country. The line of central eclipse traverses the Pacific Ocean, and the most suitable spot for observation will probably be the Friendly Islands. A partial eclipse will be visible in Australia.

Extracts from Publications.

Mr. E. W. Maunder, at the Meeting of the British Astronomical Association, said that, with regard to the question of the brightness of the corona, it had occurred to him in three total eclipses to try to note the time after sunset when the illumination was equal to the illumination during mid totality. The first time he did so was in India in 1898, two or three days after the eclipse. Being out in the open just after sunset, it struck him that the phenomena of the fading light were very like what he had witnessed during totality, and he carefully noted the time when he thought the illumination was just about equal to what it was at mid totality. Curiously enough, at the same time Mr. Backhouse, on the other side of India, had been taken with the same idea, and he also made observations and the two agreed, he believed, within about half a minute of time. Then in the Algerian Eclipse of 1900 he suggested to several observers that they should make similar observations, and here, again, although it might have been expected that the observations would be extremely rough, yet they proved to be very convenient.

In Mauritius, 1901, they repeated the same observations. These four or five observers made independent estimates and they agreed very closely.

Practically it came to this, that the illumination of the sky during the total eclipse was about equal to that when the Sun was seven degrees below the horizon; which would show that there was a very appreciable amount of illumination from the corona in a total eclipse.

[*Journal of the British Astronomical Association.*

The following note on Star Drift by F. W. Henkel, F.R.A.S., appears in the Journal of the British Astronomical Association:—"Some time ago Mr. Hardcastle, in an interesting address to the Association (January 1910), alluded to the phenomena of migrating stars, referring specially to the five well-known stars in the Great Bear. β, γ, δ, ϵ and ζ Ursæ Majoris, as having nearly the same proper motion, which was first pointed out by Proctor about 1870, though Dr. Ludendorff has since found that the proper motions are not to be considered exactly parallel, but all as diverging from the same point of the sphere. He mentioned that Sirius, too, has been shown by Lieut. Hertzspring to be a member of the same family. It may interest some to give here Dr. Ludendorff's list of stars, which he considers are also members of the group in addition to Sirius and the five stars in Ursa Major. They are β Aurigæ, 37 Ursæ Majoris, δ Leonis, and α Coronæ Borialis. Thus ten stars are now known to be connected in this way.

These stars not only drift together, but they also lie approximately in one plane, and nearly in a right line. The convergent point is given by Dr. Hertzspring as R. A. $127^{\circ}\cdot8$ Diel $+40^{\circ}\cdot2$, or in a vacant region of the Constellation Lynx, south of the fifth magnitude star 31 Lyneis. The speed of the system referred to the Sun is given by him as about 18·4 kilometers per second.

Dr. Ludendorff's detection of α Aurigæ and β Coronæ as members is given in Astronomische Nachrichter, No. 4376. See also Astrophysical Journal, 1909, September."

[*Journal of the British Astronomical Association.*

Messrs. Walmesly and Chant write to the *English Mechanic* as follows :—" At five o'clock on February 11, from the summit of Goat Fell, some 1,500 feet above sea-level, we saw below the Sun, which was then about three or four degrees above the horizon, what appeared to be a portion of the solar spectrum, crossed by bright and dark lines. So far as we could judge, the lower end of the spectrum appeared to rest on the sea, which at the time was obscured by a bank of fog. Two bright and two dark lines were prominently visible, and several others were suspected. A vertical bright ray was also visible running down the middle of the spectrum. The colour was orange-yellow, and the dispersion apparently very great. The width of the spectrum corresponded exactly with the diameter of the Sun. Wisps of cloud or fog were seen drifting across the upper portion of the spectrum, and ultimately obscured it. The phenomenon disappeared at 5 h. 4 m., reappeared for a minute or so about 5 h. 8 m., and then finally vanished. It was as sharply defined as if seen through a spectroscope. The sun was at the time shining brightly in a clear sky, immediately above a dense bank of cloud.

Our theory is that a rift in the cloud bank immediately under the sun allowed a ray of sunlight to fall on the sea below, and the sea being covered by ripples, acted as a huge diffraction grating. From a different elevation we should probably have seen a different portion of the spectrum. We should be glad of any information on the subject.

[*English Mechanic.*

Regarding Mr. Espin's Nova, Mr. Monck writes :—"I would have thought that the fact of a star having been previously seen in the position of Mr. Espin's Nova tended to establish the nebulous origin of new stars. The star was previously there ; but it has received a great accession of heat and light. What is the explanation ? Rushing into a nebula seems the most natural conclusion. It is true that no nebula may have been seen previously ; but new joint nebulæ are lines constantly discovered, and the list is probably still far from complete. On the other hand, though one small star was noticed, no companion star was previously seen ; but I must here admit that, with objects at so great a distance, it is probable that no telescopes in existence would separate two colliding stars for some years before the collision. But stars are distributed so sparsely through space that collisions between two of them must be of very rare occurrence, while, on the other hand, nebulæ occupy such extensive spaces that it seems almost certain that stars must occasionally rush into them. I do not profess to be an authority on what would occur if there were a grazing or even a direct collision between two stars ; but if a star of any considerable size rushed into a tenuous nebula I would expect to find a mere surface-heating which would soon die away when the nebula was cleared, while the shock of a collision between two large solid bodies would penetrate more deeply and take longer to die away. The two stars, however, would not improbably be surrounded with gaseous envelopes which would bear the brunt of the collision.

[*English Mechanic.*

The following are some remarks on the standard measure in possession of the Royal Astronomical Society made by Mr. H. B. Darling and others at a meeting of the Society.

It is probable that even at the present day there are very few standard measures which are entitled to be called standard scales ; and this fact lends a certain importance to the Society's scale, specially as Baily's comparisons were made with extreme care and accuracy.

In 1907 a number of comparisons were undertaken by Major MacMahon, Deputy Warden of the Standards, at the request of the Society, in order to determine whether the length of the centre yard and the relative lengths of its three component parts had altered during the period of more than 70 years which has elapsed since Baily's comparisons. The scale was compared with the official bronze bar of the Board of Trade known as SS, the length and sub divisions of which are accurately known in terms of the present Imperial standard yard. Major MacMahon found the centre yard to be 0·001050 inch longer than the present Imperial standard yard. Baily had found it to be 0·000376 inch longer than the Imperial standard yard of his day (Bird's standard yard of 1760), the length of which cannot be precisely expressed in terms of any existing standard, as it was destroyed in the fire at the Houses of Parliament in 1834. Airy has, however, referred to comparisons of the centre yard made about 1851-5, which showed that its length was 0·000420 inch greater than that of the present Imperial standard yard. The total length of the centre yard would accordingly appear to have increased relatively to the Imperial standard yard by 0·000630 inch since 1851-5. The lengths of the sub divisions were, however, found to bear the same ratio to the length of the centre yard as they did at the time of Baily's comparisons, so that their values at the present day could be obtained from Baily's results by multiplying by a constant factor.

The President—I have been much interested in Mr. Darling's account of our standard scale and of the recent comparison, as I have no doubt all the Fellows present have been ; but his remarks about the apparent changes of length of our standard bar lead me to ask whether it may not be possible to attribute some part of the apparent change to a real change in the standard yard itself ; it is impossible to say where the apparent change arises. Why should we depend for the accuracy of our standards on the constancy of length of pieces of metal ? Should we not refer all our measures to the wave lengths of the red line of Cadmium ? No doubt the Scientific Meteorologist of a hundred years hence will not refer his standard to our scale or to the present standard yard or to the irrido-platinum bar at Breteuil, but to the length of the latter as determined by Fabri and Perot in terms of the wave length of the red line in the spectrum of Cadmium. I would add that in my opinion the construction of this bar is not good mechanically, and specially the insertion of the palladium pins appears to me to be a not very good device ; but I need not say more from the Chair and will ask others to speak on this paper.

[*The Observatory.*

The Observatory has the following account by Mr. Hinks of his measures of the magnitude of the Nova in Lacerta given by him at a meeting of the Royal Astronomical Society. "What I have attempted to do is to determine the photographic magnitude of the star upon several nights ; and at once I came across the fact that Mr. Bellamy mentioned that the photographic magnitudes of the surrounding comparison stars are altogether discordant from the visual magnitudes of those stars as given in the Harvard A. G. Zone Catalogue or the B. D. One can get no sort of reconciliation between the two ; so that it seemed the best thing to do was to begin by determining the photographic magnitude for some ten stars round about the Nova, which I have done, and of which I give an account in the paper. I used half a dozen stars to give a zero of the magnitude scale, making their mean photographic magnitude equal their mean visual magnitude in the Harvard Catalogue. That zero was 8·65 ; and it was evident that the Nova was a magnitude or so brighter. No other star in the same field was as bright, so that my photographic magnitudes depend on extra polation, which always, of course, is exceedingly dangerous. The extra polation, however, was done in this way. I have in the measuring machine at Cambridge a photographic scale which was made by giving a series of exposures on the same star in the ratio $\sqrt{2 \cdot 5}$, the idea being that if 2·5 were the right ratio (which we know it is not photographically), one would get the scale interval half a magnitude. I have made two attempts to get the ratio of that magnitude scale. First, I used the Pliedas Series measured by Professor Schwarzschild in extra focal images. In that way I got two scale intervals (intended to be half magnitudes exactly), equivalent to 0·88 instead of 1. In order to get a check on that value, I reduced the same measures with Professor Pickering's photographic magnitudes of Pliedas stars, and my result was 0·65 instead of 0·88, a very serious discordance. To check that, I then measured up a number of fields of long period variables with the magnitudes given in the Harvard "Annals," Vol. 37, that gave 0·71. Time was pressing and I could not stop to discover what was the cause of that discrepancy between the ordinary results and the extra focal results ; so for the purpose of this paper and as a preliminary I adopted 0·69 as the value of two scale-intervals.

Reducing the differences of Nova and comparison stars with that scale value, I get a series of magnitudes which are given in detail in the paper results I need not trouble you with now. But I should mention that there are two sets of plates : Ilford Monarch and Wratten and Wainwright's Panchromatic. One expects of course, as it is such a red star, that one would get brighter photographic magnitudes from the Panchromatic plates than from the others, and strange to say that turns out to be the case ! On January 1, 5h. 3, the Monarch plates give with my assumed zero 7·74 ; the Panchromatic plates give 7·42 ; so that the Nova is a third of a magnitude brighter on the Panchromatic than on the Monarch. On January 6, 6h. 4, I obtained another Panchromatic plate ; and that gives me the magnitude of 7·41. Thus there did not seem to be much change of magnitude in these five days ; but I must say that is contrary to the visual impression. A Monarch plate taken January 7th gave me photograhpic magnitude 7·74. So that the photographic evidence, so far as it goes, suggests that in the 5 or 6 day intervals the magnitude of the Nova did not change at all. But I am bound to say that the perfect concordance of these measures is hardly warranted by the individual results. which you will find tabulated more in detail in the paper. I hope that some of the photographic magnitudes of the dozen comparison stars obtained may be useful to other examiners of the Nova.